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The Century Psychology Series
Richard M. Elliott, *Editor*

SENSATION AND PERCEPTION
IN
THE HISTORY OF
EXPERIMENTAL PSYCHOLOGY

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D. APPLETON-CENTURY COMPANY
Incorporated
NEW YORK LONDON

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467

PRINTED IN THE UNITED STATES OF AMERICA

TO
HERMANN VON HELMHOLTZ

PREFACE

It was in 1924 that I resolved to try my hand at writing a history of *experimental psychology*—as that term was then used to designate human adult normal generalized ‘orthodox’ psychology. I wanted to start with the men and the schools as an introduction and then to trace the history of experimentation and thought in the fields of sensation, perception, feeling, emotion, learning, memory, attention, action and thought. When I found in 1929 that the introduction on men and schools was a book in itself, I published it under the title originally planned, *A History of Experimental Psychology*, realizing only later that this title is somewhat inapt, since I had not yet got to the experimentation in experimental psychology.

At that time I had, however, some doubts about my ability ever to go farther, for the subject of visual sensation seemed to me so vast and complicated that only a specialist might undertake to assess its discoveries and abstract its history. In 1935 I ventured, nevertheless, to try my hand at the chapters on vision and subsequently concluded that it is not impossible for a layman like myself to create a sound perspective of the progress of visual science. Whether I was right or wrong, the reader of this book can now judge; but at any rate I have kept on to finish the other senses, finding once again that ‘enough to make a book’ had to it a certain amount of unity. The first two chapters of the present volume I have written last. They are less thorough than the others, for I present them only as an introduction to the next thirteen, which constitute the body of my undertaking.

There remains the possibility of a third book, if the reception of this second one should justify it. It would recount the history of experimentation in the other fields—feeling, emotion, learning, attention, action, thought. It could, perhaps, be organized about the ‘dynamic principle’ that comes into psychology with the determining tendency and with ‘choice.’ In that history it

ought not to be necessary to go back to the beginning of the seventeenth century. Is there any experimental psychology of motivation before 1850? Hypnosis, to be sure; but is there anything else? Such a trilogy would indeed complete my original intention; yet I make no promises. This more recent history is necessarily less secure, less ready for delineation, than what is older.

It is a nice question as to when the past becomes history, as to how old it needs to be before a first stable perspective of it can be limned. In 1929 I felt secure in my evaluations up to 1910, uncertain about the penultimate decade, and quite tentative about the ultimate decade of the 1920's. Now, ten years later, I find that the same relations still hold. The history of sensation and perception seems to me clear up to 1920, not quite so certain in the next decade, and since 1930 definitely confused by its recency. In part this sense of certainty about the antepenultimate decades must be due to the fact that opinions are not yet crystallized nor the perspective established by convention. An important discovery of 1912, if still important in 1942, is no longer likely to turn out to be an artifact, even though it may need reinterpretation.

Thus, in this book, it has been my intention to slow down at 1920 and to stop at about 1930 except when the momentum of discovery is irresistible—as it was in the psychophysiology of hearing, when seventy-year-old problems began to come up for solution in the 1930's. Nevertheless, the reader should not trust me after 1930, since I do not trust myself. No man can see clearly so near his face, and writing history is a matter of selection.

Indeed, so much a matter of selection is the preparation of an historical text, that I am sobered by the responsibility. The text of 1929 has existed long enough for me to see how a mood that determined the choice of an afternoon's exposition can fix the 'truth' of a certain matter upon graduate students for years to come. With industry and patience one may avoid the falsification of facts, but those virtues are not enough to make one wise in choosing what to ignore. For that one needs also wisdom and the integrity of objectivity, and who knows for sure whether he commands such?

In a strict sense neither this book nor its predecessor of 1929 is a history—an historian's history. In both I have written solely to show how psychology came to be as it is now. If any event

important in the past has no demonstrable indirect effect upon the present, then it should be omitted from a book that tries to recreate the past merely to explain the present. As a matter of fact, however, the continuity of thought is such that nearly all events important in the past have had some effect on the present, for good or ill or both, as the case may be. The effect may, nevertheless, be small. Mountains in labor may bring forth mice. One such mountain was nativism-vs.-empiricism, and another was Weber's law. If I were trying accurately to reconstitute the psychology of the late nineteenth century, I should have to draw these mountains large, letting them obscure smaller and more fertile objects. Actually, I have painted them in small, and in the distance as seen by the naked eye from the point of view of the present, whereas some other remote details I have enlarged telescopically. But that, I realize, is not quite History.

For a book of this sort the author reads original sources, discusses them, relates them, cites them. Thus a question arises about the original sources which he cannot see; should he cite them at second hand, sharing, in that fashion, all his knowledge with the reader? Or should he refrain from mentioning what he has not read, since second-hand citations tend, as we all know, to perpetuate error? I have resolved this dilemma by distinguishing two kinds of references. All the originals that I could find in Greater Boston I have seen myself, and many other important texts I have had from Washington. Less important items I have cited at second hand, labeling the citation "[n.v.]," a symbol which means "not viewed" and hence "not verified." I have been conscientious. If the first edition is unavailable to me, I note it as "n.v." and then cite, perhaps, the second explicitly. Let the critic who thinks I should have cited nothing that has not been under my nose merely imagine that these "n.v." references are not in the book. With but a little more undocumented dogmatism, they could all have been omitted.

Let me observe here that I have been repetitious, both in allusions to matters discussed more thoroughly in other parts of the book and in the reiteration of dates. It is a pleasant fiction to believe that most readers start at the beginning of one's book and read through to the end, remembering everything. Actually such a book as this is consulted more often than it is read, or is, at best, studied in a random order. Thus I have sought to add to

the comfort of the adventitious reader while disturbing as little as possible the continuous reader. When there is an important and extended discussion of a closely related matter in another part of the volume, I give a cross-reference by page. On the other hand, when the related matter is incidental, I allude to it with a phrase that indicates that it is to be found elsewhere ("as we have seen"; let the reader then use the index if he can not guess the place), or else I reintroduce the data casually in dependent clauses. In this way the adventitious reader benefits without knowing it, and the continuous careful student can perhaps be deaf to these echoes if he has had this explanation.

Dates I have scattered through the text with almost endless repetition, because I do not expect the reader to know them or to remember most of them. The reader needs, I believe, to be given, constantly and repeatedly, explicit temporal orientation, so that he can, if he will, pause to think: "That was just before so-and-so published such-and-such; he might have known better a few years later." The reiteration of "Albrecht von Haller (1763)," "Johannes Müller (1838)," "E. H. Weber (1846)" may seem tiresome, but at least it makes "Haller (1758)," "Johannes Müller (1826)," "E. H. Weber (1852)" stand out with a warning that there is something unusual about the citation.

In this book there is one persistent anachronism which I have allowed myself under what I think is justifiable literary license. I write, "Wundt found in 1859 . . .," when I mean, "Wundt published, in the part of the *Beiträge* that came out in 1859, the results of certain experiments which, for all I know, he may have performed in 1858 or even earlier." When I know there is latency, however, I indicate it. I would not find Helmholtz doing anything in 1896 when the second edition of the *Optik* appeared two years after his death; nor would I find the aging Galileo formulating the frequency theory of pitch in 1638 (the year in which he published the finding), when Mersenne, his pupil, had already been able to refer to the result in 1636 (the year in which *he* published *his* finding).

My debts are numerous and some are vague. Should I mention here my permanent debt to Titchener, who determined my interests, set my drives? I wonder if I may not have been unconsciously modeling my professional life on his, as I think he did

on Wundt's. Ought I not here to give thanks for Libraries, who patiently serve all who come with honest ignorance? When I think of the Libraries, I know myself for a rewrite man, for this book is but an index to a minute section of the Harvard College Library and the Boston Medical Library, with addenda for the Army Medical Library. Libraries stand out as a tremendous reason for the humility of such authors as I. And then what about the Great Prime Mover of all intellectual activity, the *Zeitgeist*, without whom no man would think as he does, nor have his thoughts make sense? But I have done the *Zeitgeist* honor throughout the pages of this text and of the other in 1929.

My greatest determinate debt is to Mrs. Katherine Frost Bruner, who read and revised eleven of these chapters, pushing me measurably toward felicity of expression, even though I have not as yet attained her ideal. She, Mrs. Dorothy Telfer Delabarre and Mrs. Jane D. Morgan have slaved in different years on the preparation of the manuscript, and my wife, Mrs. Morgan and Dr. Elliott, my editing friend, have faithfully battled against my errors as they read the proofs. The chapters on hearing and the sections on attributes and psychophysics were read by Dr. S. Smith Stevens who substituted truth for my fiction in more places than one. How many persons have to help when a book is written! I am very grateful to them all.

There is also a debt that I owe to Society-at-Large, which has finally, giving up a fruitless struggle to have me spend my summer vacations in Maine, left me in happy peace working on this book in Cambridge through Julys and Augusts. A studentless month in the summer is worth five of Harvard's winter hurly-burly. Sometimes I fear that, if Harvard does not give up trying to turn itself from an Institution of Learning into an Educational Institution, we may have a generation of professors whose duty it will be to disseminate information which they have not the time to acquire. But fortunately they will still have their summers.

No reader of this book will need to ask why I have dedicated it to Helmholtz. There is no one else to whom one can owe so completely the capacity to write a book about sensation and perception. If it be objected that books should not be dedicated to the dead, the answer is that Helmholtz is not dead. The organism can predecease its intellect, and conversely. My dedication asserts

Helmholtz's immortality—the kind of immortality that remains the unachievable aspiration of so many of us.

E. G. B.

December 6, 1941

Cambridge, Massachusetts

Note

n.v., in the Notes at the ends of the chapters, means “not viewed” and, therefore, “not verified.” It is used when unavailable sources are cited at second hand.

The abbreviations of the names of journals follow, in general, the forms given by the *World List of Scientific Periodicals*, but a few concessions have been made for the less sophisticated readers.

The FRONTISPIECE shows Helmholtz in 1876. It is reproduced, with permission, from the copper-plate reproduction of Franz von Lenbach's portrait in Helmholtz's *Physiological Optics*, Eng. trans., I, 1924, Optical Society of America.

CONTENTS

	PAGE
CHAPTER 1. SENSATION AND PERCEPTION	3
Sensation	4
Perception	13
Attributes	19
Nativism and Empiricism	28
Psychophysics	34
Notes	45
CHAPTER 2. PHYSIOLOGY OF SENSATION	53
Nerve Conduction	53
Sensory Conduction	62
Specific Nerve Energies	68
Projection	78
Isomorphism	83
Notes	91
CHAPTER 3. [√] VISUAL SENSATION: BEGINNINGS	97
Before Newton	97
Isaac Newton	101
Thomas Young	107
Goethe	112
From Goethe to Helmholtz	116
After Helmholtz	119
Notes	121
CHAPTER 4. [~] COLOR AND ITS STIMULUS	126
Color and Its Dependence on the Stimulus	126
Brightness and Intensity	132
Weber's Law	136
Color Mixture	139
Color Diagrams	145
Notes	149
CHAPTER 5. [~] VISUAL PHENOMENA	155
After-Images	157
Adaptation	160
Contrast	165
Indirect Vision	171
Luminosity of the Spectrum and the Purkinje Phenomenon	176
Color Blindness	182
Notes	190

	PAGE
CHAPTER 6. COLOR THEORY	198
The Young-Helmholtz Theory	199
The Duplicity Theory	203
The Hering Theory	206
Modifications of the Fundamental Theories	210
Modern Visual Theory	214
Notes	216
CHAPTER 7. VISUAL PERCEPTION OF BIDIMENSIONAL SPACE	221
The Retinal Image	222
Singleness of Vision and the Horopter	226
Eye-Movements	230
Nativism and Empiricism	233
Geometrical Optical Illusions	238
Dynamical Gestalten	246
Gestalt Psychology and the Laws of Form	252
Notes	257
CHAPTER 8. VISUAL PERCEPTION OF DEPTH AND DISTANCE	263
Secondary Criteria of Distance	263
Convergence and Accommodation	271
Stereoscopy	282
Size and Distance	288
Tridimensional Dynamics	299
Notes	303
CHAPTER 9. PSYCHOPHYSICS OF TONE	312
The Origin of Musical Notes	312
Physical Acoustics	321
The Limits of Hearing	332
Differential Sensitivity	339
Notes	346
CHAPTER 10. AUDITORY PERCEPTION	352
Beats and Combination Tones	352
Timbre	357
Tonal Fusion	359
Noise	363
Masking	365
Vowels	367
Attributes of Tone	375
Auditory Localization	381
Notes	392
CHAPTER 11. AUDITORY THEORY	399
Before Helmholtz	400
Resonance Theory of Hearing	404
Other Theories of Hearing	411

	PAGE
The New Physiology of Hearing	419
Notes	431
CHAPTER 12. SMELL AND TASTE	437
Smell	438
Taste	449
Notes	458
CHAPTER 13. TACTUAL SENSIBILITY	463
Tactual Differentiation	465
The Two-Point Limen	475
The Error of Localization	483
Stimulation	487
Weber Function	495
Summation	498
Adaptation	500
After-Sensations	501
Heat	504
Touch Blends	509
Notes	513
CHAPTER 14. ORGANIC SENSIBILITY	523
Deep Sensibility	523
Kinesthesia	524
Proprioception in the Inner Ear	535
Internal Sensibility	544
Visceral Sensation	546
Hunger	551
Thirst	558
Organic Sensory Patterns	561
Notes	564
CHAPTER 15. THE PERCEPTION OF TIME AND MOVEMENT	574
Perception of Time	575
The Time-Sense	577
Rhythm	583
Perceived Movement	588
Notes	602
CHAPTER 16. CONCERNING SCIENTIFIC PROGRESS	608
Index of Names	615
Index of Subjects	631

SENSATION AND PERCEPTION
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EXPERIMENTAL PSYCHOLOGY

Chapter 1

SENSATION AND PERCEPTION

LET us begin with a précis.

The concept of sensation became important in human thought by way of philosophical empiricism: knowledge comes to the mind through the avenues of the senses. For this reason empiricism has always been allied to sensationism and, among the British empiricists, to their associationism. Physiologists turned to the problems of sensation after Bell's and Magendie's discovery (1811-1822) that sensory and motor nerves are different—the Bell-Magendie law. Then Wundt, a philosopher 'founding' a 'new' experimental psychology, derived the structure of systematic psychology from the British associationists, making sensation the basic element of consciousness.

In the meantime both the Scottish philosophers and the later English associationists had come to distinguish perception from sensation—sensation as the bare content given to mind, perception as the apprehension of an object. An object, they contended, is more than a simple 'given,' for the mind refers to it or 'intends' it; an object is actually a meaning. Thus the empiricists undertook to explain perception by the association of sensations and images. It is associated compounds, they thought, that constitute knowledge of an object.

Wundt, consequently, in constructing his system put into it the sensations as elements and the perceptions as complexes of sensations, in that way fixing upon psychology for a long time the notion that the synthesis of elements into complexes is the proper explanation of objective reference and thus of conscious meaning. Later, however, Külpe and Titchener tended to break the supposedly elementary sensation down into attributes, which finally appeared in psychology as the abstracted dimensions of consciousness. They added, moreover, space and time to the list of attributes, whereas Wundt had had only quality and intensity, and in so doing they did much to erase the distinction between sensation

and perception, for Wundt had regarded quality and intensity as simple and sensory, but space and time as modes of complex perception.

The nativists also, from Hering on, made extension 'given' and therefore simple, and their descendants, the Gestalt psychologists, have in the present century denied both the distinction between the elements and their complexes and also the existence of meaningless conscious contents. In Gestalt psychology, therefore, perception has absorbed sensation, whereas in physiological psychology sensation has absorbed perception.

The Gestalt psychologists and those others who have inherited the tradition of introspection from Wundt tend in the present day to speak of *phenomena* and *phenomenal experience*. The physiologists speak of *sensory processes* and *mechanisms*, considering them either for their own sakes or as correlates of experience. In addition there have been the behaviorists, who later gave place to the modern positivists. They find that scientific knowledge about what used to be called sensation is acquired only when the organism—human or animal—makes discriminations, and thus they substitute discriminatory reaction for sensation.

It is with the history of all these occurrences that the first part of this chapter is concerned.

Sensation

An empiricist is apt to be a sensationist, because it is by way of the senses that the mind has experience of the external world. Both of these doctrines are very old. Heraclitos (5th century B.C.) said that knowledge comes to man "through the door of the senses," and Protagoras (ca. 485–411 B.C.), the first of the Sophists, extended this view, maintaining that the entire psychic life consists only of sensations. It was the Stoics who first used the figure of the *tabula rasa*, the blank wax-tablet of the mind upon which experience writes. Sensations, they held, are impressions of outer things upon the mind. These early Greek philosophers had this empiricism, but it was not the dominant philosophy of the time.

The Greeks also had a theory of perception that still haunts the present. Empedocles (ca. 490–ca. 435 B.C.) supposed that objects give off from their surfaces or pores effluvia, which act upon the senses to furnish knowledge of the outer world. Democritus (ca.

460-ca. 370 B.C.) and Epicuros (ca. 341-270 B.C.) described these projections as faint images, simulacra or eidola of the objects which, being conducted to the mind, give it acquaintance with the objects which they represent. These men wished to explain why perception is correct, why sensory knowledge is valid. To us their theory sounds naive; nevertheless it is natural to attempt to explain representative adequacy by similarity. Johannes Müller was still having to fight Democritos and Epicuros in 1826 when he argued that the mind, being inside the body and thus in direct contact only with its nerves, perceives not the objects themselves, but merely the nerves which the objects affect. Wertheimer, too, was opposing this ancient view when, in 1922, he argued against the 'constancy hypothesis,' the insidious and insistent belief that experience and stimulus are necessarily alike.

Empiricism came into its own after Thomas Hobbes (1651). Objecting to the doctrine of innate ideas, Hobbes wrote: "There is no conception in man's mind which hath not at first, totally or by parts, been begotten upon the organs of sense." Then, after Hobbes, came John Locke with his *Essay Concerning Human Understanding* (1690) in which he depicted the mind as a piece of white paper upon which experience writes. For Locke the mental element was the idea, which comes from experience by sensation or reflection. *Reflection*, which "might properly enough be called internal sense," was his term for the mind's knowledge of its own operations. Indeed, it was this belief in an inner sense, the belief that the mind always knows what it is doing, that tended to prevent empiricists from becoming full-fledged sensationists, from assuming that all knowledge comes from the outside. For Locke, ideas were such entities as whiteness, motion, elephant, army, sheep, murder, gratitude; and of these, whiteness is a simple idea, whereas sheep and gratitude are compound, formed, as Locke presently put it, by the association of simple ideas. Such a psychology is an empiricism, an associationism and also a partial sensationism. Sensation is in it the primary source of knowledge.

Bishop Berkeley (1709, 1710), with his idealism, followed Locke. He held that mind is the ultimate reality, that the ideas are primary, that *percipi* is *esse*. The ideas he classified in respect of Aristotle's five senses, holding that no idea can be common to two senses and thus establishing the basic classification of sensationistic psychology—sight, hearing, smell, taste, touch. In his *New*

Theory of Vision he considered at length the visual perception of magnitude, pointing out that one cannot tell the size of an object without taking into account its distance, and that awareness of distance depends in turn upon awareness of the separation between the pupils of the eyes (convergence), the straining of the eyes in focusing (accommodation), and the blurring that occurs when the focus is poor. The perception of magnitude would, therefore, involve all these items which, he thought, come to be united in experience (association). Berkeley also had a theory of objects which closely resembles Titchener's context theory of perception (pp. 17 f.). We perceive, he said, one idea, "not immediately of itself," but "by means of some other idea." Thus the idea of a coach may come into perception by a visual idea if one sees the coach, an auditory idea if one hears it rumble up to the door, or a tactual idea if one enters it. In other words, these ideas are originally sensations, and sensations are thus the primary constituents of mind.

After Berkeley came David Hume (1739-1740), who sought to "restore the word, idea, to its original sense, from which Mr. Locke had perverted it." He called the sensory and perceptual data of mind *impressions*, using the word *idea* for the "faint copies" of impressions which occur in memory and imagination. Like Locke and Berkeley, he believed in complex ideas, synthesized by association, which is a "gentle force" of attraction that unites the simple ideas. Thus Hume, establishing the distinction between sensation and image, also furthered the progress of sensationism.

Hartley (1749), the last of the early empiricists, contributed to this school a parallelistic dualism of vibrations in the nervous system, correlated with sensations and ideas in the mind. The notion that physical action in the nervous system must be vibratory he got from Newton, holding further that sensations depend upon gross vibrations in the peripheral nerves, and ideas, their copies (cf. Hume), upon diminutive vibrations ("vibratiuncles") in the brain. Both the diminutive vibrations and ideas combine, Hartley said, according to laws of association. He stated these laws in detail for the vibrations and then repeated them all for the ideas. Thus he was not only making empiricism associationistic, but he was also, by his psychophysical parallelism, making sensation depend even more upon the properties of the nervous system than had Berkeley.

This is not the place for a history of sensationism in relation to

empiricism and associationism. We must, therefore, content ourselves at this point with but brief mention of the contributions of the French School, the Scottish School and the later English associationists.

Descartes (1662) had begun the mechanistic tradition in *France*. The body is a machine, he said, a machine which "can be incited by the external objects which strike upon its organs of sense to move it in a thousand different ways." He drew the analogy between man and certain, then popular automaton, which moved when an external stimulus actuated a device that released the moving mechanism. Condillac (1754) used a different analogy to establish his sensationism. He conceived a statue in which he unlocked, one by one, the senses—first smell, then touch, and so on—showing how its mind would be generated through this constant addition of more and more sense-experience. Condillac's contemporaries, La Mettrie and Bonnet, wrote in a similar vein. La Mettrie (1748) described *l'homme machine* and Bonnet (1760) conceived independently Condillac's figure of the statue. Certainly the French had no doubt that knowledge comes from sensation and that sensation occurs by way of the sense-organs.

Thomas Reid, the founder of the *Scottish* school of faculty psychology, was another to establish the primacy of sensation in psychology, even though (see next section) he was principally concerned to preserve the God-given perceptual faculties from reduction to mere sensation. His *Inquiry into the Human Mind* (1764) has, besides the introduction and conclusion, only five chapters, treating respectively of the faculties of smelling, tasting, hearing, touch and seeing. Psychological sensationism of the next century and a half may well be said to have begun here with Reid, even though its materialistic nature echoed the defeat of his fundamental purpose. Dugald Stewart (1792–1827), the second important writer in this school, reenforced Reid, but with less to say about perception and much more about the intellectual powers of the mind. After him came Thomas Brown (1820), who brought Scottish psychology into relation with associationism, thus formulating a systematic structure that has many similarities to Wundt's and Titchener's, at least in respect of the relation of perception to sensation.

It is James Mill (1829) who begins the later *English School* with what is often regarded as the culmination of associationism.

He pictured simple ideas as compounded by association into complex, and complex into more complex, and so on up to the *reductio ad absurdum* that "the idea called Everything" may somehow be an associative compound of every idea of a thing. Hartley's distinction between sensation and idea he accepted, beginning his exposition by a consideration of the sensations as the elementary data of the mind. The initial eight sections of his book take up, in order, first Aristotle's five senses, then sensations of disorganization (pain for the most part), muscular sensations (the book was written just after Charles Bell had 'discovered' the muscular sense in 1826), and sensations of the alimentary canal. In thus starting with the elements he contributed to a custom for textbooks which Wundt later fixed upon psychology for many years.

Bain, the later associationist, did not write his *The Senses and the Intellect* until 1856, after physiology had begun to affect the philosopher-psychologists. Thus he could be the first to present a full account of the nature and functioning of the nervous system, as an introduction to his book, which then discussed in detail the senses—the conventional five, to which he added the anomalous cases of the "sensations of organic life."

Meanwhile physiology had become alive to the importance of sensation. Haller's *Elementa physiologiae* (1757–1766) discussed the senses fully, but it was Charles Bell's discovery in 1811 and Magendie's confirmation in 1822 of what later came to be called the Bell-Magendie law that provided the effective stimulus for research. They found that the nerves are of two kinds, sensory which lead to the posterior roots of the spinal cord, and motor which lead from the anterior roots. This dichotomy of nervous action into sensory and motor reminded the physiologists that the mind's sensations were as much their business as the muscles' movements.

Next Johannes Müller (1826) divided the sensory field into five by his doctrine of the specific energies of nerves. Aristotle had already made the division, of course, but Müller (Bell anticipated him, but that was not recognized at the time) gave physiological meaning to the difference by asserting that each sense has its own specific energy and can respond only with its own peculiar quality. Pressure on the eye gives light, light on the skin gives warmth, a blow on the ear produces a sound. (See pp. 68–72.) Here again classification stimulated research.

So it came about in the middle of the nineteenth century that the sense-physiology of the physiologists and the sensationistic psychology of the philosophers were ready for synthesis. Lotze's *Medicinische Psychologie* (1852) was the first truly physiological psychology, for we can hardly count Hartley's speculations about vibrations and vibratiuncles as physiology. Bain's was the next. It was about 1858 that Wundt, then Helmholtz's assistant at Heidelberg, first conceived the notion of a physiological psychology, a new and experimental psychology that should apply the methods of science to the problems of the mind. Fechner's psychophysics (1860) and Helmholtz's remarkably penetrating researches in physiological acoustics (1863) and physiological optics (1856-1866) reenforced him. So did all the other sensory researches before 1870 which the present book recounts. Wundt, therefore, published the first edition of his classic foundation for the new science, his *Grundzüge der physiologischen Psychologie*, which became in its six editions from 1874 to 1911 the standard handbook.

For systematic structure Wundt drew upon the associationists. James Mill had overdone the matter of compounding. The idea of Everything does not still have in it every idea of a thing. There is synthesis. The whole is less, as well as more, than the sum of its parts. John Stuart Mill had corrected his father on this point. Ideas, he had noted, combine in a kind of mental chemistry, for the parts are lost in the compound which also has properties that were not contained in the parts. Wundt accepted John Mill's point of view. His book takes up in order the nervous system, the psychic elements (sensations in respect of intensity and quality, and, in the first and last two editions, feelings), the formation of the sensory *Vorstellungen* (perceptions of space, time and intensity), movement and will, and finally the connections of mental processes (association, apperception, consciousness). Literally *Vorstellung* means, of course, *presentation*, but for Wundt it meant a compound resulting from mental synthesis and thus both perception and idea. His psychology is, therefore, an associationistic sensationism. Until he introduced the feelings as a second kind of element (1896), all mind was for him sensations and the results of their synthesis. It is small wonder that research on sensation at first dominated the new psychology.

There was some little question in the early days as to the number of senses upon which the mental life is founded. Of course there

are at least five, but is touch perhaps several senses? Charles Bell thought of the muscular sense, when he discovered it in 1826, as a "sixth sense." James Mill, as we have just seen, listed eight kinds of sensation. Weber (1834, 1846) distinguished between the *Tastsinn* and the *Gemeingefühl*, dividing the *Tastsinn* into *Drucksinn*, *Temperatursinn* and *Ortsinn* (cf. pp. 465 f.). Helmholtz, however, provided a method for settling this difficulty by defining *modality* as a class of sensations connected by qualitative 'continua'. Colors lie in a single modality because they can be placed in a single three-dimensional continuum, the color solid. Tones form a modality, but touch does not, for at least pressure, temperature and pain are discrete.

No sooner had Helmholtz shown how to settle this question than the problem of the number of separate elements arose. Wundt and his successors regarded the sensations as distinguished primarily by their qualities, and Fechnerian psychophysics had furnished the j.n.d. (just noticeable difference) as a means for fractionating qualitative continua and thus of counting sensations. The early Külpe (1893) and the early Titchener (1896) were the chief counters. Külpe computed the existence of 696 discriminably different visual brightnesses, 150 hues, 11,063 tones, three touches, four tastes and numerous smells. Titchener listed 32,820 colors (presumably meant to be the volume of the color pyramid, although the figure comes nearer to the area of the surface), 11,600 tones, a huge number of smells, four tastes, four cutaneous qualities, two qualities from muscle, one from tendon, one from joint, three, more or less, from the alimentary canal, one or more from the blood-vessels, perhaps one from the lungs, one for sex, and one for the static sense—a total of "more than 44,435." As against the sixty-four then known elements of chemistry, the mind seemed pretty well provided for.

It must, however, be said that Lotze and Stumpf had already made it clear that sensation can be regarded as a continuous function of stimulus, that the number of sensations in a continuum, like the number of points on a line, is really infinite. The j.n.d. must be an artifact of observation, and there must be more sensations than j.n.d., because, if two sensations a j.n.d. apart are each increased by half a j.n.d., they still remain a j.n.d. apart. There are, in fact—or at least there were then—no demonstrable quanta separated by critical points in the sensory continua.

The next question that arose about sensation concerned the number of its attributes and, presently, the nature of an attribute. Kulpe started this discussion in 1893 and it is not done yet. Since the issue is, however, important and somewhat involved, we must reserve its consideration until a later section (pp. 19-27).

Now we must ask this question: What has become of sensation in modern psychology? For some psychologists who followed Wundt's and Titchener's tradition it disintegrated into attributes. For Gestalt psychologists, who have rejected all formal elements, sensation was absorbed into perception or at least into the phenomena of their phenomenology. But for others it was translated into discriminative behavior in the following manner.

The rise of animal psychology (1898 *et seq.*), in the days when psychology was so sensationistic, meant that animals must be examined as to their sensory endowments. That aspiration was realizable. If an animal can discriminate one tonal frequency from another, can learn to take food or to salivate for one frequency and to reject the food-box or not to salivate for another frequency, then presumably he hears in the two cases different pitches. If he can 'tell' one color from another by accepting one and avoiding the other, then he must make that discrimination on the basis of different sensory experiences. After all, this is the best way of working with man. The language of introspection is never safe unless the references of its words are sure. If the color-blind person says that he sees red, one still does not know what he sees, especially if he asserts later that a given red and a given green are identical in hue. That is why Arthur König gave up introspection and trusted only to color matches in his determinations of color-blindness. If every color of the spectrum can be matched by a mixture of a given pair of colors, then and then only is the subject surely color-blind, a dichromat (see pp. 186-189). In this sense behaviorism antedates psychology.

Behaviorism was invented by John B. Watson in 1913 because he had been working with animals and had found that all these important problems of sensory psychology can be solved for different animal forms. He was tired of asking, after each experiment was finished: What sensations and other mental processes must this animal have if he makes these discriminations, if he behaves as he does? The facts of discrimination seemed to him enough. He wanted, moreover, to investigate children, whose use of language,

when they have any, is also unreliable. He noted, in addition, that the verbal method of introspection—in the Würzburg school and in Titchener's laboratory, for instance—seemed unreliable in that one laboratory often failed to confirm the findings of another. Why should one not, he asked, throw all these inferences about consciousness overboard and deal only with the actual data of verbal behavior? To demonstrate the validity of his point of view he wrote in 1919 *Psychology from the Standpoint of a Behaviorist*, including in it all the established psychological facts of any other text but avoiding any reference to consciousness or to sensations. Introspection he presented as verbal behavior.

After Watson America turned quite generally to behaviorism, while Germany cultivated the 'introspective' phenomenology of Gestalt psychology. No one else ever quite succeeded in being as simple in his behaviorism as was Watson, but American psychologists adopted the name and filled in their own specifications. The growth of mental testing helped this movement, because mental tests are usually little psychological experiments upon persons whose descriptive use of scientific terms cannot be trusted. The tested—children, foreigners who cannot speak English, the insane, and also the naive normal native adults—all behave for the psychologist and thus tell him their minds. Holt, Weiss, Tolman, Lashley and Hunter are the important behaviorists of the decade after 1917, but this chapter is about sensation and not about systematic psychology in general.

Behaviorism ultimately disappeared, in part because in the 1930's it got to be accepted as psychology, and in part because modern positivism became the sophisticated substitute for it. The older positivism of Mach and Karl Pearson—the view that the basic data of science are the immediate observations and not the entities (light, electricity, sensation, attention) inferentially derived from them—was superseded in the 1930's by the positivism of the Vienna circle under Schlick—the view that an entity derives its meaning from an understanding of the operations by which it is observed and any term its meaning by analysis of the language which gives it significance. This doctrine was called logical positivism in 1931 and is founded on the faith that meaning is secured for a concept by its reduction to simpler, more fundamental, common terms. Before 1931, however, the physicist Bridgman had undertaken (in 1928) to resolve the dilemmas of modern relativity

theory by insisting that physical entities, like space, can be understood only in terms of the operations for observing them.

All this was good gospel to the behaviorists, once they learned about the movement. Tolman, the behaviorist, now became an operational behaviorist. Others, not Bridgman, coined the term *operationism* and used it in psychology. Watson's doctrine was refined, purified, and also complicated. Sensation became nothing more than the operation by which it is got, that is to say, discrimination. This movement was, moreover, helped because the Gestalt psychologists, Koffka and Köhler, came out against it, calling it positivism and depreciating it, even while accepting some of the older positivism of Mach, which was, after all, an early form of phenomenology. Gestalt psychology, of course, wanted to keep immediate experience in psychology, whereas the operationists wanted to reduce immediate experience to behavior. The logical positivists recognized this principle when they coined the word *behavioristics*. The controversy—for it remains a controversy—represents a fundamental temperamental difference in scientific values and is not yet cool enough to handle in an historical text like the present.

It may be said, however, that the phenomenologists are on secure ground in that no one has a right to forbid their use of language. They want to keep immediate experience and could still have for use the old Wundt-worn sensations did they not deplore the artifacts of analysis. The burden of the reduction of sensation to its operational equivalent lies, however, upon the operationist, and he claims to know all the formulas for the required translation. Faced with sensation, he simply translates it, as Watson did, into discrimination. Operationism, so he claims, can eat sensation and have it too.

Perception

It was Thomas Reid (1765, 1784) who first insisted upon the distinction between sensation and perception. A sensation, he said, although occasioned by an impression upon an organ of sense, is not of the body but of the mind. Only a sentient being can have a sensation. Perception, on the other hand, although it depends upon sensation, is nevertheless much more than sensation, for it includes both a conception of the object perceived and also an immediate and irresistible conviction of the object's present existence.

This distinction, Reid went on to say, is difficult only because of the ambiguity of language: we use the same words for both sensation and perception. For instance, the smell of a rose *as sensation* is in the mind, but *as perception* the smell is in the rose itself. If the rose is to be perceived, both the conception of it and also the instantaneous conviction of its objective existence must be added to the sensation. How then, we may ask, does this expansion of sensation into perception come about? To this question Reid had many things to say, yet none of them was more final than that "the Supreme Being intended that we should have such knowledge of the material objects that surround us." Such knowledge, Reid added, must be immediate and not the consequence of reasoning; otherwise "the greatest part of men" (who "hardly ever learn to reason"), as well as all infants and children, would be wholly "destitute of it." Happily "God in his wisdom conveys it to us in a way that puts all upon a level." So sophistication is not a prerequisite of perception. This exegesis of Reid's—it is hardly a proof—means that faculties can be described but not explained. For Berkeley's and Hume's skepticism he was substituting a faith in the Creator, a faith which was, nevertheless, a scientific agnosticism.

Dugald Stewart (1792) added little to this discussion except to admit the negative character of Reid's 'theory' of perception and to defend it, but Thomas Brown (1820) brought to the problem the new principle of association, or, as he preferred to call it, suggestion. Brown had something positive to say as to how reference to the external object is added to sensation in perception. An object, he asserted, is something that has extension and that furnishes resistance, and knowledge of extension and resistance is gained through the sense of touch—or, more likely, through those sensations attached to our muscular frame which are usually considered to be touch. When the smell (sensation) of the rose is referred to the extended and resistant rose (object), the reference occurs because the sensation "suggests" (by association) the tactual and muscular feelings of extension and resistance. Certainly this insight into the Creator's methods was scientifically an advance over Reid.

Now we may turn to the associationists. In the preceding section, we saw how the empiricists came to conceive of compound ideas as being built up out of simple ideas by associative fusion,

and also how these compound ideas were, essentially, perceptions. Berkeley said that any idea is perceived by means of another idea, that a coach, being perceived by sound, sight or touch, is nevertheless the same coach because "the ideas intromitted by each sense, . . . having been observed to go constantly together, are spoken of as one and the same thing." James Mill analyzed the complex idea of a house into the ideas of brick, mortar, rafters, planks, nails, together with the ideas of position and quantity. That view failed, however, because the constituent ideas cannot be shown to be actually present in the perception. They may be, it is true, *potentially* present, ready to be realized by association if the opportunity permits, but they are not immediately distinguishable in the perceptual instant.

This point about perception's being understood in terms of potentialities was grasped by John Stuart Mill (1865). Accepting the evanescent and changing sensations as the immediate data of mind, he was trying to explain how our belief in an external world and in permanent objects arises. An object is, after all, subjective, a creation of the mind. John Mill laid down as his premise the principle that the mind is capable of expectation. Then he pointed out that, though sensations may disappear, their *possibilities* remain present to the mind. If one sees, he said, a piece of white paper on the table and then goes into another room, the sensations disappear, but their possibility remains, as is evidenced by their recurrence if one returns to the room to look again at the table. Sensations are fugitive and transitory, but the "Permanent Possibilities of Sensations" are enduring. That is why the physical objects of the external world are stable, even though generated from changing sensations. They are simply the Permanent Possibilities of Sensations. We shall meet this notion of potential sensations again in Titchener's context theory of perception.

It was on this foundation that Wundt constructed the mental chemistry of his *Physiologische Psychologie* (1874 *et seq.*). The sensations are the elements, to be studied in respect of their two attributes, quality and intensity. They are combined into *Vorstellungen*—perceptions and the ideas of memory and of imagination. The perceptual compounds are of three kinds: *intensive*, like the fusion of tones in a timbre or chord; *spatial*, like the combination of visual and kinesthetic elements in the binocular perception of distance; and *temporal*, as in the structure of a heard

rhythm. If one asks Wundt how these combinations are effected, one gets from him no simple answer. There are first the simple facts of *fusion*, as for tones. Similar to them are the *assimilations* of visual space perception, where the elements are more alike in the *Vorstellung* than they were separately, and also *contrasts*, where they are less alike. There are also *complications*—a word borrowed from Herbart and established by the complication experiment—in which sensations from different sense-departments are united. It is a complication to *see* ice as hard and cold, and the perception of visual distance is also a complication because it combines vision and kinesthesia. The simpler *Vorstellungen* of memory are *associations*, and indeed the reliance of Wundt on the associationists is so great that one scents association often where he avoids the term. At the top of the system are the *apperceptions*, the syntheses that yield *agglutinations* if they are relatively simple and *concepts* when they are maximally complex. There is no refuting Wundt. Half of his system consists of sturdy facts, described by his special terms. The rest is an army of arguments that insist and advance, tolerating no interference. If one stops to ponder Wundt and then to question him, he is already far away, thundering over the horizon of his next chapter.

We do not need to mention here Brentano and the act psychologists, who held that all conscious data are acts which refer to a content that 'inexists' logically within them, because this school did not greatly affect experimental psychology, except in so far as it is one of the antecedents of Gestalt psychology. We must, however, note briefly the rise and decline of another antecedent of Gestalt psychology, the school of *form-quality*. Von Ehrenfels in 1890 pointed out that the fact of melodic transposition means that a melody exists independently of the tones which constitute it. If you change all the tones and keep their relations the same, you have still the same melody. So with other temporal and spatial forms. A square is a square, whatever the length or color of the lines which form it. Such elements—the tones, the lines—are the *Fundamente* of perception, von Ehrenfels said. They form all together its *Grundlage*. When they are put together, however, a *Gestaltqualität* emerges, a form-quality which is what is actually perceived. This form-quality is really a secondary element of higher order. Von Ehrenfels did not say much about the relations among the *Fundamente*, but Meinong (1891) did. He made it

clear that it is the relations among the *founding contents* which determine the form-quality, which he called the *founded content*. His whole discussion has, however, a bias toward logic. Renaming the founding contents *inferiora* and the founded content a *superius*, he noted that *superiora* can come to be the *inferiora* of a still higher *superius*, and so on up to higher and higher levels of complication. Cornelius (1892–1893), who came next, objected to von Ehrenfels' notion of emergent elements and argued that the form-quality is only a founded *attribute*. Since the doctrine of attributes (see the next section) was not clearly established at that time, his conception seemed safe and was a wise step away from Wundtian elementism. After Cornelius the problem slipped back into experimental psychology with Schumann's researches on visual shape.

The next event of importance was Titchener's *context theory* in 1909. This was really a context theory of meaning, and Titchener invented it to combat the influence of the imageless thoughts in Külpe's school at Würzburg. Can one have an imageless *unanschauliche Bewusstheit*, as Ach claimed at Würzburg, or must the meaning of an awareness—a perception, for instance—always be given in palpable imagery? That was the question. Titchener answered it by saying that there are two stages to perception. A *new* perception has its specific meaning only if appropriate imagery is added. Recognition shows this relation. I learn that this face is Voltaire's, and for a while the visual core of sensations, which are the seen face, receives the Voltaire-meaning only if some image of the name—visual, auditory, vocimotor—is added as a context to the visual core. A heard rhythm may be a rhythm only because it receives accentuation by the addition of a kinesthetic context—an intermittent strain in a muscle somewhere. Kinesthetic contexts may provide the cues to the visual perception of depth—or at least so Titchener thought. When the perception becomes *old*, however, when it is habituated, then the context may drop off and the meaning "be carried in purely physiological terms." By that phrase Titchener meant that the meaning is there but unconscious. A conscious context may be needed before Voltaire's face is recognized, but the familiar face of one's wife is recognized instantly before any context can accrue to the sensory core. The unfamiliar words of a new language may need the conscious contexts of their equivalents in the mother-tongue, but it is not so for the words of the mother-tongue. The accomplished reader reads rapidly, get-

ting the meaning of every word with no time out for contexts. Thus Titchener argued, as had Berkeley, that it takes at least two sensations (or images) to make a meaning—at any rate at first. Later, when the perception is old, one sensation may be enough.

This dual theory of meaning was quite convincing in respect of new perceptions. The philosophers had already decided that meaning is a relation, so why should not a conscious meaning be a conscious relation, the relation of perceptual core to context? Trouble arose, however, for the unconscious meanings of the old perceptions. By what right does one say that the meanings are 'there' when they are not consciously realized? John Stuart Mill could have helped Titchener here, for he could have said that the meaning of the core is the Possibilities of Context, and that the test for the meaning's having been 'there' is that the context will arise if given opportunity. A reader knows all along the meanings of the words he reads so rapidly, simply because he can state each meaning later if asked. The meanings are potentially present. Titchener did not, however, appeal to Possibilities. He relied rather on indirect tests. If a reader, having forgotten the exact words, can still state in all its details the meaning of the paragraph, he must have known the meanings of all the words as he read. His subsequent performance shows it. To the piano player each note on a line means a natural, a sharp or a flat, according to the signature in which the piece is written, but the player is not conscious as he plays which of these three meanings each perceived note has for him. The evidence that he had the meanings correctly lies in his performance.

Tolman (1918) was the first person to recognize the fact that adequate behavior thus carries meaning and may be the effective context in a perception. Titchener, never reconciled to behaviorism, would not have accepted that principle, but of course operationism did. For it meaning can have no meaning apart from the means of testing it. The person or animal who responds in a specific fashion to a particular stimulus shows what meaning he attaches to the stimulus—and to the sensation, if one is talking also about consciousness. The stimulus-object that an animal eats is food—to the animal. Thus, although psychological operationism has had little to say directly about perception, its theory of perception is the stimulus-response relation. Response is the context that gives the stimulus its meaning for the responding organism. One sees, there-

fore, that a theory of perception lies implicit in modern psychological positivism.

The other modern school, Gestalt psychology, which abhors current positivism, does not have a theory of perception because it holds that meaning is given immediately in phenomena. You cannot have a theory of the *a priori*. Lotze said that nativism is not a theory of space perception, because to say that space is given is not a theory. No more did Thomas Reid have a theory of perception when he rejected Locke, Berkeley and Hume to say that perception is a faculty with which man is endowed by the Creator. The phenomena of Gestalt psychology are intrinsically meaningful, as meaningful as were Brentano's acts and Külpe's *Bewusstheiten* from which the Gestalt phenomena are descended. To perceive an object is simply, for Gestalt psychologists, to perceive an object, a whole that is both more and less than the sum of its parts, a datum that is not to be understood by analysis or by its genesis in experience. Let us take an example. This table I perceive with that book on it. The perception of the book does not cover, so the Gestalt psychologists say, a hole in the perception of the table. One can *see* that the table is continuous and that there is double representation—book and table—where the book lies. Nor is the case different if I perceive honesty in a face. Phenomenology takes what it finds and is content—as does all science when it gets to its ultimates.

Attributes

Description is necessarily analytical. One cannot open his mouth to describe anything without saying something about parts or aspects or properties in relation to one another. A house is walls, door, windows and roof, with the roof on top and the windows and door in the walls and not in the roof. A stone is shape, color, weight and kind of substance in complicated relations. When such descriptive ultimates are general properties which can vary continuously or discretely, when they are, in short, parameters, they may, if one chooses, be called attributes of the object described.

It is doubtful if Wundt meant much more than this when he characterized sensations as having two attributes, quality and intensity. Because he was immutably a systematist, he put these attributes into the structure of his psychology, yet he never formalized their systematic position by giving them a name and stick-

ing to it. *Merkmal*, *Eigenschaft*, *Stimmungscharakter* and even *Bestandtheil*, each in proper context meant *attribute* for him. The index of his *Physiologische Psychologie* contains no word meaning *attribute* and referring to sensation or feeling. It was Külpe who later began the formal doctrine of attributes, employing the word *Eigenschaft*. Wundt's important elements were, then, sensations with the attributes of quality and intensity. Feeling, however, varied in his system. At first (1874) it entered as a second element with its own attributes of quality and intensity. Then (1880-1893) Wundt turned feeling into a third attribute of sensation but left its qualities and intensities intact, so that feeling appeared as an attribute with other attributes of its own (as Külpe later objected). Finally (1896 *et seq.*) Wundt changed feeling back into an element with intensive variation in three qualitative dimensions. The point about Wundt is that he did not care; the attribute for him was not a formal term in the system, like *Element*, *Vorgang*, *Vorstellung*, *Verbindung*.

It remained, then, for Külpe in 1893 to give formal recognition to the attribute. First, he listed the attributes of sensation. They are, he said, *quality*, *intensity* and *duration* for all five senses, with *extension* added for vision and touch. By adding duration and extension to Wundt's quality and intensity, Külpe increased the number of attributes to four, thereby making a concession to the nativists who believed that the basic data of experienced space and time are immediately given and not generated in experience (cf. pp. 31 f., 234). Külpe kept Wundt's spatial and temporal *Vorstellungen*, calling them colligations, but the point here is that, not seeing how spatiality can be generated out of the non-spatial, he had to make extension primary as a sensory attribute.

Külpe defined the attributes in terms of their two properties. They are *inseparable* from the sensation. A sensation that has no quality or no intensity or no duration simply does not exist. Reduce any attribute to zero and the others disappear. The attributes are also *independently variable*; how else could they be distinguished? It must be said, however, that Külpe did not give to independent variability the same formal status that he gave to inseparability. He merely used the principle in his discussion of the attributes, and Miss Washburn stressed it later (1902).

Finally, Külpe denied that feeling can be an attribute (*Gefühlston*) of sensation, because it has attributes of its own (quality,

intensity, duration), and because the fact that a sensation may be neither pleasant nor unpleasant, quite uncolored by feeling, shows that feeling is separable from sensation, thus contradicting the primary criterion of an attribute. Titchener thought that this argument of Külpe's was the occasion for Wundt's changing his views about feeling (between 1893 and 1896), but Wundt, the polemicist, never admitted that.

This doctrine of attributes was clear but it did not prevail at once. G. E. Müller helped to establish it by his formal distinction in 1896 between intensive (i.e., intensity, duration and extensity) and qualitative series. Nevertheless, four difficulties arose, as follows.

(1) In the first place, it was not clear that vision has the attribute of intensity. Colors have three attributes, supposedly all qualitative: hue, brightness and saturation. All the colors, moreover, can thus be placed in a three dimensional figure which employs these attributes for its dimensions. Where is intensity? The black-gray-white series should be qualitative because it resembles the red-orange-yellow series, yet this series can be created by changing the intensity of the illumination and Weber's law (a law of intensity) can be tested out in respect of it. This matter is discussed later (pp. 132-136).

(2) In the second place, Titchener (1908), taking the common observation of Stumpf and others that low tones appear to be large and high tones small, had added an attribute of volume to tone. Although this addition might seem to be allowing tones to share with vision and touch the attribute of extent, Titchener thought of volume as qualitative because it varies, like pitch, with frequency of the stimulus. If pitch and volume vary together, how can they be independently variable and thus satisfy the second criterion of an attribute? Titchener appealed to their manners of variation. Tonal volume seemed to him to change less rapidly than pitch, at least in the middle of the musical scale. Later Rich verified this supposition by working out the differential volumic limens for volume and pitch. Since the limen for volume is the larger, a small change of pitch can occur without any change of volume. That is independent variability. (See pp. 378 f.) Still later Titchener got Gates to try to find a similar relation in the gray series between visual brightness (quality) and visual intensity, but without much success (p. 134).

(3) The third difficulty is that psychologists did not wish to be limited by formal considerations. They wanted freedom in the use of descriptive terms. Especially did this need appear in the work on tonal attributes that began about 1913. If tones, besides being high and low, loud and soft, big and little, of long and short duration, are also bright and dull, they wanted to say so, without having to decide whether brightness is a different attribute from pitch and from all the others. If tones resemble vowels, they wanted to say that tones have the attribute of vocality, without settling the question as to whether vocality is simply pitch, or pitch + loudness + volume, or what. (See pp. 376, 380.) This movement was the stirring of the new phenomenology in embryo, wanting freedom of description, objecting to analysis.

(4) The fourth difficulty was the most serious of all, for it was the raising of the question as to whether the attributes are not, after all, the conscious elements, whether they are not really separable in experience. Miss Talbot (1895) had said that they are not. Miss Calkins (1899), however, pointed out that the attributes are the ultimates of analysis and must, therefore, be the elements. Miss Washburn (1902) had composed this difference by showing that the argument is but one of definition: because the attributes are inseparable from sensation and one another, sensation is the conscious ultimate; nevertheless the attributes remain the logical ultimates of description. It all depends on whether you are talking about psychological analysis or logical analysis.

At this point (1904) Külpe himself turned on his own doctrine, but first we must mention Titchener's fifth attribute. Titchener was bothered by the concept of attention. It seemed to him a loose concept, used without rigor as a catch-all for seemingly dynamic events that could not be accurately described. He said of it (as Külpe had said of thought): Why not let observers simply describe what it is like to pay attention? That was good positivism of the Machian kind. Whenever the observers attended for Titchener, they reported that attention is merely the division of the field of consciousness into the clear and the less clear (focus and margin, Wundt's *Blickpunkt* and *Blickfeld*). Consequently Titchener introduced (1908) the sensory attribute of *clearness* (later named *vividness*, then *attensity*) to psychology. A sensation can be clear (attended to) just as it can be loud or yellow or big.

Now we can go back to Külpe. There is an early Külpe and a

later Külpe. The early Külpe (*ante* 1900) was the young man remodeling Wundt's system by substituting rigorous simplicity for what seemed to be Wundt's ambiguous complication. It is to this Külpe that the doctrine of attributes belongs. The later Külpe (*post* 1900) was the sophisticate of Würzburg complicating his own system and coming around to the less rigorous, negative belief that thoughts, whatever they are on the positive side, are certainly imageless. This was the Külpe who, with a freedom almost phenomenological, commented in 1904 on certain experiments on abstraction that W. L. Bryan had performed in his laboratory. Külpe and Bryan had shown their subjects, tachistoscopically, sets of four nonsense syllables. Each syllable had three letters (twelve letters in each set) and the colors of the syllables varied at random among four hues. The spacing of the syllables was not regular but formed different patterns in the different sets. Four different instructions were given to the subjects: to report (a) on the total number of letters visible, (b) on the colors of the syllables, (c) on the pattern formed by the syllables, and (d) on the identity of the letters seen. These four aspects of the perception are not the true sensory attributes that Külpe listed in 1893, but they are attributes in the sense that they were inseparable from the total object and could be varied independently. Külpe and Bryan found that the acceptance of one task meant abstraction from the others, that the subjects, set to observe letters, for instance, might be wholly unable to report immediately afterward on the colors, and that in such a case they "believed that they had actually perceived no colors" at all. Thus Külpe concluded that the attribute may be all that exists in mind at the observational moment, that it is the "conscious actuality," whereas the sensation or perception, the sum total of these attributes, must be regarded as the "psychic reality," a scientific entity that is built up upon many observations. In coming to this conclusion he went a long way to justify Miss Calkins' contention that the attributes are the true mental elements—the "actual" elements, if not the "real" elements.

In 1913 Rahn published from Chicago a critical monograph dealing with the concepts of sensation and attribute in contemporary psychology. He had worked with Külpe and was favoring Stumpf and the later Külpe as against the earlier Külpe and Titchener. He attacked Titchener's attribute of clearness with the argument that Külpe himself had used against Wundt's attribute of feeling. Clear-

ness, he said, can be observed—is, in fact, observed in experiments which measure it; but observation depends upon attention, and attention is, according to Titchener, clearness. Hence to observe clearness is for the clearness to be clear, for the clearness to have an attribute of clearness. In respect of the other attributes he appealed to logic and to Külpe's experiment on abstraction. Every psychophysical experiment on sensation is accomplished under some set to observe and to judge a particular attribute. All that the observation shows is the attribute, which must, therefore, be regarded as the *observed datum*. The sensation, on the other hand, consists of all the attributes which might have been observed under all the possible instructions; in other words, it is a *physiological entity*, a total excitation which carries with it these potentialities for attributive report.

After that—in 1915—Titchener altered his system. The attribute, he said, is the *determinative* factor in observation, that which comes out as the result of the particular observational set. The sensation is the *systematic* term, the construct that is built up by logic from many observations. He did not mention elements at that time, and presently (1924) we find him discussing, not sensations and attributes, but the dimensions of experience. The *dimensions* were the old attributes with new names for some: quality, intensity, protensity (duration), extensity and attensity (clearness). He had used the word *dimension* in this sense back in 1908, and now his intent was to describe consciousness as a changing pattern in respect of these five dimensions. He never published fully on this matter, and it was left for the present author (1933) to give the idea explicit form, a form of which Titchener would undoubtedly have disapproved.

That Külpe's experiment is valid for the conventional attributes of sensations was shown subsequently by Yokoyama (1924), Wilcocks (1925) and Chapman (1932)—at least they found that subjects can report more adequately on attributes for which they are set to observe before the exposure than on the other attributes which they have to judge after the exposure from the immediate memory of the stimulus-field. Zoll (1934) showed, however, that several attributes can function at once. He asked his subjects to report *same* or *different* for pairs of colors that might differ in respect of hue or brightness or saturation, finding their reports ac-

curate and immediate even when they did not know in advance which of the three attributive differences to expect.

Recently the problem of attributes has entered a new phase. (See pages 377-381.) Halverson (1924) showed that tonal volume varies with the intensity of the stimulus as well as with the frequency.

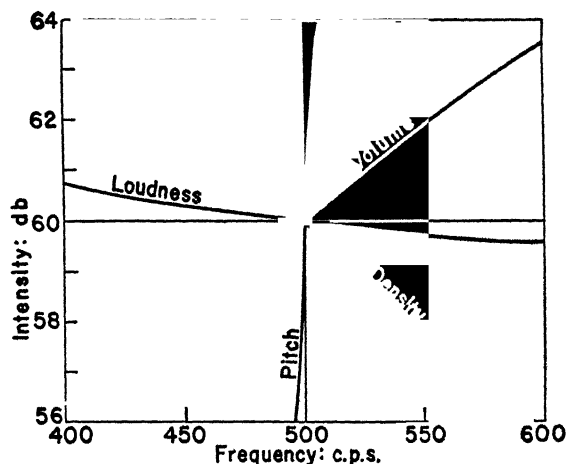


FIG. 1. ISOPHONIC CONTOURS FOR FOUR TONAL ATTRIBUTES:
STEVENS (1934)

The standard tone (corresponding to the center of the diagram) has a frequency of 500 c.p.s. and an intensity of 60 db. The 'equal-loudness' contour shows the locus of those combinations of frequency and intensity which will give tones equal in loudness to the standard tone. The contours for pitch, volume and density are similar. These graphic relationships are considerably altered for standard tones of different frequency and intensity.

Soft tones, as well as high tones, tend to be small. It ought, therefore, to be possible to equate a loud high tone to a soft low tone in respect of volume, and Stevens (1934) finally succeeded in finding the method by which such equations can be made. Fig. 1 shows the isophonic contour along which one set of tonal volumes would be equal: if frequency and intensity decrease together in the right manner, volume can stay constant. This figure also exhibits isophonic contours for constant pitch (which varies somewhat with intensity as well as with frequency), constant loudness (well known to be a joint function of frequency and intensity) and con-

stant density, a new attribute which Stevens discovered (1934) and which shows a function different from the other three. These functions indicate that the criterion of independent variability is too simply stated: it is not possible to vary one of these four attributes while keeping the other three constant. It is possible, on the other hand, to keep one constant while the other three are varied, and the curves of Fig. 1 show just how the stimulus must be changed if this end is to be accomplished. Stevens, therefore, suggested that, in addition to *inseparability*, an attribute must satisfy the criterion of *independent constancy*.

Several other consequences follow from Stevens' conclusions. Ever since Galileo correlated pitch with frequency, it has been customary to look for one-to-one relations between the dimensions of the sensation and the dimensions of the stimulus—pitch goes with stimulus-frequency, loudness with stimulus-intensity. Fig. 1 shows that this relation is not true. All four attributes are joint functions of changes in both the dimensions of the stimulus. It has also been conventional to seek in the sense-organ explanations of the attributes, such as the dependence of pitch upon place of stimulation in the ear, and loudness upon amplitude (or later total amount) of excitation. There is at present no known way of localizing the physiological conditions for attributive analysis in the sense-organ. The attributes express the capacity of the organism to make judgments about certain relationships of excitation. They might even be learned; as would be the case if low and loud tones were called *large* because they usually come from large instruments or other objects. The only reason to believe that they are not learned is the close agreement of many persons with respect to their measurements, and the ease and immediacy with which they are judged. All that can be said is that they depend upon properties of the nervous system, either congenital properties or, conceivably, properties acquired in experience.

The present author has shown (1935) that there is theoretically no limit to the number of possible attributes, except the nature of the nervous system, which must furnish some limit, although there is at present no way of saying in advance what it is. Even if attributes were learned, there would be, however, a practical limit. No one could learn to make accurate discriminations in accordance with a large number of superimposed functions in Fig. 1. If we were, for instance, to start inventing new attributes by making

up new arbitrary functions for subjects to learn, then we should have to stop very soon, for the limits of judgmental differentiation would soon be reached.

Unity of the senses. There has always been a question as to whether quality is a primary attribute, whether the other attributes depend on quality in such a way as to make it sensible to say that we have an intensity of a quality, the duration of a quality, the extent of a quality, but never the quality of an intensity or the extent of a duration. There is, of course, sense in speaking of the duration of an extent when size is changing, of the extent of an intensity within a spatial pattern of intensities. Titchener was inclined ultimately (1915) to have all the attributes coordinate contributors to the sensory whole. Such a view seems to imply that each attribute exists in its own right, and that for the intensity of a tone an identical intensity can be found associated with a color. Wundt in 1858 showed that a tactual extent can be equated to a visual extent. Münsterberg in 1890 tried equating the intensity of a sound to the intensity of a weight, but concluded that the judgment is made in terms of kinesthetic strains which function vicariously for the intensities of both pressure and sound.

Nevertheless the problem has persisted. It was one of the arguments for tones' (and smells'!) having volume, since extensity applies to visual, tactual and perhaps gustatory sensations. Von Hornbostel in 1931, having regard to the fact that colors have brightness and that brightness is one of the vigorous candidates for attributehood with tones (pp. 376-378), undertook to equate the brightness of a gray to the brightness of an odor, and then the brightness of a tone to the same olfactory brightness. He found that things equal to the same thing equal each other, that the brightnesses of the gray and tone appear equal when both equal the brightness of the odor, but Cohen (1934) came to the conclusion that von Hornbostel's triangular equivalence was an artifact of method. Thus the question remains open. Von Hornbostel (1925) coined the phrase *unity of the senses* to express the conception that all senses are alike in respect of their attributive dimensions, and indeed the physiological similarities of the ways in which the different fields of the sense-organs are projected upon the brain creates an initial presumption that they might resemble one another in respect of their discriminable aspects.

Nativism and Empiricism

The two 'dreary' topics in the history of experimental psychology, so convention has it, are nativism and empiricism, on the one hand, and psychophysics, on the other. Certainly the endless pages of futile talk—one of psychology's least happy inheritances from its parent philosophy—are dull, but psychophysics, which got somewhere, is not dreary, if one ignores the futilities, nor is the interminable argument about nativism and empiricism tiresome, if one forgets the talk and studies the events as an example of man's effort to see himself clearly in the dark. Psychophysics' successes are considered in the next section, and the problem of nativism and empiricism comes up again in the chapter on the perception of visual space (pp. 233–238), where it appears that every empiricist is also somewhat of a nativist, and conversely. Here we need only a prospectus of the debate about the origin of the knowledge of space.

The background is clear. Descartes (1637) held a belief in innate ideas, Leibnitz (1714) in preestablished harmony, Kant (1781) in *a priori* intuitions, of which space was one. The English empiricists, from Hobbes (1651) and Locke (1690) on, were on the side of the *tabula rasa*: mind is not 'given' but is generated in experience. Berkeley (1709) was the first empiricist to argue that the perception of extension is originally founded on the perception of the body's own movement, which becomes associated in experience with tactual and visual impressions. Thomas Brown (1820) insisted further on this same point, reducing visual space to movement, and thus perceived extension to perceived succession, since movement is serial. The notion that spatial localization is given in perception by movement or kinesthesia, associatively compounded with the sensation localized, was already in associationistic psychology before Lotze reenforced it.

After Kant and Thomas Brown there are to be traced four lines of development, two on each side of this dichotomy. We may omit all but the famous names. First there is *nativism*: Kant, Johannes Müller, Hering, Stumpf. Closely allied with nativism there is *phenomenology*: Goethe, Hering, Stumpf, Gestalt psychology. On the other side, *empiricism*: the associationists including Brown, Lotze, Helmholtz, Wundt. A synthesis of nativism and empiricism was offered by Külpe and Titchener. Allied to empiricism is the

line which, from the perspective of the present, looks like *operational behaviorism*, the views of those men who regarded actual movement, not the sensations of movement, as the key to the perception of space.

Nativism. Johannes Müller is regarded as having given formal status to the nativistic theory in 1826. His theory of the specific energies of nerves (pp. 68-72), a theory of sensory quality, assumed that the mind, being in the brain, can have direct contact only with the nerves and thus is directly aware only of the states of the nerves, never of the external bodies themselves. It learns about the states of external bodies, because the bodies affect the nerves and the mind perceives the states of the nerves. Müller's theory of visual space perception was related both to this theory of specific energies and to Kant's *a priori* intuitions. Thus the mind directly perceives the spatial relations of the retinal image, because these relations are preserved in the arrangement of the optic nerve fibers with which the mind has a direct apprehending contact. That the retinal image is upside down caused Müller no trouble, for the spatial relations within an inverted image are, of course, identical with those of the erect image.

Hering, writing in 1861-1864 after Lotze had invented local signs, attached the signs to every retinal point. With the facts of stereoscopic vision in mind, he sought to account for tridimensional space in terms of the retina alone—without the empiricist's appeal to movement of the eyes and body. Thus he posited three local signs for every point—one for vertical position, another for horizontal position, and a third for depth and distance. He held that this third sign might be positive or negative, contributing thus algebraically to the binocular perception of depth. Such a system of primitive spatial labels was fully enough to classify Hering as a nativist, but, like all the nativists, he was also somewhat of an empiricist, for he had a great deal to say as to how the concept of solid space gets built up in experience out of these primary localizations.

Stumpf, a student of Lotze's and Brentano's, carried this tradition further, writing his *Ueber den psychologischen Ursprung der Raumvorstellung* in 1873 and getting called, on account of it, to succeed Brentano in the chair of philosophy at Würzburg. Stumpf had once met E. H. Weber, who had, with his compasses, demonstrated the sensory circles on Stumpf's own skin. Weber's circles

(pp. 476 f.) suggested local signs, first to Lotze and now to Stumpf. Whether a belief in local signs made one, in those days, a nativist or an empiricist depended entirely upon whether one thought the signs were in themselves spatial, consciously labeled as to their loci, or whether one thought the signs were intensive (Lotze) or qualitative (Wundt), coming only after experience to indicate spatial relations. Stumpf thought, of course, that the spatiality is native in the signs. He is chiefly important in this tradition, however, because he forms a link between the older nativism and the Gestalt psychologists, who are also, in a sense, nativists.

Empiricism. Johannes Müller's nativism might never have become so important, had not Lotze in 1852 been so persuasive about the empiricistic view. Müller thought against a Kantian background. Lotze had the associationists, their conception of complex ideas' being intimate fusions of many sensory components and also their notion that all space perception depends upon muscular sensations. Weber's sensory circles furnished him with evidence of local signs. He did not, however, believe that spatiality is given in the local signs themselves. He argued rather that the signs are intensive, that the idea of space is built, not out of muscular qualities as Brown had said, but out of intensities. The tactual signs exist because any stimulus feels differently on different parts of the body, depending upon whether it is near a tendon or fatty tissue or some other special conformation which makes the intensive pattern of stimulation specific for the spot touched. (Much later, after Lotze was dead, Külpe said that this theory would require the maximal confusion of localization to occur between symmetrically opposite points on the body, because such places are the most similar in conformation, even though also the most remote.) For vision Lotze appealed to the tendency of the eyes to move so as to bring to the fovea any stimulated point that comes under attention. Every retinal point might thus have associated with it an intensity of a particular sensation of movement. Such signs, both visual and tactual, Lotze thought, get themselves ordered by experience: we learn that *b* is between *a* and *c* because with continuous movement the signs are experienced in the order *abc*. In that way we form the concept of lines, and from lines come areas. This was a positive theory, and Lotze asserted that nativism is no theory at all but rather a *petitio principii*, "the confounding of the solution of a problem with its data."

To Lotze's theory Helmholtz added the clarity of his thought and the enthusiasm of his exposition. He did not accept the intensive pattern as determining the local specificity, but thought rather that difference of nerve-fiber would itself be enough to explain perceived difference of locus. He left open the question of conscious local signs. Thus he added little to the theory except—and it was no mean addition—the force of his insistent exposition as he defended empiricism again and again from 1855 to 1894. It was he, for instance, who argued so effectively that the geometrical axioms are not innate, are not *a priori* intuitions, but learned relationships. He explained how people who lived only on a spherical surface would learn that all parallel lines intersect in two points, how people on an egg-shaped surface would eventually realize that circles of the same radius may have different circumferences, how men confined to a plane would never be able to get out of a square, and how beings who had four dimensions at their disposal could step out of a sphere as easily as we can step out of a circle. His views became so popular that Zöllner, the astronomer, perhaps mixing sense with nonsense, suggested that astronomical space must be curved and finite, else all matter would long ago have been volatilized, and that Slade, the medium, probably was able to get objects out of a closed box by using the fourth dimension!

The thorough Wundt was less deft but equally insistent—from 1859 to 1911. Every tactual and visual point, he said, has a qualitative local sign—not a spatial nor an intensive one but a local quality. Out of these qualities spatial continua get themselves built up in experience by movement. Eye-movements establish the visual continuities, bodily movements the tactual. Movement, moreover—and here Wundt was reverting to the associationists—continues to give the meaning of extension. The optical illusions deceive because they induce eye-movements that do not accord with the dimensions of the part judged, as when the eye moves too far with the obtuse angles of the Müller-Lyer illusion, or too little with the acute angles.

Külpe and Titchener count as empiricists, but they were really achieving a synthesis of the empiricistic and nativistic views. Külpe (1893) was accepting nativism when he added extension to the list of attributes for touch and vision. You cannot, he thought, get space out of the non-spatial. Man would never have conceived

of space had it not been given him in the first place—but then even Lotze had held that the general conception of space must be inherent in the nature of mind. Külpe made spatiality inherent in the nature of sensation. The local signs, he thought, are unconscious or conscious, depending upon what you mean by the term. The local *differentiae* among sensations are unconscious, for they are merely the physiological potentialities for different localizations, merely the neural connections of different points on the skin or retina with different points in the brain. A touch on the right hand may be identical with a touch on the left, as far as the qualitative-intensitive-durative-extensive pattern of the sensation itself goes; yet the two points may have different potentialities for conscious localization, as appears when one stimulation leads to the visualization of the one hand, and the other stimulation to an image of the other hand.

Titchener (1910) followed Külpe in all these matters and added the context theory of perception. There is no mysterious extra local *quale* to the extended sensation itself, but a conscious context of imagery or kinesthesia provides the localization. Most people know where they are touched because of added visual imagery, though the blind localize by kinesthesia. Even localization in the third visual dimension is contextual, Titchener thought, consisting of the addition to the visual core of a kinesthetic context derived from accommodation or convergence.

Such are the main points between which may be plotted these two historical lines. The argument is clear, yet what, we may ask, has become of these lines now? Why did nativism and empiricism seem to disappear in the twentieth century? The answer to this question is that they did not exactly disappear, but that each became something else. Nativism turned into phenomenology, and empiricism into habits of thought of the behaviorists and operationists. Let us see.

Phenomenology. If the givens are the important things, then all one does is to describe; he does not theorize. In spite of Lotze's remark about theories being necessarily positive, one is not begging the question if he finds it better merely to describe events and to create no theories about them. Thus the phenomenologists, in sticking to the immediately given, tend to be nativists. Goethe (1810; see pp. 112–116) makes a good starting point for phenomenology, with his innumerable observations about colors and his

implausible classifications which he called theory. Purkinje (1819-1825), that other excellent observer, perhaps also belongs at the start of this line. At any rate, with two such examples before him, Hering could well become the later exponent of what we now call phenomenology. His texts on brightness and color are replete with good observations. His theories, which of course he had, are less frequent and less important. Of elaborate experiments he had few, but of *experimenta crucis* he had many. The *experimentum crucis*, the little critical demonstration which makes a point, is a special tool of phenomenology, of Hering as it is now of the Gestalt psychologist. Stumpf at Berlin was also a phenomenologist, both by systematic conviction and, in music, by observational habit. It was at Berlin that Gestalt psychology began, with a small debt to Stumpf and a larger debt to Hering.

Thus it would seem that the Gestalt psychologists have a good chance of being the modern nativists. Are they? Well, they ask you to look and see, printing a little diagram upon the page—an *experimentum crucis*—to make the point. They do not analyse the *phi*-phenomenon of seen movement into attributes; they merely ask you to see it. They do not tell you what organization is; they ask you to examine a diagram and to see how some parts of it stand together to constitute a whole. It was they who showed that a perceived line is not a row of perceived points, or a square four lines when it can just as well be given by four points. A line, for them, is a line, a square a square, an organization an organization, and *phi* is *phi*. The givens are not to be explained. That is nativism, and, as Lotze implied, it is a faith and not a theory.

Behavioral operationism. If the Gestalt psychologists are the modern nativists, then the behaviorists, operationists and positivists must be the modern empiricists. Their sort of thinking is very old, but it does not lie in a self-conscious school. The men who said that the inverted image on the retina is seen right-side-up, because an image on the bottom of the retina leads one to orient his body toward the top of the perceived world (pp. 237 f.), were thinking behavioristically and operationally. So were the 'capacity psychologists,' the early mental testers and the functionalists, who thought that you can tell what a man knows by what he does. So too the animal psychologists, who thought that you can tell what any organism knows by what it does. All these men are the modern empiricists simply because so much of behavior is *learned*. Not all

of it, but most of it. If sensation *is* discrimination and if the consciousness of space *is* the capacity to behave adequately in space, then experience must play an important role in space perception. So it would seem that empiricism disappeared because it was absorbed by behaviorism and its later sophisticated substitutes, just as nativism disappeared because Gestalt psychology kept it by swallowing it.

Psychophysics

Fechner created psychophysics, making it secure by the publication of his *Elemente der Psychophysik* in 1860. We must ask now how this achievement of his was possible. What was the intellectual situation in the 1850's that made the measurement of sensation an appropriate and reasonable undertaking? Creative imagination is more than spontaneous generation; always is it fathered by the *Zeitgeist*.

In the first place, then, there was in the nineteenth century the general awakening of science, the experimental investigation of everything, the invention and improvement of instruments of observation, like the telescope and the microscope, in short all that marks that century as the beginning of the age of science. With science comes the refinement of observation and, of course, measurement. Astronomy and physics were full of measurements, and the new experimental physiology—not so much older than experimental psychology, for Johannes Müller was called its father—had been measuring whatever it could find about the organism to measure. Sensation was simply one thing to be measured, since the Bell-Magendie law showed it to be half of the function of the peripheral nervous system.

To some extent it is true that sensation already had been measured. Its stimuli had been measured, and sensations had been shown to be equal, or just noticeably different, a demonstration that is the beginning of measurement. Fraunhofer in 1815 (p. 177) had shown what must be the intensities of different spectral lights if the colors are to be equally bright. Bouguer in 1760 (pp. 136 f.) had anticipated Weber by showing what must be the relative illuminations of a shadow and its background if the shadow is to be just noticeably different from its background. There were dozens of such observations, and then E. H. Weber in 1834 by numerous experiments established—or at least seemed to establish—what

Fechner later christened Weber's law (p. 495). Weber's point was, of course, that two sensations are just noticeably different as long as a given constant ratio obtains between the intensities of their stimuli. So sure was he of this fact that he spoke of the barely discriminable differences in terms of ratios, without always mentioning the absolute intensities: the skin, he asserted, can appreciate a difference of $\frac{1}{30}$, be that $14\frac{1}{2}$ and 15 half-ounces or ounces or drams, or 29 and 30 half-ounces or ounces or drams. Fechner did not start from scratch.

Against experimental measurement were dualism and Herbart. Cartesian dualism was against the measurement of sensation because it held that mind is incorporeal and does not occupy space. Even psychophysical parallelism, which was coming more and more to represent the thinking of experimental physiologists about mind, seemed to leave sensation on an unmeasurable side of a dichotomy. Herbart, on the other hand, favored the measurement of ideas and the application of mathematics to psychology, although he denied the possibility of psychological experimentation. His *Psychologie als Wissenschaft* in 1824-1825 is filled with arguments, mathematically explicated, as to how ideas of different strengths interact with one another. He 'measured' but did not experiment. Perhaps that was *Wissenschaft*, but it was not *science*.

Thus Herbart, while hindering Fechner, also helped him. He helped him because he made the notion of measuring and mathematizing mind respectable, even though he believed withal that the method of measurement should be metaphysical. He also supplied for Fechner the concept of the limen, which he himself had from Leibnitz. A weak idea, in competition with stronger ideas, does not, Herbart thought, enter consciousness, is not apperceived, but, being inhibited, remains in a state of tendency. This conception of a limen for consciousness is basic to psychophysics, even though Fechner's consequent belief in the existence of "negative sensations" below the limen remained too vague to become scientifically useful.

The rest of psychophysics Fechner got from himself. A man with the broad intellectual interests of the German *Gelehrter*, he was trained as a physicist, resigning his chair at Leipzig in 1839 on account of a nervous breakdown. During the long period of illness which followed, his interest turned to philosophy and, more specifically, toward a campaign against the current materialism. The

last thirty-five years of his life (1842–1887) he devoted, it is not too much to say, to promoting a philosophy of panpsychism, the view that mind and matter are one and thus that mind is all. His psychophysics was but an incident in this campaign, for Fechner regarded the *Fundamentalformel* of “Weber’s Law,” which expresses, at least in respect of sensory intensity, the relation of matter (stimulus) to mind (sensation), as the paradigm for the translation of the material into the spiritual. Even here, however, we see that Fechner’s soul was not entirely his own but subject to the *Zeitgeist*. His philosophy the world for the most part ignored; his psychophysics it took very seriously indeed. First there appeared experiments based on the conceptions of the *Psychophysik* of 1860; then there was criticism of them, and in the criticisms came also the recognition of their importance.

The *Psychophysik* made the following important contributions to experimental psychology.

(a) It established and worked out details of what have since come to be the three fundamental psychophysical methods of the making of basic measurements and the computation of the psychophysical constants: the method of limits, the method of right and wrong cases, the method of average error.

(b) It showed how three of the five fundamental measures are obtained and what their meanings are: the absolute limen, the differential limen, and sensory equivalence. (It did not consider the problem of the determination of sense-distances or of sense-ratios.)

(c) From the facts of the differential limen, it established the meaning of what we now call Weber’s law, and from Weber’s law it derived Fechner’s law (which Fechner called “Weber’s law”). Weber’s law gives the relation of the just noticeable difference (j.n.d.) to the stimulus: a j.n.d. is given by a constant determinable ratio of two stimuli. Fechner, assuming further that all j.n.d. are equal and that the j.n.d. may therefore be used as the unit of a sensory scale, integrated Weber’s law to get Fechner’s law; that is to say, he counted up j.n.d. from the absolute limen in order to obtain the points on a scale of sensation (S) that is correlated with the scale of the stimulus intensity (I). Fechner’s law is, in these terms, $S = k \log I$. Fechner began by asserting that you cannot measure sensation directly, but only sensitivity. (The measure of sensitivity is simply the reciprocal of the limen.) By the assumption

of equality among j.n.d. and by integration, he thought, however, that he had succeeded in measuring sensation indirectly, and thus in achieving his goal of stating the relationship of mind to body.

In addition to all these matters the *Psychophysik* contains (d) a great many results of actual measurements and (e) many pages on the problems of "inner psychophysics," a phrase which Fechner used to designate the relation of sensation to neural excitation, in contradistinction to "outer psychophysics," which deals only with the relation of sensation to stimulus.

Before we take up the more important lines of discovery to which Fechner's work led, we must mention the later outstanding books. Delboeuf, who established the concept of sense-distance, published two important monographs, one in 1873 and another in 1883. In them he gave status to the method of equal sense-distances. G. E. Müller, who criticized Fechner and in particular modified and corrected the method of right and wrong cases, published one book in 1878 and another in 1903. Meanwhile Wundt kept discussing the methods and their results in the successive editions of his *Physiologische Psychologie*, and his students, especially Merkel, were publishing in the *Philosophische Studien*. Titchener's incomparable psychophysical manuals came out in 1905, with 281 pages out of the 587 of the second volume devoted to the history of psychophysics. The book was just late enough to take account of Müller's contribution in 1903, but too early for Urban's in 1908. Urban corrected the tables for the method of right and wrong cases and contributed the concept of the psychometric function. Later, psychophysics got itself tied up with the statistical methods of mental measurement, as Guilford's book (1936) shows.

Now for the scientific developments.

Methods. No one of the three fundamental methods was quite original with Fechner. Fechner established them; he did not invent them. The history of thought is nearly always continuous. Every important 'new' idea has its less important, often obscure, anticipations.

(1) The *method of limits (minimal changes)* is the procedure in which the stimulus is changed by successive, discrete, serial steps until a critical point is reached, a point where the subject changes his judgment. One starts with a supraliminal intensity and diminishes it until the subject reports *no sensation*; or one starts with a subliminal intensity and increases it until the subject reports

sensation; or better, one does both and averages the results. That gives an absolute limen. A differential limen is got by taking two stimuli, a standard and a comparison, and by varying the comparison stimulus until a report of *difference* changes to *no difference*, or conversely, or both. That is the most natural way to locate a critical point, to approach it gradually from one side and then from the other. Consequently, it is no wonder that various investigators should already have used this procedure incidentally—Sauveur in 1700, Bouguer in 1760, Delezenne in 1827. (See pp. 136 f., 339 f.) Titchener was inclined to give Delezenne the credit as originator.

When this method is used to determine the differential limen it becomes the *method of just noticeable differences* (*least perceptible differences*). The j.n.d. is the average of the mean first noticeable difference when the difference increases and the mean first not noticeable difference when the difference decreases. Although there is a good deal of variability about this average, the j.n.d. came to be regarded as a fixed unit, largely because Fechner started the custom of counting up j.n.d. in order to place the measure of a sensation on a sensory scale. Later, as we have seen (p. 10), psychologists like Külpe and Titchener counted j.n.d. for the purpose of determining the total tale of sensory elements. The counter-argument of Lotze and Stumpf (p. 10) that sensation can just as well be regarded as a continuous function of stimulus, that its magnitude is not made up of discrete quanta, was convincing but nevertheless never consistently accepted. Only recently have Stevens, Morgan and Volkmann (1941), with a method of right and wrong cases and but little variability about the average, presented evidence that the sensory scale may, after all, be quantal and not continuous. Indeed, one rather expects that finer analysis will ultimately come up against quanta—molecules, atoms, electrons in the case of matter, and discrete nerve-fibers or the all-or-none nerve-impulses in the case of sensation.

(2) The *method of right and wrong cases* (*constant stimuli, constant method*) presents the stimuli or stimulus-differences in quasi-random order, and, treating each stimulus (or stimulus-difference) as a constant, determines the relative frequency with which every category of judgment has been used for each stimulus (or stimulus-difference). The constancy appears in the treatment of the data, not in the experimental presentation. One gets in this way what

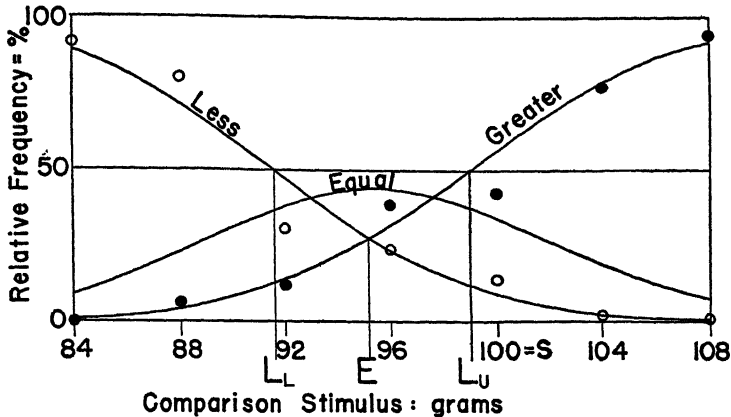


FIG. 2. PSYCHOMETRIC FUNCTIONS AND THE METHOD OF CONSTANT STIMULI

For comparison of lifted weights (one of Urban's subjects). The plotted points show the observed relative frequencies of the judgments *less* (open circles) and *greater* (solid circles) at five constant values of the comparison stimulus. The observed psychometric functions could be shown as lines connecting these points. The psychometric functions of the figure are, however, the normal ogives fitted to the points by the method of least squares. The function for the judgment *equal* need not have been drawn, since it shows only the residual frequencies (for the judgments that are neither *greater* nor *less*). $S = 100$ grm. is the standard stimulus, hence objective equality. $L_L =$ lower liminal point $= 91.68$ grm. $L_U =$ upper liminal point $= 98.95$ grm. $L_U - L_L =$ interval of uncertainty $= 7.27$ grm. The differential limens could be taken as $(S - L_L)$ and $(L_U - S)$, but there is here the usual constant error (time-error) with L_U lying below S , making the upper limen negative. It is, therefore, better to use the interval of uncertainty (or half of it $= 3.63$ grm.) as the measure of differential sensitivity.

The point of subjective equality, E , is usually taken as the stimulus difference for which the judgments *less* and *greater* are equally frequent (intersection of the two psychometric functions $= 95.24$ grm.). Sometimes, however, E is taken as the midpoint between L_L and L_U (95.31 grm.), or as the maximum of the *equal* function (95.52 grm.). These points are identical when the functions are symmetrical.

For an absolute limen the *equal* category disappears and the functions for *less* and *greater*, being then necessarily symmetrical, intersect at 50%, thus fixing there the position of the absolute limen.

The steepness of the normal ogives is expressed by the measure of precision, h (.461 for *less*, .444 for *greater*). Sensitivity varies with h , which has in a normal probability function a constant relation to the probable error.

Urban in 1908 called a *psychometric function*, a curve of percentages plotted against stimulus-values. An absolute limen is the point at which sensation is reported as frequently as not, the fifty-per-

cent point on the function. For the differential limen there are usually three functions, one for each of the three categories of judgment, *greater*, *equal* and *less*. Ordinarily two limens result, an upper and a lower. The upper limen lies at the fifty-per-cent point where *greater* is reported as often as not. The lower limen lies where *less* is reported as often as not. Urban called the distance between these two liminal points the *interval of uncertainty*, which can be regarded as twice the limen. See Fig. 2 and its legend.

Some psychologists have argued that the *equal* category is essentially a doubtful category and should be excluded, either by dividing the *equal* reports between the *greater*s and the *less*es, or by instructing the subject to avoid saying *equal* and, when uncertain as between the two remaining categories, to guess. Peirce and Jastrow (1884) and Fullerton and Cattell (1892) recommended this procedure. Both Warner Brown (1910) and Fernberger (1914) found that the *equal* judgments are reduced in frequency or even eliminated if the subjects assume the attitude of trying to perceive a difference. Of course, if the *equal* category is eliminated, there remain only two psychometric functions, symmetrical with each other and crossing at the fifty-per-cent point. Hence there is no limen. The investigators who use this method have, therefore, taken the probable error (the 75-per-cent point) as indicating the limen. This is the point that is half-way between 'chance' (50 per cent) and 'certainty' (0 or 100 per cent). Some others have measured sensitivity by h , a mathematical constant that varies with the steepness of the psychometric function.

The method of right and wrong cases, as it was called in the beginning, was first used by Vierordt and his pupil, Hegelmaier, in 1852. Fechner developed it, passing a normal ogive (the integral form of the normal probability curve) through only two stimulus-values, and computing the limen from this function. Müller in 1879 used more stimulus-values and fitted the normal ogive to them by the method of least squares. He also figured out the different weights to use for different frequencies in accordance with probability theory. Urban, as we have remarked, shifted the emphasis in the use of this method from the limens to the form of the function, which he named the *psychometric function*. He also corrected Müller's weights for the different frequencies, prepared tables to reduce the labor of the computations, and raised the question as to whether the normal ogive is the right function to use. This ques-

tion, after some trials with other functions, he answered with a Yes, but it is not yet clear how such a broad generalization can safely be made. The validity of the *a priori* use of the normal probability function, both in statistics and in psychophysics, seems, at least to the present author, still to be an open question.

(3) The *method of average error* (*production method*, *Herstellungsmethode*) provides the subject with a standard stimulus and with the means of varying a comparison stimulus. He must change the comparison stimulus back and forth, until it is subjectively equal to the standard stimulus. This is a production method because the subject himself produces the significant value in the stimulus. It is a method of average error, because in the treatment of results the average constant error and the average variable error are computed. The method was popular in America with the pre-behaviorists because it depends upon the behavior of the subject and not upon his introspective judgment. It measured his capacity to be 'right,' not the frequencies of his sensory experiences. From the point of view of the subject's relation to the external world of stimulus, moreover, every judgment of *equal* is 'wrong,' because no two stimuli ever are exactly equal. He might as well guess when he thinks the stimuli are equal, and then he can have the degree of his error determined. Thus the subject aims at no-difference in order to have his inevitable difference measured. It is also true that the subject finds it easier to adjust a stimulus by continuous trial and error than to judge fixed stimuli. It was for this reason that Stevens (1934; see p. 379) was able with this method to get consistent results on tonal volumes when others had failed with the method of constant stimuli.

The method of average error was first used by Fechner and Volkman (1856) in an experiment which Fechner later reported in the *Psychophysik*. It may be becoming popular again with the positivistic reaction away from the uncertainties of 'verbal behavior.'

Measurement. Just what values were the psychophysical methods used to measure? There are at least five basic items, any one of which could be obtained by any one of the three methods.

(1) The *absolute limen* is the critical point marking the end of a sensory scale—the threshold for intensity, the limits of audible frequency for sound, the limits of visible spectral wave-length for light. Fechner made the absolute limen basic to his system. In fact,

$S = k \log I$ is true on his own assumption only if the absolute intensity, I , is measured with the absolute limen as the unit.

(2) The *differential limen* is supposed to be a just noticeable difference, but we have seen that it is necessarily a statistical quantity, that no stimulus-difference is always just noticeable, that a liminal difference is noticed only as often as it is not. Fechner's use of the j.n.d. as a constant unit of sensory magnitude has, however, persisted up the present. The fact is that counting j.n.d. may or may not give us a scale. If Fechner's critics are right, if the j.n.d. as measured in relation to some other 'truer' scale are unequal, then we have only an *ordinal scale* of sensation, a scale in which the order of sensations is determined but not the relative distances between them. On the other hand, if we make all j.n.d. equal *by definition* (a j.n.d. equals another j.n.d. because both are j.n.d.), then counting differential limens does give us a ratio scale, with an absolute zero and relative distances significant in terms of the unit of the count.

(3) *Equivalents* can be determined as between a point on one scale and a point on another. For instance, Weber in 1834 determined the fact that a pressure of four ounces on the forehead is approximately equal to a pressure of one ounce on the lips. Wundt in 1858 determined the equivalents of tactual to visual extent. Fechner called the determination the *method of equivalents*, comparing tactual distance on one part of the skin with tactual distance on another. Recently the method has been applied to the problem of the unity of the senses, the determination of the brightness in the scale of grays and in the tonal scale that is equivalent to the brightness of a standard odor (p. 27).

(4) *Sense-distance* is measured when equivalence of two supraliminal stimulus-differences in the same sensory series is established. This determination was called by Wundt the *method of mean gradations*, by Müller the *method of supraliminal differences*, by Titchener the *method of equal sense-distances*. The method was first used by Plateau in the 1850's, although he did not publish his results until 1872. He provided artists with a black and a white piece of paper and had each of them paint a gray which seemed to lie half-way between the black and the white. (They agreed pretty well.) Then Delboeuf, at Plateau's suggestion, took up the problem in 1865 and finally published his results in 1873 in his *Étude psychophysique*. He used the term *contraste sensible* (sensible differ-

ence) for the sense-distance, whereas Müller spoke of the *Kohärenzgrad* (degree of approximation) of the differences. It was Titchener who coined the term *sense-distance* and argued, against the objection that sensations do not have measurable magnitude, that they can be measured because they can be placed on a scale of sense-distances.

Ever since Plateau it had been customary, because easier, to use *bisection* to establish this scale. One takes the series of stimuli *abc* and varies the serial position of *b* until $ab = bc$. Then one bisects *ab* and keeps on until the scale is as finely divided as may be desired. Titchener held that the resultant scale represents true measurement, and was right to the extent that physical measurement reduces ultimately to judgment of equality. Such a scale, however, having no absolute zero point, is what has been called an interval scale, which is not as potent an instrument of measurement as a ratio-scale, like the physical scales of weight or length.

There is a large literature on the use of this method, but it was not so large in the early days as to answer conclusively the question of whether Fechner's law holds for supraliminal differences. Had it been found that the law applies to both j.n.d. and supraliminal distances, then it would have been possible to argue that all j.n.d. are truly equal sense-distances, and Fechner's assumption of equality would have been supported. Only recently have we been coming to a solution of this eighty-year-old problem. The establishment of subjective ratio scales for pitch and loudness indicates that j.n.d. for pitch are equal, j.n.d. for loudness not.

(5) It is also possible to determine *sense-ratios*. Merkel (1888-1894) tried having the subject make one intensity just twice as great as another. He called the procedure the method of the *doubled stimulus*, although it was the sensation, not the stimulus, that was doubled. Nothing much was done with this method, however, until it was applied to tonal loudness in the 1930's. Then Richardson and Ross (1930) had subjects estimate the loudness of tones on an absolute subjective scale, assigning to a heard loudness a number representing the ratio of the loudness to an imagined standard loudness, and plotting these ratios against the relative intensity of the stimulus. Laird, Taylor and Wille (1932) had the subjects adjust a comparison tone to make it half as loud, and also twice as loud, as the standard. Ham and Parkinson (1932) asked the subjects to select from several comparison tones the one that

they judged to be one-half, one-third and one-fifth, and also twice, thrice and five times, as loud as the standard tone. Geiger and Firestone (1933) used the method of adjustment to establish nine ratios of paired loudnesses, ranging from 100:1 through 1:1 to 1:100. Churcher, King and Davies (1934) had the loudness halved and quartered by the subjects, and also the halves halved. From such data Fletcher (1933), Churcher (1935) and Stevens (1936) have constructed true ratio scales of loudness, scales with an absolute zero and all the metric power possessed by any physical scale.

Weber's Law and Fechner's Law. Weber's law is $\Delta I/I = a$ constant, when ΔI is the stimulus increment for the j.n.d. and I the stimulus from which the increment is measured. Fechner's law, got, as we have seen, from assuming all j.n.d. subjectively equal and integrating Weber's law, is $S = k \log I$, when S is the magnitude of sensation in appropriate units and I the stimulus measured in terms of the absolute limen as the unit. The interminable argument of the late nineteenth century, the argument which made psychophysics seem to William James so sterile, was centered upon the validity of these two laws. There were two objections to Fechner's law. The first, the "quantity objection," was that you cannot measure sensation anyhow. The second was that the measurements when made do not support the law. We must deal cavalierly with both complaints.

The *quantity objection* was based upon introspection. A great intensity does not seem to contain within itself many small intensities. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow-candle within itself." "This sensation of 'gray,'" remarked Külpe, "is not two or three of that other sensation of 'gray.'" These sentences are but samples of what was debated *in extenso*. Greater magnitude is not greater complexity, their authors said. Stimulus-magnitude, however, is also not in immediate observation a matter of complexity. A kilogram weight is not one thousand gram weights, except as it can be shown to be equivalent by the technique of measurement with a balance. Similarly magnitude of sensation is only a statement of relation between sensations obtained in accordance with certain operations of measurement. Those operations were pretty well established by Del-

boeuf, and have been developed one stage further in the modern ratio scales of sensation.

The other complaint was valid if you speak strictly—although there is a rough sense in which Weber's law is true. There is, truly, a law of diminishing differential limens with increase of the stimulus intensity. It is more nearly correct to say that the differential limen is proportional to the stimulus than to equate it to any constant stimulus-difference. This gross fact it was that the defenders of Weber's law could never get out of mind. Presently, however, they came to realize that Weber's law does not hold at low intensities and often not at high intensities. It then became customary for them to say that it is true, but only in the middle ranges of intensity. The question of the degree in which Weber's law is factually verified is taken up in the proper places in this text (pp. 136–139, Fig. 10; pp. 339–346, Fig. 55; p. 443; p. 458; pp. 495–498, Fig. 83). The final generality is that the function plotted between $\Delta I/I$ and I , which should be a straight line if Weber's law is true, is really a curve, with values of $\Delta I/I$ decreasing rapidly as the low intensities increase and often rising a little again with increase of the high intensities. No part of the curve is indicated to be truly straight.

Fechner's law, being dependent upon Weber's law as well as on some other assumptions, naturally cannot be any more correct than Weber's law.

Notes

A great many of the matters touched upon in this chapter have been considered in *extenso* in E. G. Boring, *A History of Experimental Psychology*, 1929, to which appropriate reference is made below, and which cites many of the secondary sources and criticisms.

Sensation

On Greek empiricism and sensation as the means of knowledge, see any large history of philosophy, using the index.

On the early English empiricists, see Boring, *op. cit.*, 168–206. The important books are: Thomas

Hobbes, *Humaine Nature*, 1650; *Leviathan*, 1651; John Locke, *An Essay Concerning Human Understanding*, 1690; 4 ed., 1700, with the chapter on association of ideas; George Berkeley, *An Essay towards a New Theory of Vision*, 1709; *A Treatise Concerning the Principles of Human Knowledge*, 1710; David Hume, *A Treatise on Human Nature*, 1739–1740; David Hartley, *Observations on Man*, 1749.

For the French School, see René Descartes, *De homine*, 1662, or *L'homme*, 1664; E. B. de Condillac, *Traité des sensations*, 1754; J. O. de La Mettrie, *L'homme machine*, 1748; C. Bonnet, *Essai analytique*

sur les facultés de l'âme, 1760. See also the other citations in Boring, *op. cit.*, 207.

For the Scottish School, see Thomas Reid, *Inquiry into the Human Mind* on the Principles of Common Sense, 1764; *Essays on the Intellectual Powers of Man*, 1785; Dugald Stewart, *Elements of the Philosophy of the Human Mind*, I, 1792, II, 1814, III, 1827; Thomas Brown, *Lectures on the Philosophy of the Human Mind*, 1820, esp. Lects. 22–24 on muscular sensation; also the numerous writings of Sir William Hamilton.

On the later English associationists, see Boring, *op. cit.*, 208–236. The important books are: James Mill, *Analysis of the Phenomena of the Human Mind*, 1829, or the ed. with notes by J. S. Mill, 1869; John Stuart Mill, *op. cit.*; *Logic*, 1843; *Examination of Sir William Hamilton's Philosophy*, 1865.

On the contribution of physiology to sensationism, see Boring, *op. cit.*, 35–46 (general physiology), 77–94 (specific nerve energies), 95–114 (early research in sensation). The books cited in the text are: Albrecht von Haller, *Elementa physiologiae corporis humani*,*8 vols., 1757–1766, esp. V, 1763; *Prima lineae physiologiae*, 1747 *et seq.*, Eng. trans., 1766 *et seq.*; Charles Bell, *Idea of a New Anatomy of the Brain*, 1811 (privately printed edition of only 100 copies), reprinted, *J. Anat. Physiol.*, 3, 1869, 154–157; Johannes Müller, *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, esp. 44–55; *Handbuch der Physiologie des Menschen*, II, Bk. v, 1838, introductory section, or Eng. trans., 1842.

For the Wundtian tradition in sensationism, elementism and associationism, see Boring, *op. cit.*, 310–344 (Wundt), 361–430 (G. E. Müller, Ebbinghaus, Külpe, Titchener and some others). In particular, see W. Wundt, *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, up to

6 ed., 1908–1911; O. Külpe, *Grundriss der Psychologie*, 1893, Eng. trans., 1895; E. B. Titchener, *Outline of Psychology*, 1896; *Text-Book of Psychology*, 1910.

On modality and quality, see Boring, *Physical Dimensions of Consciousness*, 1933, 150–186, and in particular H. Helmholtz, *Die That-sachen in der Wahrnehmung*, 1878, 8–13, reprinted in his *Vorträge und Reden*, 1884, II, 223–226.

For the counts of discriminable sensations, see Külpe, *op. cit.*, 1893, 109 f., 125 f., 131 *et passim*, Eng. trans., 106, 121 f., 127 *et passim*; Titchener, *Outline, op. cit.*, 74 f.

On sensation as a continuous function of stimulus, see, in general, E. B. Titchener, *Experimental Psychology*, II, ii, 1905, cxlvi–cxlix and the score of references there cited; and, in particular, R. H. Lotze, *Metaphysik*, 1879, 513 f., Eng. trans., 1887, II, 212–214; O. Funke, in L. Hermann's *Handbuch der Physiologie*, III (2), 1880, 358 f.; C. Stumpf, *Tonpsychologie*, I, 1883, 351–353. Stumpf did not render final judgment on the matter, which has therefore sometimes been called “Stumpf's paradox.”

On behaviorism in general, see Boring, *op. cit.*, 1929, 580–589, 594 f. J. B. Watson's invention of behaviorism is Psychology as the behaviorist views it, *Psychol. Rev.*, 20, 1913, 158–177; his animal psychology, *Behavior, an Introduction to Comparative Psychology*, 1914; his human psychology, *Psychology from the Standpoint of a Behaviorist*, 1919.

On operationism, logical positivism, logical behaviorism and physicalism, see S. S. Stevens, *Psychology and the science of science*, *Psychol. Bull.*, 36, 1939, 221–263, with its annotated bibliography of 66 titles. The book that influenced psychologists most is P. W. Bridgman, *The Logic of Modern Physics*, 1928. For the first ways in which the psychologists were influenced, see Stevens, *The op-*

erational basis of psychology, *Amer. J. Psychol.*, 47, 1935, 323-330; The operational definition of psychological concepts (includes sensation), *Psychol. Rev.*, 42, 1935, 517-527; Psychology: the propaedeutic science, *Phil. Sci.*, 3, 1936, 90-103; E. C. Tolman, An operational analysis of 'demands,' *Erkenntnis*, 6, 1936, 383-392; Operational behaviorism and current trends in psychology, *Proc. 25th Anniv. Celeb. Univ. So. Calif.*, 1936, 89-103; Boring, Temporal perception and operationism, *Amer. J. Psychol.*, 48, 1936, 519-522. Against positivism in psychology, see K. Koffka, *Principles of Gestalt Psychology*, 1935, *passim* and esp. 684 f.; W. Kohler, *The Place of Value in a World of Facts*, 1938, *passim* and esp. vii. Movements gain strength, clarification, point and interest by opposition. Gestalt psychology, which got its start by stressing the opposition against itself, similarly now helps to clarify positivistic psychology by opposing it.

Perception

An excellent discussion of the older theories of perception *ante* 1785 is given by Thomas Reid, *Essays, op. cit.*, 1785, Essay II, Chap. 5.

For the Scottish School on perception and sensation, see Reid, *Inquiry, op. cit.*, 1764, Chap. 6, sects. 20, 21; *Essays, op. cit.*, 1785, Essay II, Chaps. 4, 7, 8; D. Stewart, *Elements, op. cit.*, I, 1792, 63-97; T. Brown, *Lectures, op. cit.*, 1820, Lect. 25, esp. 564-571.

On the two Mills, see E. G. Boring, *A History of Experimental Psychology*, 1929, 208-223, 234 f., where these matters are more fully considered; or the original sources, James Mill, *Analysis, op. cit.*, 1829, esp. Chap. 3; J. S. Mill, *Examination, op. cit.*, 1865, esp. Chap. 11.

On Wundt, see Boring, *op. cit.*, 310-344, and references there cited.

On Brentano and act psychology,

see Boring, *op. cit.*, 345-351, 369 f., 393-402, 427, 431-451.

On form-quality, see Boring, *op. cit.*, 433-440, 448-450. The important papers are: C. v. Ehrenfels, Ueber Gestaltqualitäten, *Vjschr. wiss. Phil.*, 14, 1890, 249-292; A. Meinong, Zur Psychologie der Komplexionen und Relationen, *Z. Psychol.*, 2, 1891, 245-265; H. Cornelius, Ueber Verschmelzung und Analyse, *Vjschr. wiss. Phil.*, 16, 1892, 404-446; 17, 1893, 3-75.

On the context theory of perception (meaning), see E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*, 1909, 174-194; *Text-Book of Psychology*, 1910, 364-373; and, thus incidentally, Boring, *op. cit.*, 185 f. (Berkeley), 408, 411 f., 428 f. (Titchener). For Titchener on meaning, see his Description vs. statement of meaning, *Amer. J. Psychol.*, 23, 1912, 165-182. There is an experimental literature on this theory. T. V. Moore, The temporal relations of meaning and imagery, *Psychol. Rev.*, 22, 1915, 177-225, publishing work from Külpe's laboratory, then at Munich, and thus defending imageless thought, showed that, in getting the meaning of a picture or a word, sometimes the image precedes the meaning and sometimes the meaning the image, a result which indicated that imagery is not essential to meaning. E. C. Tolman, More concerning the temporal relations of meaning and imagery, *ibid.*, 24, 1917, 114-138, repeated Moore's experiment and concluded that there are individual differences as to whether meaning or imagery comes first. H. P. Weld, Meaning and process as distinguished by the reaction method, *Studies in Psychology* (Titchener Commemorative Volume), 1917, 181-208, publishing from Titchener's laboratory, showed that it all depends upon how much meaning and how much imagery, and on what kind of meaning and what kind of

imagery, are required. A little meaning may precede a complete image, and a meaning carried in one kind of imagery (e.g., kinesthetic) may seem to precede imagery when the subject is instructed to report only another kind of imagery (e.g., visual). The remainder of this discussion is in Moore, Meaning and imagery, *Psychol. Rev.*, 24, 1917, 318-322; Image and meaning in memory and perception, *Psychol. Monogr.*, 27, 1919, No. 2, A. R. McDonough (who worked under Moore's direction), The development of meaning, *ibid.*, 27, 1919, No. 5; R. H. Wheeler, The development of meaning, *Amer. J. Psychol.*, 33, 1922, 223-233. The experiments have point in respect of Titchener's theory only because they dealt with new perceptions. Titchener agreed with the school of imageless thought that old perceptions can have meaning without imagery.

For E. C. Tolman's systematic views on the way in which stimulus-and-response and situation-and-behavior account for perception, cognition and meaning, see first his initial paper, Nerve process and cognition, *Psychol. Rev.*, 25, 1918, 423-442; then the six other papers cited in Boring, *op. cit.*, 594, which show his views developing; then their culmination in *Purposive Behavior in Animals and Men*, 1932; and finally the notes of the preceding section which cite the articles in which he was becoming an operational behaviorist.

To see why Gestalt psychologists think that phenomena are given as meaningful, one has but to read almost any paper by Wertheimer, Koffka or Köhler since 1920. W. Köhler's *The Place of Value in a World of Facts*, 1938, is founded on this conception. See also K. Koffka, *Principles of Gestalt Psychology*, 1935, esp. Chaps. 1, 15, and, on double representation, 178-183, 260-264. Whether conscious meanings do or do not 'exist' is still debatable.

Attributes

In general on the doctrine of attributes, see E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Lect. 1; E. G. Boring, *Physical Dimensions of Consciousness*, 1933, esp. Chap. 2.

Altogether one finds in Wundt, given the right context, the following words that mean attribute: *Merkmal, Eigenschaft* [der Empfindung], [Gefühl]ton, *Bestimmungsstücke, Bestandtheil, Stimmungscharakter, Färbung*, but not the uncommon German word *Attribut*. On feeling in Wundt's system as an attribute and with attributes, see Titchener, *op. cit.*, 125-133.

On the doctrine of attributes, in its first phase, see O. Külpe, *Grundriss der Psychologie*, 1893, 30-33 (doctrine), 230-236 (feeling), Eng. trans.; 29-32, 225-230; G. E. Müller's discussion of qualitative and intensive series, *Zur Psychophysik der Gesichtsempfindungen*, *Z. Psychol.*, 10, 1896, 1-82, esp. 2 f., 25-36; Titchener, *loc. cit.* Then see also E. B. Talbot, The doctrine of conscious elements, *Phil. Rev.*, 4, 1895, 154-166; M. W. Calkins, Attributes of sensation, *Psychol. Rev.*, 6, 1899, 506-514; M. F. Washburn, Some examples of the use of psychological analysis in system-making, *Phil. Rev.*, 11, 1902, 445-462.

For E. B. Titchener on attention as attributive clearness, see his *op. cit.*, 1908, 171-206; *Text-Book of Psychology*, 1910, 265-281; *Beginner's Psychology*, 1915, 90-103 (vividness). On his use of the term *at-tensity*, see *loc. cit. infra*, 1924.

Külpe's experiment (with W. L. Bryan) is *Versuche über Abstraktion*, *Ber. I Kongr. exp. Psychol.*, 1904, 58-68. C. Rahn's critique, built in part upon this experiment, is The relation of sensation to other categories in contemporary psychology, *Psychol. Monogr.*, 16, 1913, No. 1. Titchener's reply and reorientation

of his system toward dimensions is his Sensation and system, *Amer. J. Psychol.*, 26, 1915, 258-267. Titchener never published on his dimensionalism, except a tiny note, The term 'attensity,' *ibid.*, 35, 1924, 156. The present author's reinterpretation of Titchener's dimensionalism, *op. cit.*, 1933, is the work of a younger man trying to make his master clear and simple, and would probably have been no more accepted by the master than was Külpe's similar attempt (1893) to clarify and modify Wundt.

For the repetitions and modifications of Külpe's experiment, see M. Yokoyama as reported by E. G. Boring, Attribute and sensation, *Amer. J. Psychol.*, 35, 1924, 301-304; R. W. Wilcocks, An examination of Külpe's experiments on abstraction, *ibid.*, 36, 1925, 324-340; D. W. Chapman, Relative effects of determinate and indeterminate Aufgaben, *ibid.*, 44, 1932, 163-174; P. M. Zoll, The pluridimensionality of consciousness, *ibid.*, 46, 1934, 621-626.

For S. S. Stevens' determinations of isophonic contours for tonal volume and density and his conception of an attribute as a sensory parameter, see his The attributes of tones, *Proc. nat. Acad. Sci. Wash.*, 20, 1934, 457-459; Volume and intensity of tones, *Amer. J. Psychol.*, 46, 1934, 397-408; Tonal density, *J. exp. Psychol.*, 17, 1934, 585-592. For a similar conception of the attributes of color, see E. G. Boring, Isochromatic contours, *Amer. J. Psychol.*, 39, 1937, 130-134. On the general relation of attributes to the stimulus and the number of possible attributes, see Boring, The relation of the attributes of sensation to the dimensions of the stimulus, *Phil. Sci.*, 2, 1935, 236-245.

Unity of the Senses

In general, see G. W. Hartmann, *Gestalt Psychology*, 1935, 141-151. The basic, if somewhat poetic, paper

is E. M. von Hornbostel, Die Einheit der Sinne, *Melos: Z Musik*, 4, 1925, 290-297, [n.v.], Eng. trans., *Psyche*, 7, No. 28, 1926, 83-89. See also C. Hartshorne, *The Philosophy and Psychology of Sensation*, 1934, esp. 29-36, 209-242, 277. Von Hornbostel's triangular equation of brightnesses is his Ueber Geruchshelligkeit, *Arch. ges. Physiol.*, 227, 1931, 517-538, which was not confirmed by N. E. Cohen, Equivalence of brightnesses across modalities, *Amer. J. Psychol.*, 46, 1934, 117-119.

For older comparisons, see, in general, E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 190 f.; and, in particular, W. Wundt, *Beiträge zur Theorie der Sinneswahrnehmung*, (1858) 1862, 33-40; H. Münsterberg, *Beiträge zur experimentellen Psychologie*, III, 1890, 56-91.

Nativism and Empiricism

In general, on the nativistic and empiricistic theories, see T. Ribot, *La psychologie allemande contemporaine*, 1879, Eng. trans., 1886, Chaps. 3, 4 (dreary); J. von Kries, in H. Helmholtz's *Handbuch der physiologischen Optik*, 3 ed., III, 1910, 497-511, 520-534, Eng. trans., III, 1925, 607-625, 635-652 (best reference); O. Klemm, *Geschichte der Psychologie*, 1911, Eng. trans., 1914, Chap. 11; G. S. Brett, *A History of Psychology*, III, 1921, 110-116. James' chapter on space perception is classic and replete with references to, and discussion of, this literature: W. James, *Principles of Psychology*, 1890, II, 134-282. See also the further discussion in this book, esp. pp. 233-238, and also pp. 475-483.

For Johannes Müller's nativism, see his *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, esp. 71-90.

For Ewald Hering's nativism, see esp. his *Beiträge zur Physiologie* (which is all *Zur Lehre vom Ortsinne der Netzhaut*), 1861-1864. Von

Kries has noted that Hering admitted his sympathy with Johannes Müller's views. In this instance, Hering followed Müller, and Helmholtz rejected Müller, though Helmholtz applauded Müller's doctrine of specific nerve energies as the greatest psychological law of that day.

For C. Stumpf's nativism, see his *Ueber den psychologischen Ursprung der Raumvorstellung*, 1873. See also E. G. Boring, *A History of Experimental Psychology*, 1929, 352 f. For Stumpf's phenomenology, see Erscheinungen und psychische Funktionen, *Abh. preuss. Akad. Wiss., phil.-hist. Cl.*, 1906, No. 4.

For R. H. Lotze's empiricism, see his *Medizinische Psychologie*, 1852, 325-452, and scattered references in his later philosophical works. See also Boring, *op. cit.*, 250-259, 261 f.

For H. Helmholtz's empiricism, see primarily his *Die Thatfachen in der Wahrnehmung*, 1878 (or 1879), reprinted in *Vorträge und Reden*, 1884, II, 217-271. Helmholtz first had this idea in 1855. It is implicit in the thought of the *Optik*, 1856-1866, and explicit, for instance, in his discussion there of perceptual unconscious inference as learned; see the *Optik*, *op. cit.*, 1st ed., 1866, or 3 ed., or Eng. trans., III, sect. 26. Three papers on the axioms (1866-1878) are reprinted in Helmholtz's *Wissenschaftliche Abhandlungen*, II, 1883, 610-660. See also J. G. McKendrick, *Hermann Ludwig Ferdinand von Helmholtz*, 1899, 250-267; G. S. Hall, *Founders of Modern Psychology*, 1912, 256-259; von Kries, *loc. cit.*; Boring, *op. cit.*, 296-303, 308 f.

For W. Wundt's empiricism, see its beginning in his *Beiträge zur Theorie der Sinneswahrnehmung*, 1862, esp. 66-104 (1859), 376-451 (1862); *Vorlesungen über die Menschen- und Tierseele*, 1863, Lect. 17 (not in 2 ed.). For the final form of his views, see his *Grundzüge der physiologischen Psychologie*, 6 ed., III, 1911, 530-736, esp. 702-736.

On Külpe and Titchener, see O. Külpe, *Grundriss der Psychologie*, 1893, 346-393, Eng. trans., 331-379; E. B. Titchener, *Text-Book of Psychology*, 1910, 303-306, 321-326, 330-332, 364-471.

Psychophysics

Many of the matters mentioned briefly in the text are discussed at greater length elsewhere in this book; see text for cross-references.

The incomparable secondary source for the history of psychophysics is E. B. Titchener, *Experimental Psychology*, II, ii, 1905, xiii-clxxi, 99-121, 160-187, 191-194, 210-232, 275-318. Külpe called this book the most erudite psychological book in English. On G. T. Fechner's account of his own antecedents, see his *Elemente der Psychophysik*, 1860 or the 1889 reprint, 548-560. On Fechner, see E. G. Boring, *A History of Experimental Psychology*, 1929, 265-287.

J. F. Herbart's classical work is *Psychologie als Wissenschaft, neu gegründet auf Erfahrung, Metaphysik und Mathematik*, 1824-1825. On Herbart, see Boring, *op. cit.*, 238-250, 259-261; and on Leibnitz, 165-168, 177 f. The birth of Weber's law occurs in E. H. Weber, *De pulsu, resorptione, auditu et tactu: annotationes anatomicae et physiologiae*, 1834, 86-92, 132-142, 159-161; *Der Tastsinn und das Gemeingefühl*, in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 546-549, or 96-100 in the 1905 reprint.

J. R. L. Delboeuf's chief contributions to psychophysics are as follows: *Étude psychophysique*, *Mém. Acad. Sci. Belg.*, 23, 1873, No. 5; *Théorie générale de la sensibilité*, 1876; both reprinted together with slight abridgment as *Éléments de psychophysique*, 1883; *Examen critique de la loi psychophysique*, 1883. G. E. Müller's books are *Zur Grundlegung der Psychophysik*, 1878; *Die Ge-*

sichtspunkte und die Tatsachen der psychophysischen Methodik, in L. Asher and K. Spiro, *Ergebnisse der Physiologie*, Jhrg. II, abth. ii, 267-516, which is more often seen as the reprint with separate paging of 1904. W. Wundt's discussions of psychophysics are scattered through the six editions of the *Physiologische Psychologie*, 1874-1911. J. Merkel published 14 articles on psychophysics in the *Phil. Stud.*, vols. 4-10, 1887-1894. Titchener's text is cited above. F. M. Urban's classic is *The Application of Statistical Methods to the Problems of Psychophysics*, 1908; but see also *opp. cit. infra*. A modern text combining the psychophysical and statistical methods is J. P. Guilford, *Psychometric Methods*, 1936.

On the history of the *method of limits*, see Titchener, *op. cit.*, 99-121. On sensory quanta, see S. S. Stevens, C. T. Morgan and J. Volkman, Theory of the neural quantum in the discrimination of loudness and pitch, *Amer. J. Psychol.*, 54, 1941, 315-335.

On the history of the *method of right and wrong cases* (constant stimuli), see Titchener, *op. cit.*, 275-318. K. Vierordt inspired the first, if crude, use of this method by his pupil, F. Hegelmaier, Ueber das Gedächtniss für Linearanschauungen, *Arch. physiol. Heilk.*, 11, 1852, 844-853. Then came Fechner, *op. cit.*, I, 93-120; Müller, *op. cit.*, 1878, 7-55; Ueber die Massbestimmungen des Ortssinnes der Haut mittelst der Methode der richtigen und falschen Fälle, *Arch. ges. Physiol.*, 19, 1879, 191-235 (weights for frequencies, method of least squares); *op. cit.*, 1904, 12-163; Urban, *op. cit.*, 1908, 19-39, 99-171; Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. ges. Psychol.*, 15, 1909, 261-355; 16, 1909, 168-227. As to whether the psychometric functions really are integrated normal probability func-

tions, see Urban, The method of constant stimuli and its generalizations, *Psychol. Rev.*, 17, 1910, 229-259; Boring, Is there a generalized psychometric function?, *Amer. J. Psychol.*, 35, 1924, 75-78.

On the elimination of the *equal* category and the use of the probable error instead of the limen, see C. S. Peirce and J. Jastrow, On small differences of sensation, *Mem. nat. Acad. Sci.*, 3 (1), 1884, 75-83; G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences*, 1892, esp. 1-20, 150-152. On the way in which instruction and attitude reduce or eliminate the *equal* judgments, see Warner Brown, The judgment of difference, *Univ. Calif. Publ. Psychol.*, 1, 1910, No. 1, esp. 28-34, 41-51; S. W. Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, *Amer. J. Psychol.*, 25, 1914, 538-543. That, when attitude is controlled, *equal* need not mean *doubtful* has been shown by D. Cartwright, Relation of decision-time to the categories of response, *ibid.*, 54, 1941, 174-196.

On the history of the *method of average error*, see Titchener, *op. cit.*, 160-187. On the *method*, see Fechner, *op. cit.*, I, 120-128.

On the history of the *method of equivalents*, see Titchener, *op. cit.*, 191-194. On the early use of the method, see Weber, *De pulsu*, *op. cit.*, 1834, 94-113, *Der Tastsinn und das Gemeingefühl*, in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 548 f., or 98-100 in the 1905 reprint; Wundt, *Beiträge zur Theorie der Sinneswahrnehmung*, (1858) 1862, 34-43; Fechner, *op. cit.*, II, 315-322.

On the history of the *method of equal sense-distances*, see Titchener, *op. cit.*, 210-232. The early experiments are J. A. F. Plateau, Sur la mesure des sensations physiques, *Bull. Acad. Sci. Belg.*, 2 ser., 33, 1872, 376-388; J. R. L. Delboeuf,

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Chapter 2

PHYSIOLOGY OF SENSATION

IN this chapter we consider the history of the general problems of the physiology of sensation: conduction and its relation to the perception of quality and intensity, specific nerve energies and the general theory of quality, projection in its relation to the perception of quality and extension, and the problem of mind and body as it appears in the theories of psychophysical parallelism and isomorphism.

Nerve Conduction

How does the incorporeal soul, residing within the brain, move the muscles of the body to which it is attached? One answer to this question was the following. The inspired air of the lungs passes to the heart, where it is distilled into the vital spirits which, distributed to the body through the arteries, constitute the breath of life. Coming to the brain, the vital spirits are there, however, changed into animal spirits, which are stored in the brain's ventricles or conveyed to the muscles through the nerves to produce motion. These animal spirits, entering the muscle, distend it, causing it to shorten and to move the member to which it is attached. Anyone feeling the contracted biceps knows that a muscle swells when it is activated by the soul. The soul itself is these spirits, a *Pneuma*. That was the pneumatistic doctrine of Erasistratos in the third century before Christ (ca. 258 B.C.), a doctrine which lasted a long time.

Galen (ca. 175 A.D.) accepted Erasistratos' doctrine, complicating it. The natural spirits, necessary for growth and nutrition, are distilled, he believed, in the liver. In the heart, however, they are combined with air from the lungs to produce the vital spirits, essential to life, which in turn are transformed in the brain into the animal spirits for distribution through the nerves to the body. Mankind seems always in civilized ages to have needed the concept of *spirit*, an incorporeal substance which constitutes the es-

sence of a corporeal object. In man it was plain that life and the soul are contained in the body without occupying any space there, that they are in character spiritual.

When Descartes (d. 1650; published, 1650, 1662) came to this problem of neural conduction, he followed Erasistratos and Galen in accepting the animal spirits as the essence of the soul, and their penetration of the pores of the brain and conduction through the nerves as the reason for movement. The muscles contract, he repeated, because the animal spirits inflate them. The soul, conterminous with the body, is inextended substance; the spirits are a "very quick wind," "a very active and pure fire." One begins to get at this time the notion that the animal spirits have in themselves force, are, as Huygens put it in 1669, a *vis viva*; the soul directs the active spirits to the muscles and does not have to pump the muscles full of them.

Willis in his important *De motu animalium* of 1670 accepted the doctrine of animal spirits, as did Boerhaave later in 1708; yet already the paradox of an incorporeal substance was beginning to trouble the theorists. The trend toward a more substantial mechanical action began early.

It was Glisson in 1677 who performed an experiment and also invented an important concept. The experiment consisted in the use, obviously the first, of a 'plethysmometer': the arm was inserted downward in a long glass vessel, which was filled with water through a funnel at the top; when the muscles of the arm were contracted, the water did not rise in the funnel. In other words, the muscle, though it may change shape on contraction, is not enlarged; nothing is added to it to make it swell. Glisson's important concept was *irritability*: muscular tissue is in itself irritable, he said; it contracts when stimulated, not because it is inflated by another substance, but because contraction is inherent in its nature. With this word Glisson introduced into physiology the notion that serial bodily action results not from the transmission of any substance, fluid or spirit, but from the release of action inherent in the nature of the tissues themselves.

Borelli (1680) followed Glisson. He held that a corporeal agent, the *succus nervus* (not the incorporeal animal spirits), is transmitted through the spongy ducts which are the nerve fibers; yet he left the matter open as to whether the transmission might not be the conduction of "some emotion" along the nerves rather than

a material *succus nervus*. In any case, he asserted, something new must, in contraction, be added to the muscles; "on the arrival of the influence transmitted by the nerves there takes place [in a muscle] something like a fermentation or ebullition, by which the sudden inflation of the muscle is brought about." He did not, therefore, settle the question as to whether the *succus nervus* inflates or merely irritates the muscle.

There was another important experiment performed by Swammerdam (d. 1680) about 1660, although it was not published until 1737, long after its author's death. Swammerdam used a nerve-muscle preparation to show that the muscle contracts when the nerve, thus separated from the central nervous system, is stimulated by pressure. That result certainly tells against the animal spirits and for irritability. Swammerdam also showed that the volume of the muscle, when thus contracting, does not change, thus anticipating Glisson, although his failure promptly to publish his results robbed him of the immediate influence that his discovery might otherwise have had.

In the eighteenth century the notion of animal spirits or *vis viva* persisted in the common thought of physiologists but was looked upon askance by the sophisticates, who, if they used the terms, gave them a more mechanical connotation. Robert Whytt, a pioneer in the study of reflex action, spoke of animal spirits in 1751 but specified that he meant only "the power or influence of the nerves." Unzer in 1770 substituted the phrase *vis nervosa*.

Meanwhile the knowledge of electricity was growing. Gilbert worked out the laws of electrical attraction and repulsion in 1600. Guericke invented the friction machine in 1672. The Leyden jar dates from 1745. Benjamin Franklin reported his experiments on electricity in 1774. It was at some time before 1780 that Galvani started his experiments on the stimulation of frogs' legs (nerve-muscle preparations) by electric discharges. Thus by 1791 he had constructed the first electric battery out of a frog's leg and two metal rods. These experiments are the reason that direct currents from a battery were later called galvanic currents—although the really effective pioneer in the invention of the battery was Volta (1800). The first galvanometer was made in 1811; the first one sensitive enough to be practicable for measurement, in 1821. Ohm formulated his well-known law for galvanic currents in 1827, and Fechner, then a promising young physicist, made his first important

contribution to science in 1831 on a problem of the galvanic chain. It was also in 1831 that Faraday discovered electromagnetic induction, a discovery which resulted in the invention of the induction coil and the consequent availability of faradic currents for the stimulation of nerve and muscle. In these years physiology and physics helped each other: the first battery was a live frog's leg, yet without faradic currents to stimulate nerve and muscle and without galvanometers to measure physiological potentials, du Bois-Reymond would never have published in 1848 the first volume of his famous work on animal electricity. Now let us go back to Galvani.

Galvani worked mostly with frogs' legs to which the nerve and lower section of the spinal cord had been left attached. His initial discovery was that the legs, when hung from an iron trellis by brass hooks passing through the attached section of the cord, would twitch when discharges from an electric machine or a Leyden jar occurred in the neighborhood, or when lightning flashed in a thunder storm. Later he found that he could cause a leg to kick at any time by completing a circuit from the foot to the nerve through two rods of different metals. The end of one rod was placed in contact with the foot; the end of the other rod touched the nerve. When the free ends of the two rods were brought together, the muscles contracted, the leg kicked. A connection from foot to nerve by a single rod would not work; two metals are required. A leg, suspended by the nerve from a brass hook connected to the ground and with its foot touching a silver plate, would continue kicking indefinitely, since each kick broke the connection, allowing the leg to drop back into contact with the plate. From these experiments Galvani concluded that animal tissues generate electricity. He presented this theory in *De viribus electricitatis in motu musculari*, published in 1791. He was, of course, right in his belief in animal electricity: a living organism establishes potential differences in different parts of itself. On the other hand, he was wrong in assuming that his experiments proved the point conclusively. Why was one metal not enough? Why did he have to complete the circuit with two? What he had here was simply the world's first moist battery, with the frog's tissues providing the electrolyte.

Volta was prompt to make this criticism. He verified Galvani's experiments, emphasizing the dependence of the contraction upon

there being two different metals in contact in the circuit. To prove his point he substituted paper moistened with water, brine or lye for the frog's leg and nerve, connecting many of these units in series to get a very considerable electromotive force. His first arrangement, described by him in 1800, was the Voltaic pile, as it came to be called: he stacked in a pile a disk of silver, a disk of moist paper, a disk of zinc (these three make the first unit), and then more disks, silver, paper, zinc, silver, paper, zinc. Between the topmost zinc and the bottommost silver a very considerable electromotive force existed. He also got the same effect with a series of cups, each containing a liquid electrolyte, a silver plate and a zinc plate—with the silver of one cup fastened, outside the cups, to the zinc of the next. Such series were presently called galvanic chains. Volta finally clinched his argument against Galvani by making out a list of pairs of metals in the order of their electric effectiveness. He was able to arrange the metals in such a series that the remoteness of the metals in the series predicted their joint effectiveness in the galvanic chain. Thus he built upon Galvani's discovery to invent the electric battery and series connection, turning attention away from animal to inorganic electricity.

Just the same there is 'animal electricity.' Johannes Müller, dismissing in 1834 the animal spirits as the nerve-theory of the ancients, nevertheless suggested that the nervous impulse might be electrical. Its speed, which he thought too rapid ever to be measured, seemed to him to imply that electricity might be the agent.

Several men were working on these problems. It was in 1841 that Matteucci presented to the Académie des Sciences a paper which showed that a galvanometer indicates a current when connected between the surface of a muscle and a wound in the muscle, a current that was later called the "current of injury" and also the "current of rest," for it flowed without observable muscular contraction. Müller showed this paper to his brilliant pupil (later his successor at Berlin), du Bois-Reymond, whose interest was at once captured by the topic. Du Bois, publishing his first paper in this field in 1843, had advanced so far by 1848-1849 that he could then issue his two—now classical—volumes on *thierische Elektrizität*. In them he formulated what amounts to a theory of the polarization of animal tissues, for he suggested that muscle and nerve consist of electrically charged molecules, with a posi-

tive charge on one face and a negative opposite. These molecules, he argued, must orient themselves—much in the way that oriented magnetized particles make up a big magnet with a north pole at one end and a south at the other. Obviously this theory was not correct; yet it was an advance in the right direction because it led toward the notion of polarization.

It was du Bois-Reymond's experiments that led Helmholtz in 1850 to attempt to measure the velocity of the nervous impulse—and to succeed. Haller had estimated the speed at somewhere near 150 feet per second, but Johannes Müller, already suspecting a relation between neural and electrical conduction, had thought the velocity so great, perhaps comparable to the speed of light, that it might never be measured—since it would never be possible, he said, to have a single length of nerve stretching through as great a distance as is required for the measurement of the velocity of light. Müller, nevertheless, made some reservation on this matter, for he noted that the experiments of the astronomer Bessel on the personal equation (reaction times for the observation of stellar transits) show that impulses can be slowed up in such manner that a visual impression, starting before an auditory one, may nevertheless be perceived after it. Anyhow Helmholtz, inventing the myograph (a smoked glass plate on a falling pendulum which records tracings of muscle twitches and instants of stimulation; the antecedent of the kymograph), did measure the speed of the impulses at about 27 meters per second in the frog's nerve. He merely determined the difference in time between stimulus and contraction for stimulation at each of two different points on the nerve, dividing this difference in time by the distance between the points. At first there was great skepticism, but Helmholtz was right.

As research went on, it became apparent that the nerve impulse is electrical in nature and also that it is not simply an electric current. Bernstein, a pupil of both du Bois-Reymond and Helmholtz, showed in 1866 that the impulse is a "wave of negativity" passing along the nerve. He placed electrodes at successive points along the nerve, finding that the surface of the nerve in the region of the rapidly moving impulse is always electrically negative with respect to the surface ahead of the impulse and behind it. It is as if the impulse were an injury passing along the nerve, for an injury is negative with respect to the uninjured surface; or, to put

the same idea in other words, it is as if the impulse were a moving region where the negative inside of the nerve spreads to the outside. It was only a short step from this position, which Bernstein amplified and expounded fully in 1871, to the membrane theory which he helped to establish much later in 1902. The wave of negativity in 1871 became in 1902 a wave of depolarization.

Since the wave of negativity has a measurable duration, one would expect some limit to the rate at which impulses can succeed each other. Kronecker in 1874 found this limit for heart muscle; if a second stimulus comes too soon after a first, it does not excite at all. In 1876 Marey, well known for his development of the graphic methods, coined the term *refractory phase* to designate the interval during which the second stimulus is ineffective. He, too, was working with the muscle of the heart. It was Gotch and Burch who, in 1899, showed that nerve, like muscle, is refractory. "A second electrical response does not occur if a second stimulus succeeds a predecessor at less than a certain interval." This interval, they showed, varies greatly with the temperature of the nerve. Gotch was not sure in 1899 whether their experiment might not have involved an arti-

fact resulting from an inadequate means of measuring the presence of the impulse, but in 1902, with a capillary electrometer, he clinched the matter. A decade later (1912) Adrian and Lucas plotted the curve of the recovery of excitability after an initial stimulation (Fig. 3), showing that there is at first an *absolute refractory period* during which no stimulus is strong enough to excite the nerve, and that it is followed by a *relative refractory period* in which excitability returns gradually to normal. (Their initial finding of supernormal excitability just preceding normal, Fig. 3, was not born out by later research.)

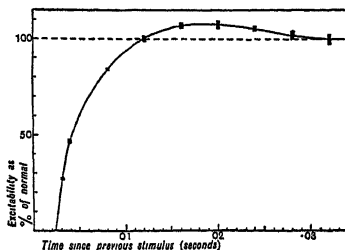


FIG. 3. EXCITABILITY AND REFRACTORY PHASE: ADRIAN AND LUCAS (1912)

Excitability of nerve (reciprocal of threshold value; see Fig. 4) as a function of the interval by which the stimulation follows a previous stimulation. 100 = normal excitability. The supernormal phase (above 100) turned out later to be an artifact; the function should approach normal excitability as an asymptote. From Adrian and Lucas, *J. Physiol.*, 44, 1912, 114.

The facts about refractory phase are best shown by the curve for the threshold of excitation during recovery after initial stimulation (Fig. 4). This function, however, tells us nothing at all about the magnitude of the threshold response, which is, of course,

governed by the *all-or-none law*.

That law dates from Bowditch's research in 1871, when he found that any stimulus produces in heart muscle either the maximal response, or, if the stimulus is too weak, no response at all. An intermediately strong stimulus does not produce an intermediately strong response, for the stimulus acts only to release the irritability inherent in the muscle fiber. Lucas in 1905 extended this finding to skeletal muscle, showing that a continuous increase in the strength of the stimulus at the nerve results in a graded series of responses in the muscle: the increase in response is stepwise.

In 1909 he named this relationship the *all-or-none principle*. Working at that time on a very small muscle supplied by a nerve with only eight or nine fibers in it, he discovered that the muscular response was stepwise and that there were never more steps than there were fibers in the nerve.

Actually this finding implied that the all-or-none principle might reside in nerve as well as in muscle, but the matter was not fully settled until Adrian, Lucas' pupil, showed in 1912 that an impulse, after passing through a region of the nerve where its strength has been decreased by a narcotic, spontaneously recovers its full strength, provided only that it succeeds in getting through the narcotized region at all. He passed the nerve through a small chamber, within which it was exposed to alcohol vapor, measuring the action current (Bernstein's wave of negativity) before the chamber, in the chamber and after the chamber. He was wrong in supposing that the strength of response diminished progressively within the chamber (conduction with a decrement; Kato in

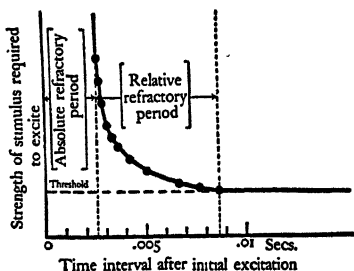


FIG. 4. THRESHOLD AND REFRACTORY PHASE: ADRIAN (1913)

Absolute and relative refractory periods as shown by threshold value of a second stimulus. This sort of function is the basis for the function of Fig. 3, but this particular curve shows no supernormal phase as does Fig. 3. Adapted from Adrian by Boring, *Physical Dimensions of Consciousness*, 1933, 39.

1924 showed that to be an artifact), but right in concluding that normal nerve gives its all, if the stimulus is strong enough, or nothing if the stimulus is too weak. If any doubt remained about this matter, it was removed by Adrian's second paper in 1913. It is hard to say whether Lucas or Adrian, the master or his pupil and successor, established the all-or-none law for nerve. Lucas'

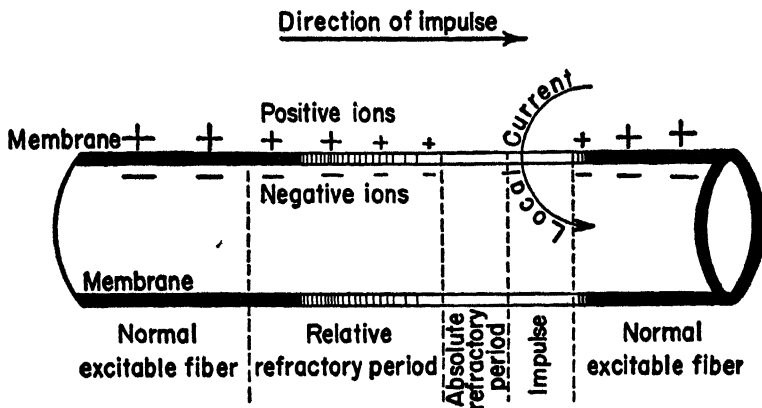


FIG. 5. MEMBRANE THEORY OF NERVE-CONDUCTION

See text for explanation. The semi-permeable membrane is black; the permeable membrane at the region of the impulse is white; continuous recovery of partial impermeability during the relative refractory phase is cross-hatched. Adapted from Forbes by Boring, *Physical Dimensions of Consciousness*, 1933, 40.

untimely death in 1916 cut short his work, and it was left for Adrian to publish Lucas' lectures summarizing the knowledge about nerve-conduction as of that time.

Closely connected with the facts of refractory phase and all-or-none response is the *membrane theory* of nerve-conduction. This theory supposes that the nerve is covered with a semi-permeable membrane (see Fig. 5), and that it is polarized—with the small positive ions penetrating the membrane and rendering the surface electrically positive, and the large negative ions bound electrically inside the membrane. Stimulation breaks down the partial impermeability of the membrane, initiating a local eddy current, which is the impulse. This local current itself destroys the impermeability of the adjacent membrane, thus extending itself to the next region, leaving the membrane fully permeable and thus

depolarized and refractory. The nerve, however, soon recovers excitability, passing through a phase of relative refractoriness and presently reestablishing its normal polarization. Such is the membrane theory.

It was the chemist, Ostwald, who, in 1890, first proposed this theory. He worked out the electrochemistry of the process without experimentation upon actual tissues. The theory was fairly well established by Bernstein in 1902, who found that the facts of conduction in both muscle and nerve vary with temperature after the manner in which membranous permeability ought to vary. He called the impulse a concentration current. The weight of evidence for the membrane theory was, however, accumulated by R. S. Lillie in a long series of publications beginning in 1909. In 1920 he duplicated the neural phenomenon with an iron wire immersed in nitric acid. Such a wire has a membrane of oxide formed upon it. The acid supplies the positive ions on the outside. Any stimulus that breaks the film of oxide, like a scratch or an electric shock, sets up a concentration current which travels in both directions, propagating itself by destroying the film ahead of it. Then the wire remains refractory after the passage of the impulse, until the acid reforms the polarizing film. The adequacy of such a model in duplicating events that are proper to a nerve fiber has gone a long way toward securing the acceptance of the membrane theory.

• Sensory Conduction

In the days of the animal spirits and the *vis nervosa* there was no very special theory of sensation. The spirits were supposed to go through the nerves from the brain to the muscles to produce motion, or from the sense-organ to the brain to produce sensation. Haller (1747), who thought of the nerves as tubes and the spirits as a watery, albuminous juice, noted that muscular motion, as seen in convulsions, can be more violent than sensation. He suggested that this difference occurs because the brain is at the top of the organism, so that the course of the spirits is always downward for motion, whereas in sensation the organs of sense have to force the spirits upward. There might sometimes, he thought, be conflict and then the downward movement would prevail: sensation occurs only when "it is resisted by no sensitive torrent coming from the brain in the opposite direction."

Sensation came, however, into its own as a separate physiological

problem with the attention gained for it by the Bell-Magendie law—the rule that the sensory and motor nerve roots at the spinal cord are separate. It was, of course, not entirely a new notion of the nineteenth century that movement and sensation should have separate nerves. Erasistratos had said that they do. So had Galen, Descartes and Whytt. Beliefs without experiments are, however, not always convincing. Certainly they did not convince Haller, whose appetite for both evidence and the opinions of others was omnivorous. Bell it was who, in an obscure pamphlet privately printed for his friends in 1811, described how he sectioned the posterior nerve-root at the spinal cord without convulsing the muscles, whereas touching the corresponding anterior root with his knife, after he had cut the posterior root, produced violent movement. From such evidence he concluded that the posterior roots have only a sensory function, and the anterior roots only a motor function. Magendie, in 1822, not having seen Bell's private publication, described a much more convincing experiment. He sectioned the posterior root to find that the limb supplied by it was quite insensitive: no pricking or pressing could get it to move. He was just about to conclude that it was paralyzed, when the animal moved the limb spontaneously, proving to both Magendie and us that the limb was not paralyzed, but only anesthetic. Section of the anterior root, on the other hand, destroyed all movement, whether the dorsal roots were cut or not, except, of course, when Magendie applied the stimulus to the distal side of the cut. Thus Bell had the idea first, but Magendie made the same discovery independently, performing a more convincing experiment. History has settled the controversy that arose about priority by naming this law of the spinal roots for both of these great physiologists.

It was in this same obscure paper that Bell noted that every sense has its own peculiar quality, that pressure on the eye gives a sensation of light and that a touch on a taste papilla gives a gustatory sensation. Johannes Müller in 1826 expanded this kind of argument into what became presently (1838) the famous doctrine of the specific energies of nerves—a doctrine which we must consider in the next section of this chapter. Here we may note only that the doctrine represented a five-fold subdivision among the sensory nerves. Instead of a single *vis nervosa* for all motor and sensory nerves, Müller asked us to believe in five sensory *vires*

nervosae—call them what you will—*vis*, *Kraft*, *force*, *Energie*—there was not much difference in the use of those words in the days before the theory of conservation of energy became important. Later, of course, Helmholtz and others hypothesized a different energy for every quality within a sense—but see the next section.

The character of the nerve impulse was determined almost entirely, as we have seen, by experiments with motor nerves. Galvani could see the frog's leg kick. Gotch could record muscle twitches on a kymograph. Ultimately it became quite clear that the principles of refractory phase and of the all-or-none response apply alike to muscle fiber and to motor nerve fiber. The facts, worked out for muscle first, were then found true for nerve. It was supposed that sensory nerves must be like motor nerves, but at first no one was sure.

Knowledge of the nature of sensory conduction had to wait, however, not only until motor conduction was understood, but also until the advances in physical science provided suitable apparatus for measuring action potentials in nerve. In the nineteenth century one could learn something about motor conduction because one had a muscle at the end of the nerve to give observable twitches. At the end of a sensory nerve there was only introspection and no one could tell whether the differences between stimulus and perception occurred in the peripheral or the central nervous system. It is true that the nineteenth century had galvanometers but they were not sensitive enough. In the twentieth century there appeared first the sensitive string galvanometer, then the capillary electrometer, then the method of thermionic amplification, then the cathode ray oscillograph, and lately all sorts of methods for observing and recording the frequency, amplitude and wave-form of amplified currents. For these later techniques physiology owed its thanks to the development of the radio.

Thus it has come about that the great advances in the knowledge of afferent conduction—not the very beginnings but most of the important insights—belong to the 1920's and 1930's. The literature is large, but we may summarize it under three topics. Adrian and Forbes were the pioneers.

1. *Sensory action potentials.* Steinach in 1896 seems to have been the first to measure sensory action potentials. He found that the wave of negativity in frog's nerve varies directly with the pressure of the stimulus on the frog's skin. After Steinach there

were two or three other measurements of potentials in active somesthetic nerve, and then in 1910 Buitendijk recorded, with electrodes on the auditory nerve, potentials that were synchronous with a heard percussion shot. We now know that these measurements were probably not action potentials from the nerve but induced effects derived from the microphonic action of the cochlea (the 'cochlear response'; pp. 420-423), but at that time the artifacts of electrical induction were not well understood.

The nerve physiologists, especially Forbes and Adrian, all had the problem of sensory conduction clearly in mind; yet they needed the discovery of the thermionic amplification of small potentials before they could make the necessary observations. It was, therefore, not until 1924 that satisfactory records of afferent potentials were obtained—by Forbes, Campbell and Williams who used amplification with a string galvanometer to record action potentials initiated in somatic nerves by muscle receptors. In the next few years, however, research accumulated so rapidly that we must content ourselves here with a simple list of the kinds of sensory potentials recorded or measured.

1924 Forbes, Campbell and Williams	muscle: tension
1926 Adrian	skin, muscle, lungs, heart
1926 Adrian and Zotterman	isolated muscle spindle: stretch
1926 Adrian and Zotterman	skin: light touch and heavy pressure
1926 Adrian	skin: penetration by a needle
1928 Adrian and R. Matthews	eye (eel): light
1929 Adrian and Umrath	Pacinian corpuscle: pressure
1930 Wever and Bray	ear: speech and tone
1931 B.H.C. Matthews	isolated muscle spindle: contraction
1931 Davis and Saul	ear: speech and tone

Perhaps Wever and Bray do not belong in this list as of 1930, for certainly much of the speech and the higher tonal frequencies that they put into the cat's ear and then heard in a loud speaker as potentials amplified from an electrode on the cat's auditory nerve came not from the nerve itself, but, like Buitendijk's, as a spread of biomicrophonic action in the cochlea. Davis and Saul were first to make this correction, and soon, after appropriate controversy, the fact was admitted generally. The discovery of the Wever-Bray effect was, however, important in that it set off a series of investigations in which the action potentials of the auditory nerve became quite as common an object of observation as had been the potentials of motor nerve. This bit of history is discussed elsewhere in this book (pp. 420-423).

We must, however, note here also the fact that Forbes, Miller and O'Connor in 1927 observed in the medulla action potentials synchronous with the clacks of a watchman's rattle or the burr of a card held against a moving toothed wheel—observed them up to frequencies of two hundred per second. Faster sequences their string galvanometer could not record.

2. *All-or-none and refractory phase.* Before 1912 the fact that sensations vary in intensity made no trouble for psychologists or physiologists. A *vis nervosa* would presumably not be fixed in amount: the greater the *vis*, the stronger the sensation. A wave of negativity might have any amplitude. Nor did the discovery of the all-or-none principle for motor fibers suggest at once that there ought not to be continuously graded strengths of sensation if the sensory fiber can give only its all or nothing.

Forbes and Gregg in 1915 were the first to emphasize this difficulty. Obtaining continuously graded reflex responses from a continuously increasing strength of stimulus, they asked how the continuity of the stimulus can be represented in the reflex, if the afferent link in the chain can vary only by discrete steps as each new fiber enters the field to add its all to the total excitation. They answered their own question with the frequency theory, which we must consider presently.

Troland also saw this problem for vision in 1920, when he undertook to show how the facts of physiological optics are not necessarily inconsistent with the application of the all-or-none principle to the optic fibers. This matter was not, however, settled until the researches of Adrian (1926–1929) showed that a continuous stimulation of a sensory organ at a fixed intensity gives rise to a series of impulses which are all of the same magnitude.

If further evidence than Adrian's were needed, the history of theory of hearing furnished it. In 1926 the present author suggested that the all-or-none law might mean that the pitch of tones depends upon the frequency of neural excitations and the loudness upon the number of fibers excited, but Forbes, Miller and O'Connor at once objected (1927) that the refractory period, apparently being no shorter in auditory fibers than in other nerves, would not permit the passage of the higher audible frequencies. They were right. The auditory "volley theory" of Wever and Bray in 1930 presumed the all-or-none law and normal refractoriness, and finally Derbyshire in 1935 got the most direct evidence. He

determined the maximal potential that can be had from the auditory nerve of different frequencies of tonal excitation, finding that the maximal potential obtainable is constant for frequencies below 800 c.p.s., a rate at which the refractory period permits every fiber to respond to every wave of the stimulus. At rates too fast for the refractory period (e.g., 1000–1500 c.p.s.) a fiber can respond only to every other wave and the maximal potential is, therefore, cut in half: only half the fibers respond to every wave. When the rate is more than three times the refractory period (e.g., at 2000 c.p.s.) the maximal potential is cut to one-third. (See Fig. 66, p. 424.)

3. *Sensory intensity.* It has turned out that sensory intensity depends upon the number of afferent fibers excited or upon the frequency of impulses in them or upon both factors working together—that is to say, sensory intensity is a function of the total excitation within the fibers that contribute to the sensation during some limited period of time. Both the successive impulses and the impulses concurrent in different fibers may be summated into a single potential in the apparatus that measures potentials; and they must, moreover, be similarly integrated in the organism whenever the total excitation determines either the strength of a reflex or the observed magnitude of a sensation. The question is: how did we come to accept this duplex theory of sensory intensity?

The theory that intensity of response results from the multiplication of innervated fibers grew up in the investigations of the reflexes. A stronger stimulus excites more nerve fibers, which in turn excite more muscle fibers, resulting in a stronger contraction of the muscle. Stellar brightness, moreover, is measured by stellar magnitude: the brighter stars look brighter (sensory intensity) and larger (more fibers innervated). Strong stimuli are ‘irradiated’; they spread on the skin, in the retina, and, according to both Helmholtz and recent investigators, in the organ of Corti in the inner ear. There was plenty of ground for belief that sensory intensity depends upon multiplicity of fibers innervated. But why did magnitude of sensation seem then to be a continuous function of intensity of stimulus? Why was it not quantal, showing step-wise increase for a continuously increasing stimulus, like the reflexes under certain conditions? (Cf. pp. 38, 59–61.)

Forbes and Gregg found the answer to this question in the frequency theory of intensity. They argued in 1915 that a continu-

ous stimulus of constant strength, having rendered the nerve fibers refractory by discharging them once under the all-or-none principle, would then discharge them again after the refractory period had passed, and that, with the continuation of this cycle, the stimulus would establish a frequency of impulses whose rate would be determined by the duration of the refractory period. A stronger stimulus thus would yield a faster frequency, because then the refractory period, lying within the interval of relative refractoriness, would be less. (Fig. 4, p. 60.) The stronger the stimulus, the shorter the relative refractory period; hence the more rapid the frequency.

This plausible hypothesis was amply confirmed by Adrian's researches (1926-1929). He found that a constant strength of stimulus always gives rise to a series of impulses, whose frequency increases with the strength of the stimulation. He also found that the frequency for a constant stimulus diminishes rapidly as the stimulation persists, and he explained this phenomenon as evidence of adaptation in the sense-organ: the receptors in use rapidly lose their effectiveness; thus the resultant frequency diminishes too.

All told the 1920's were a profitable decade for increase of knowledge about afferent conduction. In 1920 there was really little more than a belief in parsimony: nature, it seemed, would not make sensory nerves different from motor, nor give them different refractory periods. The biochemistry of the two kinds of nerve, moreover, appeared to be the same. In 1930 psychophysiology had taken a long step toward Fechner's goal, the substitution of 'inner' for 'outer' psychophysics. Scientific knowledge of sensory stimulation has always thus progressed from without in—we have learned first of the nature of the external stimulus; next have come fact and theory about the processes in the sense-organ; now we know something about afferent conduction.

Specific Nerve Energies

In order that we may understand the thought which lies beneath Johannes Müller's doctrine of specific nerve energies, let us try to comprehend the epistemological principle involved. Not all the men who discussed this problem understood it.

The classical view of the mind is that there exists within the brain a sentient being, a Sensorium, that seeks knowledge of the external world and can never come closer to it than the direct contact provided by the nerves. Suppose I was permanently imprisoned and wanted to know what the Venus de Milo looks like. How could I find out? (a) I might have the statue itself brought to me for inspection. (b) If that were impossible, I could have copies, pictures, images, eidola of it brought to me. (c) Failing them, I could acquire a verbal description of it. The first plan is impossible for the mind: it cannot get at the objects themselves. Thus the ancients chose the second principle: the objects give off images of themselves, which are carried to the mind by the nerves (pp. 4 f.). In other words, representation of the objects to the mind is *in kind*. If, however, there are no copies of the objects or if the nerves cannot carry them, it is still possible to have a symbolic representation in which the data furnished the mind do not resemble the object—any more than the words of a description themselves resemble the object which they describe. Such symbols must have to the object a *fixed functional relation*, if from them the mind can learn to infer the nature of the object to which they refer or are related. The specific nerve energies are just such symbolic data. Having a fixed relation to their adequate stimuli, they imply the stimuli, thus providing information about them. The distinction here lies between direct representation and functional implication.

Put in this manner there is not enough difference between the two theories for them always to remain distinct. It is no wonder, then, that the representative theory (eidola) could persist so long that the sophisticated supporters of the theory of functional adequacy had repeatedly, throughout two centuries, to win the battle against it over and over again.

For instance, John Locke's theory of the secondary qualities (1690) was a clear enough argument against the image theory. The objects have "powers" to produce these qualities, yet the powers in the objects in no way resemble the secondary qualities in the mind. The power of an object to produce the quality red does not, he said, look red. A tone is not a vibration, even though there never was a tone without a vibration to cause it. Always some reliable causal connection intervenes, changing the nature of the

event and making sure that a tonal sensation implies, when knowledge is adequate, the prior vibration without the tone's ever being itself a copy of a vibration.

Thus it comes about that the functional relations which make correct perception possible lie, in part, in the properties of the nerves. The mind, hearing a sound, is perceiving that the auditory nerve is excited and thus knows that some object, capable of exciting that nerve, is without. Not all objects are sonorous. To hear a sound is to perceive the presence of such an object as has the power of stimulating the ear.

This logic was quite clear to the thoughtful men at the beginning of the nineteenth century who reasoned about these matters, although the image theory of perception still pervaded common sense. Thomas Young, convinced in 1801 that there is an infinitude of perceivable colors and that they can all be composed of mixtures of three primaries, suggested that there must, therefore, be three kinds of optic nerve fibers (pp. 110 f.). A sensation of purple would mean that the red and blue nerves were excited and the yellow not at all; a white that all three nerves were equally excited. He did not picture the Mind in the brain, weighing the relative amounts of the three excitations; yet that was the implication of his thought.

Charles Bell in 1811 anticipated Johannes Müller's theory in considerable detail, pointing out that one perceives sensory quality according to the nerve that is excited, not according to the object that excites. If a pressure on the eye-ball stimulates the retina, light is perceived, not pressure. Electricity may excite many senses, yet we never perceive electricity as such, but as light or taste or touch, depending upon what nerve is excited by it. Bell's argument was brief but explicit; it is possible to cite from him sentences that explicate every one of Müller's more important laws. Bell, however, enunciated no formal doctrine. It was, nevertheless, obvious to him "that neither bodies nor the images of bodies enter the brain."

The image theory was, however, so well grounded in common thought that Johannes Müller was constrained to combat it with his formal theory of "specific sense energies" (1826), or "specific energies of nerves" (1838). He laid down ten laws with elaborate illustration and discussion, but we must content ourselves with a

simple restatement of the five most important principles which his laws include. These are they.

(1) The Sensorium is directly aware of the states of the sensory nerves, not of the external objects. How could it perceive objects directly, when only the energies of the nerves come to the brain?

(2) There are five of these specific energies, one for each of the five senses. Müller did not go even so far as Young who thought that there might be three different kinds of optic nerve fibers. When he called the nervous essential an *energy*, he must have been influenced by such doctrines as the *vis viva* and the *vis nervosa* in his choice; yet he remarked in one place that *quality* would do as an alternative term for energy.

(3) The same stimulus acting on different nerves gives rise to different qualities; different stimuli acting on the same nerve give rise to the same quality. It is the nerve, not the stimulating object, that matters. Müller's text is replete with instances too well known to need repetition here. In 1838, it was possible to argue that electricity is a stimulus to all five of the senses, arousing in each its own peculiar quality.

(4) The Sensorium gains correct information about external objects, because, in general, only particular objects excite particular nerves. This is the doctrine of what Sherrington later called the *adequate stimulus*. Haller actually had the idea in 1752, when he suggested that the seeming insensitivity of the internal organs is due to the fact that their manipulation is an inappropriate or unnatural stimulus. Müller spoke of the *specific irritability* of the organs of sense and of the "adaptation" of the stimulus to the sense-organ. He also said that the sense-organ and its proper stimulus are "homogeneous"—a term which seems, however, to imply the similarity implicit in the old image theory which Müller was fighting. Hering, named the effective thermal stimulus the "adequate temperature" in 1880, leading Sherrington to attempt a definition of the "adequate stimulus" to thermal sensation in 1900. In 1906 Sherrington gave this term *adequate* formal status: "the main function of a receptor is therefore to lower the threshold of excitability of the [reflex] arc for one kind of stimulus, and to heighten it for all others." Later others began to speak, somewhat ambiguously, of "inadequate stimuli," meaning stimuli which, being only adventitiously adequate, were unnatural or inappropriate, like a pres-

sure on the eye in arousing color, or heat on a cold spot arousing cold paradoxically. The principle was clear in Müller, though the words have changed.

Müller had, of course, to stress the specific irritability of sense-organs in order to show why perception is usually reliable. The old theory had explained this reliability by the similarity of the image to its object. Müller substituted functional adequacy. The specific energy of the optic nerve means light to the Sensorium because that nerve is specifically irritable by light. It is the exceptions that prove this rule: a less well-adapted stimulus nevertheless produces the quality proper to the nerve *if* it succeeds in exciting the nerve at all.

(5) Finally, Müller held that the locus of the specificity lies either in the central portion of the nerve or in its termination in the brain. It cannot lie in the peripheral portion, because direct stimulation of the nerve beyond the sense-organ gives rise to its peculiar quality. Müller's critics from Lotze, who was one of the first, all the way down into the twentieth century have forgotten this proviso. They have argued that sensory nerves are all alike, that there is, therefore, no evidence for a difference in their energies, forgetting that sensory nerves, even though their impulses may be identical, are all different inasmuch as they end in different places. It was much too early for Müller to have espoused the doctrine of sensory centers, and only natural for him to speculate about specific sensory *vires nervosae*. He left open, nevertheless, this possibility that the specificity is, after all, the place where the nerve ends, or, as we think now, the connections that it makes there.

Of Müller's doctrine there was criticism aplenty, criticism which turned mostly upon the point that the nerves are passive conductors and all alike. Müller's choice of a name for his theory was responsible for this objection. It was not likely that the sophisticates would object to his underlying principle, which, as we have seen, they accepted even before he built it into a formal doctrine. Nor did they want to go back to the image theory. Instead they accepted the main thesis and extended its use.

They extended it to the qualities within each sense-department. As we have just observed, Thomas Young had already made that application when he suggested three kinds of nerves for the three primary colors. Natanson in 1844 took up the question formally, arguing that every neural organ must have a function and

every function an organ. He thought there might be three specific energies for touch, three for taste, three (of course) for vision, and an indeterminate number for smell. He made no proposal about hearing, for the tonal series does not appear to be a mixture of a few primaries, and he lacked Helmholtz's courage to suggest several thousand specific auditory energies. A. W. Volkmann, later in the same year, criticized Müller—somewhat paradoxically—on the ground that the theory would require, not merely five specific energies, but one for every sense-quality, and then went on to say that surely every nerve must have its specific excitability, that there must, therefore, be separate nerves for pressure, temperature, tickle, every color, every taste. In a way he was right in criticizing Müller: specific energy implies a difference in content of the nerves, whereas specific excitability implies only a different functional relation. Volkmann was accepting what Müller called *specific irritability*, not specific 'energy.'

Helmholtz seems to have taken this extension of the theory for granted (p. 199). He praised Müller, said that the theory was for psychology as important as the theory of conservation of energy for physics. Indeed, it was the only theory of sensory quality that there had been—or that there is now, if one thinks only of the basic principle. In 1852, when he first adopted Thomas Young's theory of color, Helmholtz posited three specific visual energies. In 1863, when he first put forth his famous theory of hearing, he assumed the existence of enough thousands of specific energies to account for all the discriminably different tones. One can see here why Müller was both criticized and accepted. To posit a thousand different energies for a single sense seemed extravagant, if theories should be parsimonious and if the waves of negativity in the nerves appeared all to be alike; yet to posit a thousand specific irritabilities was hardly to do more than to describe Ohm's law of tonal analysis. A thousand fibers, must, moreover, have a thousand different terminations. The distinctions here are slight. Lotze and Volkmann saw a difference; Müller and Helmholtz did not. Helmholtz did not even show that he knew he was extending Müller's theory until he added a paragraph about it in the posthumous second edition of his *Optik* in 1896.

The effect of the theory was tremendous. Thus extended, it indicated that there must be spatial differentiation of qualities in the sense-organ, that theories of sensation must show how ele-

mentary sense-qualities are got onto different afferent nerve fibers. Hering's theory of color vision thus required six specific energies, one each for black, white, blue, yellow, green and red (pp. 208 f.). The theory of specific energies lay back of the search for the final elementary qualities of taste (pp. 450, 453 f.). It led directly to the search for spatial differentiation of the tactual qualities and the discovery of the warm, cold, pressure and pain spots (pp. 467-469). Even where the theory could not be applied, its terms were used: a chapter on smell would end with a discussion of the probable number of olfactory specific energies.

The theory also led, in connection with other discoveries, to the localization of sensory centers in the brain. Müller had left open the matter as to whether the specificity might not lie in the terminations of the nerves in the brain. After 1870 the advances in the knowledge of brain localizations made it appear that the essential condition of sensory qualities might consist of the existence of specific sensory centers—at least for the five senses, since no one ever proposed the separate existence of red, green and violet centers. It is not possible, however, to enter here into an adequate account of the history of brain localization, and indeed the localization of sensory centers has thus far helped psychology very little. We may, nevertheless, sketch the important events.

It was, of course, no new idea of the nineteenth century that every bodily organ should have a function and every mental faculty a bodily seat. Anatomy had inventoried the organs, and faculty psychology—notably Thomas Reid and the Scottish School—the faculties. About 1800, it was, that Gall, presently assisted by Spurzheim, created phrenology by trying to find seats for the faculties. Gall was an excellent brain anatomist. His *Anatomie et physiologie du système nerveux en générale, et du cerveau en particulier, avec observations sur la possibilité de reconnaître plusieurs dispositions intellectuelles et morales de l'homme et des animaux par la configuration de leurs têtes* appeared in 1810-1819. The public liked this doctrine that the faculties and propensities of men should show in 'bumps' on the surfaces of their skulls, but the Church got the government to stop Gall's lectures in Vienna in 1802 and, when he shifted the scene of his activities to Paris, the Académie des Sciences—in a report signed by Cuvier but instigated perhaps by Napoleon—refused election to Gall.

Flourens, a protégé of Cuvier's, was a pioneer in the method of

ablation for the study of the functions of the brain. (See also pp. 536 f. for his work on the semicircular canals.) He interpreted his experiments, published in 1824-1825, as signifying that localization is limited. Every part of the brain has its specific function yet acts as a whole in respect of this function, just as the total brain also acts as a whole. All the perceptions, for instance, occupy, he asserted, the same seat in the brain concurrently. We shall see presently, in connection with the work of others, how he could come to such a conclusion. Flourens fought phrenology, and the esteem in which scientists held him, added to the disesteem they had for Gall, helped to delay for a long time any assured belief in exact localization of cerebral function.

In 1861 Broca, though anticipated by two lesser men, discovered the speech center. At any rate, he discovered degeneration of tissue in the third frontal convolution of the left cerebral hemisphere of a patient who for thirty years had been unable to speak, although his vocimotor organs were normal. When Broca was able to supplement this case with others, the pendulum began to swing away from Flourens, although hardly toward Gall.

Then, in 1870, Fritsch and Hitzig found that with weak electrical stimulation of a limited region of the cerebral cortex they could get specific movements of different muscle groups—five regions for five groups. Strong current spread to produce convulsions, but weak determined the position of motor centers. No other part of the cortex produced, with electrical stimulation, any movement at all. This experiment started physiologists off on a vigorous search for motor and sensory centers, a search which has not stopped yet.

Just as it had been more difficult to find out about sensory conduction than about motor, so now it was harder to locate the sensory centers than the motor. Vivisection and good introspection cannot often be combined. There were presently many investigators, but the pioneers in this search were Ferrier, Munk and Goltz, whose papers date from 1876 on. Ferrier's and Munk's localizations did not always agree, whereas Goltz, remaining closer to Flourens, held that specificity of function is not so exactly localized.

The position of the visual center was the most definitely fixed. Ferrier found that monkeys, with an occipital lobe removed, were subject to abnormal eye-movements and seemed to be blind in the

eye on the side opposite to the side of the ablation. Munk showed that the removal of an occipital lobe does not entirely blind either eye, but produces hemianopia, a blindness of half the field of vision in each eye. Goltz doubted these facts, but Munk was right. Hearing they localized in the temporal lobe, though less certainly. The somesthetic sensations were placed in the post-central region of the cortex. About the centers for taste and smell there was considerable uncertainty. It was altogether a confusing period with much outspoken disagreement.

From the point of view of the present we can see how there had to be disagreement. Here are five reasons why reliable localizations were—and are—hard to secure. (a) The stimulation, as Hitzig had noted, tends to spread. Luciani's maps in the early days showed huge centers—with the midregions, nevertheless, most effective. (b) Specific localizations tend to be instable. Other conditions may shift them temporarily. (c) Localization becomes more exact and shifts toward the higher neural levels (encephalization) in ascent in the evolutionary scale. Hence it is difficult to argue from one animal to another, or from lower vertebrates to man. (d) Vicarious function or equipotentiality is common. It is surprising how much brain tissue the animal can get along without. (e) Restitution of function after ablation is also common, indicating that the initial effects are not final. Some of the initial effects may be due to operational shock and not to the loss of essential neural tissue. These facts of restitution of function after operation were recognized from the first, but some of the other factors have been realized only gradually. It is no wonder, then, that there was disagreement in the early experiments.

The present century began with a swing back toward Flourens and Goltz. Franz (1902 *et seq.*) and Lashley, Franz's student (1917 *et seq.*), showed how variable, how temporary, how inexact localization of cerebral function may be. In 1929 Lashley summarized two decades of his and Franz's work under the principles of equipotentiality and mass action. *Equipotentiality* means that any part of the cortex is as good as any other in contributing to certain functions, like 'intelligent' action. *Mass action* means that all equipotential parts contribute together and that the loss of any part diminishes total efficiency proportionately to the loss, no matter what part is destroyed. Hunter, however, in 1930 suggested that equipotentiality may be merely vicarious function,

that 'intelligent' activity, at least, may be possible by a great variety of means, and that the removal of one means leaves the others, and thus the function, intact. Lashley, on the other hand, never denied some specificity. Pattern vision in the rat he found to depend absolutely on the preservation of a particular occipital region, a region specific for the visual perception of shape, yet equipotential with others in its contribution to 'intelligence.' In the rat, moreover, it turned out that discrimination of brightness does not require the cortex at all—a finding which cannot be extended, however, to man.

Certainly it will be a long time before the final word on the cerebral localization of sensory function has been said. We know as the results of many experimental ablations and clinical observations that there is a visual cortex in the occipital lobes, essential to any kind of vision in man, that there is an auditory region in the temporal lobes, that the post-central portion of the parietal lobes plays an important, if not essential, role in somesthetic perception, that smell involves the olfactory projection. Of Johannes Müller's two hypotheses—the one that the specificity might lie in the central portion of the nerve, the other that it might be localized at the nerve's termination in the brain—the latter is more nearly correct.

The nerves themselves are now known to be passive conductors in which all impulses are alike—or nearly alike. Thus far only one difference has been found among afferent impulses. Gasser and Erlanger (1924-1930) discovered that somatic nerves may have at least three kinds of fibers, fibers of different sizes in which the impulse travels fastest in the largest, slowest in the smallest. Adrian (1931) added to this distinction the argument that the slow, small fibers, for the most part, mediate pain. This discovery came, however, long after it had been seen that the important kind of specificity lies not in the nature of the impulse, after all, but in the locus of the terminus and in the connections that a fiber makes in the central nervous system. Müller could have imagined the Sensorium recognizing a stimulus as painful because the impulse arrived slowly on a small nerve. Today we ask merely: what subsequent neural events does such an impulse set up?

Thus Johannes Müller's second hypothesis is going out of date about twenty years after his first. Specificity seems today to lie, not in the nerves, nor even in their terminations, but in the total re-

active pattern. Centers are already a little passé. The positivists claim that sensation *is*, so far as it is knowable in introspection, discrimination or differential response. A light differs from a sound, they say, because it is possible for the organism to discriminate the one from the other, to react in one way to light and in another to sound. This statement means that the reacting system 'knows' the difference between impulses that have come along the optic nerve and those that have entered by way of the auditory nerve. Müller's Sensorium is reaction—for the positivist. The phenomenologist can still get along, it is true, with centers whose activation is the physiological substrate of the sensation; but the positivist needs, not centers, but tracts. The impulses, not being caught in a cerebral pool, are but photographed as they pass. The positivist, therefore, gets some comfort out of the difficulty of localizing centers, because what he needs are routes, not stations. A route cannot be marked on a map with a push-pin. It can, indeed, be destroyed by being cut at some particular place, but then in many cases there turn out to be alternative routes; ablation does not, then, completely block the traffic but only hinders it by requiring a detour. (But *vide infra*, pp. 78-83.)

Projection

The receptor-fields of the sense-organs are 'projected' upon the central nervous system in the sense that the afferent fibers lead to the central system. Indirectly by way of synaptic connections in nuclei, the tracts of all five senses establish in man connection with the cerebral cortex, although they also make other connections at subcortical 'reflex' levels which do not involve the cortex. Thus neural anatomy has come to support Johannes Müller's theory of specific nerve energies, which eventually became, as we have just seen, a projection theory of sensory quality. Sight is not hearing because the optic fibers are projected upon the occipital lobes and the auditory upon the temporal lobes. If you could cross-connect the optic and auditory nerves, you could, du Bois-Reymond imagined, see tones and hear colors.

We have seen how easy it was for Helmholtz and the others to extend Müller's theory of specificities from the differentiation of the five senses to the differentiation of the qualities within each sense. In general, the theorists accepted a principle of parsimony.

They did not suppose that there are 300,000 specific energies for colors, more or less, but, by appealing to Newton's law of color mixture, assumed that there may be three specific visual energies, capable of 300,000 specific combinations. Taste and cutaneous sensibility were got down to four primary qualities each, and smell, it now seems, might perhaps have as few as six. A thousand energies for hearing constitute simply the unavoidable extravagant exception to the rule of parsimony. We might expect, therefore, that the extension of Müller's doctrine to qualities would have led to an extension of the parallel theory for the brain. The sense-physiologist might have sought cortical centers for red, green, violet, sweet, sour, salt, bitter, warm, cold, pressure, pain, fragrant, ethereal, resinous, spicy, putrid and burnt—eighteen centers plus some more for hearing. They did not. Why?

They did not seek to establish a projection theory for these eighteen and more primary qualities because the qualities belonged, at least in the cases of vision and touch, to spatial senses, for which projection had already been preempted as the theory of extension, of the *Ortsinn*. It is strange that there has been so little recognition of this potential conflict that arises from the use of the principle of projection to account for qualitative difference in the large and extensive difference in the small.

In vision spatial differentiation in projection had long been recognized as the basis of spatial perception. The anatomy of the optic chiasma was known even to Galen (ca. 175 A.D.) who explained the singleness of binocular vision by assuming that some of the optic fibers from each eye cross at the chiasma and join corresponding fibers from the other eye. The discovery of the horopter (the locus in space of points seen singly in binocular vision) by Aguilonius in 1613 certainly supported some such view, and Newton in 1717 assumed that half the fibers cross at the chiasma to join the corresponding fibers from the other eye either at the chiasma or at the brain. Wollaston in 1824 observed hemianopia in himself; half the field of his vision disappeared when he was greatly fatigued, indicating that Newton's notion of visual projection was correct, that the fibers from the left halves of both retinas lead to the left half of the brain, and conversely. Thus, when Müller came to the problem of vision in 1826, he had little choice about the matter. His nativism, furthermore, also led him to assume that spatial difference on the retina must mean spatial

difference in the Sensorium. It is true that he might have envisaged three superimposed systems of fibers, such as Thomas Young must have had in mind, but Müller did not. The weight of the *Zeitgeist* was against him. (See pp. 234-236.)

The projection of optic fibers was also implicit in the concept of the retinal image. Kepler, arguing in 1804 that the crystalline lens is not the visual percipient organ but a true lens which focuses an inverted image on the retina, was furthering visual theory by getting the image into the eye, onto the retina and thus one step nearer the Sensorium. That theory would have been no advance had it not also been supposed that the spatial relations would be preserved by projection upon the Sensorium. Thus visual projection was already good doctrine in the early nineteenth century, even though Flourens' ideas were against it. (See pp. 226-228.)

After Müller had first put forth the theory of specific sense energies, Weber entered the field with his proposals about the *Ortsinn* in 1834 and 1846. He had invented the compass test to measure the capacity of a subject for the discrimination of two adjacent simultaneous touches. He pictured the skin as divided up into "sensory circles," such that the stimulation of two circles next but one would give rise to the perception of duality. That conception implied projection for the skin. In 1852 Weber even suggested that the circles may not be circular, that on the forearm they would be elliptical because there discrimination is finer in the transverse direction than it is parallel to the axis of the arm. (See pp. 475-477; also Fig. 79.)

It was in 1871 that Julius Bernstein, having especial regard to the theory of the sensory circles, took all these assumptions and implications and made them into an explicit projection theory. His figure, often reproduced in modified form, is shown in Fig. 6. There *aa* is some sensory surface, like the skin or the retina. It is projected by the nerve fibers upon the brain centers at the line *gg* (ganglion cells). If the point at *e* and the cell at *c* are stimulated, the intensity of excitation is measured in the diagram by the height of the ordinate *cn*. This excitation is, however, "irradiated"; it spreads from the center *c*, diminishing in intensity with the remoteness of the irradiation, as shown by the curve of excitation. Finally, it is reduced to a threshold value c_1m_1 or c_2m_2 , beyond which it is not effective. The radius $cc_1 = cc_2$ is the radius of the "irradiation circle," a circle of dispersion which in the brain re-

sembles in function and nature the "sensory circles" at the periphery. Bernstein also drew the diagrams for dual stimulation: the case where two stimulations close together summate, as they are known to do in vision and were later found to do in touch,

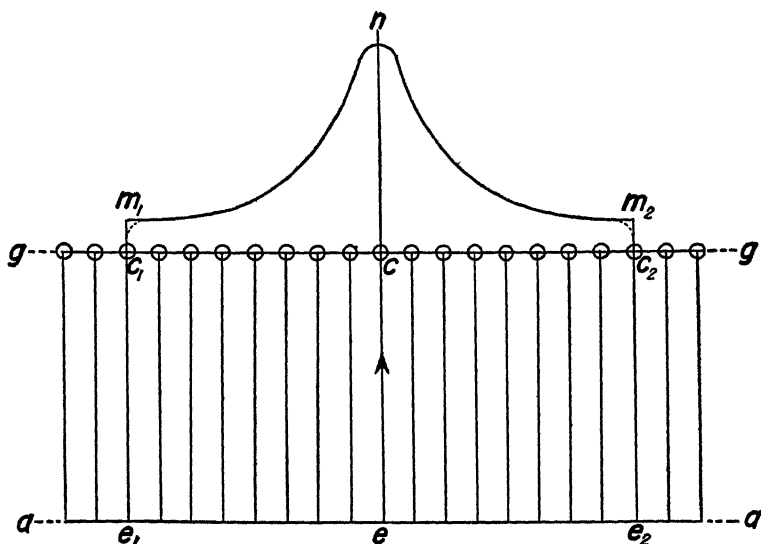


FIG. 6. PROJECTION AND IRRADIATION: BERNSTEIN (1871)

Stimulation of e excites the ganglion-cell c , producing excitation of intensity cn . The curve shows the intensity of excitation irradiated about c as far as c_1 and c_2 , where the excitation falls below the threshold value $= c_1m_1 = c_2m_2$. Radius of irradiation circle $= cc_1 = cc_2$. Projection is shown by the point-to-point correspondence of peripheral field aa , with the ganglion field gg , e being projected upon c , e_1 upon c_1 , e_2 upon c_2 .

and the case where the stimulations, being somewhat more remote from each other, produce a total excitation which has two modes with a 'saddle' between them. This latter situation fits the 'dumb-bell' case that Weber and others after him found as intermediate between the perception of one tactual impression and two separated impressions. Altogether the theory was convincing in 1871 and remains plausible today after seventy years.

It cannot be said, in fact, that we have advanced much beyond Bernstein. It is no longer necessary to assume sensory circles at the periphery or irradiation at the center, for it is clear that all stimulation spreads in the peripheral organ, much or little accord-

ing to its degree. In vision the spread is partly optical dispersion and partly retinal and neural spread: the brightest stars have the greatest magnitudes. On the skin, the dispersion comes about by way of pressure gradients or thermal gradients. In the inner ear, a loud tone affects more of the organ of Corti than does a weak tone. Thus central irradiation may be given up; but projection stands.

Projection is, of course, in this sense one-to-one. The receptor field and the cortical field are held to be isomorphic, that is to say, the spatial orders at the periphery are supposed to be reconstituted topologically in the brain—not the exact shapes, but the orders. (On the nature of isomorphism, see the next section.)

Isomorphic projection is implicit in the notion that the singleness of binocular vision arises from the community of projection of identical points of the two retinas (pp. 226 f.). It lies back of any attempt to explain stereoscopic vision in terms of small retinal disparities (pp. 285 f.). It gives meaning to the fact that injury to the visual cortex produces a scotoma (blind area) in the field of vision, and that larger lesions produce larger scotomata. If it can ever be shown that the shape of the lesion has a definite relation to the shape of the scotoma, then the theory of visual projection would be finally established.

Projection also furnishes a satisfactory explanation of the facts of the cutaneous two-point limen (as Bernstein showed) and of the error of localization, and it makes these two measurements consistent with each other (pp. 484 f.). It provides a sensible meaning for the abnormal errors of tactual localization which occur after injury to a cutaneous nerve (p. 475). It explains also the fact—so obvious as almost never to be mentioned—that large errors of localization are less frequent than small—for, if projection did not approximately maintain the spatial order, why should not two near points at the periphery often be more readily discriminated than two remote points, since, unless projection is isomorphic, remote points at the periphery might be projected upon adjacent points in the brain?

In hearing, as the place-resonance theory becomes better established (at least for high tones), it would seem that position in the sense-organ is correlated with perceived pitch. Are the pitches projected? Culler has presented direct evidence of their projection at an immediately subcortical level, and there is considerable

indirect evidence. Why else does one hear a single pitch binaurally when the two ears show a difference of pitch in monaural stimulation? There must be community of binaural projection to explain that fact. Why else does the localization of a binaural tone shift when the intensive difference or the phase difference between the two ears is changed (pp. 387-391)? Why does one hear a beating intermediate pitch (the intertone) between the pitches proper to two low tones which are adjacent in pitch and generate the beats? The fibers from each ear pass to both cerebral hemispheres and it looks as if the spatial orders in the organ of Corti were preserved in the brain.

Some degree of isomorphic projection is also required by Köhler's theory of the isomorphic relation between perception and the patterns of excitation in the brain (see the next section). If the pattern of perception, being, in general, correct in spite of all the exceptions which Gestalt psychology has exhibited, resembles the pattern of stimulation, and if, as Köhler's theory asserts, it also resembles the brain pattern, why then the pattern in the brain must also resemble the pattern of stimulation.

Brain anatomy—as far as it has gone, and recently it has gone forward rapidly—supports psychology in most of these matters. The sensory tracts are well established. Point-to-point correspondence is accepted for vision, but exact correlation is somewhat disturbed by the fact that axons in the cortex arborize at their terminations. The problem here is no greater, however, than the problem created by the spread of excitation in the inner ear where exact localization is nevertheless taken to be effective (pp. 408 f., 430 f.). Perhaps a mechanism will be found by which the spatial relations of the receptor-field are precisely maintained by interconnections or field-forces, even though the correlation of a particular receptor-point with a particular cortical point is not permanently fixed. A spatial neural transposition, analogous to the transposition of pitches in a melody, might be possible, with the peripheral relations maintained in the brain even when all the exact connections had been altered.

Isomorphism

One system is said to be isomorphic with another in respect of their spatial relations if every point in the one corresponds to a point in the other and the topological relations or spatial orders

of the points are the same in the two. If a system of points is marked on a flat rubber membrane and the membrane is then stretched tightly over some irregular surface, then the points in the stretched membrane are isomorphic with the points in the flat membrane. As we have just noted, perception and stimulus are spatially isomorphic in as far as the perceived spatial orders correspond with the spatial orders in the stimulus. Projection of the stimulus field upon the cortex tends to be isomorphic, at least to the degree which we have discussed in the preceding section. If perception and brain field are both isomorphic with the stimulus field, they must be isomorphic with each other. It is to this solution of the mind-body problem that Köhler has applied the term *isomorphism*—meaning psychoneural isomorphism. The simplest test of such isomorphism is to see whether adjacencies and inbetweennesses are preserved from one system to the other.

This word has sometimes been extended to other sensory attributes than space. There would be temporal psychoneural isomorphism if the time-order of perceived events is the same as the time-order of the neural events underlying them. Intensive isomorphism would mean that sensory intensity always corresponds with degree of the total underlying excitation. Qualitative isomorphism, at which Köhler has hinted, implies that difference in sensory quality implies difference in excitatory quality, as if different kinds of ion-concentrations in the brain could explain the difference between yellow and blue or between sweet and sour—a rather improbable assumption in view of the uniformity of nervous action.

Psychoneural isomorphism, however, is a special case of psychophysical parallelism and of the mind-body problem in general. It was believed to be axiomatic long before the anatomical and physiological knowledge of projection was sufficient to justify it. How did psychologists come to hold to this view? Why did it seem axiomatic to them?

In a way this whole matter goes back to Empedocles, to the image theory of perception and to representation in kind (pp. 4 f.). If the nervous system were to pass along to the Mind an eidolon of the object, then the Mind in the brain could perceive directly the nature of the object in as far as the eidolon is a correct copy. Locke's theory of secondary qualities, Müller's theory of specific

nerve energies, substituted functional implication for this representation in kind, yet left the Mind to perceive directly the state of the brain. Even for Müller's objectivity the Sensorium was an intracranial homunculus with discriminatory capacities.

That there should be difficulty about the way in which the immaterial mind perceives material states is the reason for *dualism* of mind and body. We need not go back to the origins of the dualistic conception in Plato, but content ourselves with noting that Descartes (ca. 1650) made the belief in an insubstantial mind axiomatic in psychology. The consequent easy view about the relation of mind to body, the view that results from such a dualism, is *interactionism*: mind affects body and body affects mind. The instances of both these relations are innumerable, and the theory remains irrefutable as long as there is no exact observational knowledge about the functioning of the brain. Descartes was an interactionist; so was William James as late as 1890, when he refused to accept the automaton theory of human conduct and appealed to common sense for the evidence of interaction.

Parallelism, however, entered the field against interactionism. This theory begins with Leibnitz and his isolated, non-communicating monads. The orderliness of nature in such an individualistic universe comes about, Leibnitz thought, by way of preestablished harmonies among the developing monads. If nothing interacts, then, of course, mind and body would not interact. One would appear to depend on the other only because the preordained events in each would harmoniously coincide—like two clocks which tell the same time without either being able to affect the other.

The first thorough theory of psychophysical (really psychophysiological) parallelism was Hartley's (1749). He seems to have come to it by trying to combine Newton with Locke—Newton's theory that the transmission of physical impulses is vibratory (Newton did not always hold to the corpuscular theory of light) and Locke's theory of association. At any rate Hartley described mind as formed by associations of ideas which parallel associations of diminutive neural vibrations in the brain.

For a century after Hartley parallelism and interactionism were equally plausible views. Theologically minded psychologists might prefer interactionism as leaving more scope for the operation of a free will. Scientifically minded psychologists might prefer paral-

lelism as leaving the paradoxically insubstantial mind-stuff off at one side in the picture of self-sufficient physical action in the brain.

In the middle of the nineteenth century, however, the theory of the conservation of energy came of age. Notions about equivalences of work go all the way back to Archimedes and the lever. Newton is said to have glimpsed the general theory of conservation for mechanical action. Joule's memoir, summarizing the history of the theory and bringing together his own contributions on the mechanical equivalent of heat, was written, however, only in 1849. Then, in 1851, William Thomson, later Lord Kelvin, formulated the law of the conservation of energy and also the second law of thermodynamics (the law of entropy). For at least two centuries men had been scheming to devise instruments for the production of perpetual motion. These laws put an end to that dream. Although the law of the conservation of energy did, in fact, assert that all motion is perpetual, the law of entropy denied its practicability, showing that the transformation of energy is always directed toward a dissipation in heat and that the universe ought, therefore, eventually to 'run down,' becoming an inactive constellation of matter all at the same temperature.

These laws, which transformed physical thought, also affected psychology. Because of them it became impossible to think of the transmission of activity without a transfer of energy. Hume had defined *cause* as recurrent concomitance. Now it was necessary to find an energetic equivalence between a cause and its effect, or else, if there is a heat loss, to find less, not more, energy in the effect than there was in the cause. How was the interactionist to measure the energy in the mind, when the body effects a change in it or when it moves the body? It was clear, moreover, that the brain, being material, must be regarded as a closed system: whatever energy is delivered to it must be given up again in motion or lost in heat. There seemed, indeed, to be no way of breaking such a system open to introduce within it mental links.

Thus the theory of the conservation of energy told for parallelism and against interactionism. The action of the nervous system must be complete in itself, explicable, when all the facts are known, under the energetic laws. Mental events might parallel its action but could not cause it or be caused by it. By 1872 this conception was so well established that Bain could base his *Mind and Body*

upon it, and two years later add a chapter on the matter to Balfour Stewart's *The Conservation of Energy*.

It was natural, then, for the hard-headed, physicalistic, physiological psychologists to accept the limitations of the conservation of energy and to become parallelists. Wundt was a parallelist, as were G. E. Müller, Külpe, and practically all of the pioneers in the 'new' psychology which sought to be scientific and experimental. Fechner was also a parallelist, but for a different reason. He wanted to show that the spiritual and material are not different worlds, but simply different aspects of a single world. The *Nachtansicht* shows matter, the *Tagesansicht* mind. Thus Fechner metaphysically was a monist, but epistemologically he was a dualist. Parallelism could be accepted by all these philosophically sophisticated German psychologists because it did not have to prejudice their metaphysics. They could believe in a parallelism either of two substances (metaphysical dualism) or of two aspects of the same substance (metaphysical monism). Nowadays the Gestalt psychologists assert only an epistemological dualism, letting the metaphysical problem rest for separate consideration.

That, then, is the atmosphere of conviction in which the explicit axioms of isomorphism developed. How did they become established?

In 1852 Lotze recognized and discussed this problem. He noted that sensory series correspond to stimulus series, although there is no exact proportionality between. He discussed what were to become later the four basic attributes: quality, intensity, extension and duration. He laid down certain general principles about mixture or simultaneous stimulation. Two stimuli of the same quality acting on the same nerves summate, he said, yielding a stronger sensation. Two stimuli of different qualities may or may not arouse an intermediate quality; colors usually do (red and yellow, though not blue and yellow), tones do not. These differences must depend upon the different ways in which stimuli affect nerves. Qualities from different senses, however, never yield an intermediate quality, he added, but only a division of attention.

In the next year (1853) Grassmann, a mathematician, solved the problem of color complementaries by an application of *a priori* logic. Newton had been unable to get perfect grays from the mixture of only two colors. Helmholtz had succeeded only with blue

and yellow. For most pairs of approximately complementary colors they got grays tinged with some hue. Grassmann laid down the axiom that a sensory series is correlated with a stimulus series and that change in one must indicate a change in the other. Thus he could show that, for every color, there must, within the series of nearly complementary colors, lie an exact complementary which, mixed with the original color in the right proportions would give a perfect gray. Helmholtz accepted this logic and presently, with improved apparatus, verified it experimentally. (See pp. 141 f.)

The effect of Fechner's *Psychophysik* in 1860 was further to establish this correlation of sensory continuities with stimulus continuities. Psychophysics, as a matter of fact, is little other than the determination of critical points in the scales of stimulus as the conditions of critical points in the scales of sensation. Fechner's aspirations, however, went further, for he argued that the laws of outer psychophysics (sensation and stimulus) should be transferred to inner psychophysics (sensation and brain process).

Then Mach wrote in 1865: "To every psychical there corresponds a physical, and conversely. Like psychical processes correspond to like physical, unlike to unlike. If a psychical process can be resolved, in a purely psychological manner, into a multiplicity of qualities, a, b, c , then to these there correspond an equal number of different physical processes, α, β, γ . Particulars of the physical correspond to all the particulars of the psychic." Similarly Hering, formulating his theory of color vision in 1878 and returning to Grassmann's conception of serial relations, wrote: "To two qualities of sensation, which we designate as white or bright and as black or dark, there correspond two different qualities of chemical events in the visual substance; and, to the different relations of distinctness or intensity with which these two sensations appear in the unitary transition between pure white and pure black, or to the relations in which they appear to be mixed, there correspond different relations of the intensities of the two psychophysical processes." (See also the further discussion of this problem, pp. 206-209.)

It is clear that psychologists were confronted with certain principles inherent in psychophysical parallelism, principles which were axiomatic in that they commanded universal assent. No direct proof was then possible since the physiological processes in

the sense-organs and the brain were not yet open to immediate observation. Thus G. E. Müller, recognizing this situation in 1896, undertook to clarify it by laying down five "psychophysical axioms." The first three, the most important, are as follows:

"1. The ground of every state of consciousness is a material process, a psychophysical process so-called, to whose occurrence the presence of the conscious state is joined."

"2. To an equality, similarity or difference in the constitution of sensations . . . there corresponds an equality, similarity or difference in the constitution of the psychophysical process, and conversely. Moreover, to a greater or lesser similarity of sensations, there also corresponds respectively a greater or lesser similarity of the psychophysical processes, and conversely."

"3. If the changes through which a sensation passes have the same direction, or if the differences which exist between series of given sensations are of like direction, then the changes through which the psychophysical process passes, or the differences of the given psychophysical processes, have like direction. Moreover, if a sensation is variable in n directions, then the psychophysical process lying at the basis of it must also be variable in n directions, and conversely."

Only by way of such axioms could Hering have formed his color theory, or could G. E. Müller have added to it the cortical gray. Hering had no direct evidence about the retina, nor Müller about the cortex. Yet there was some observational ground for the axioms—the observations of Bell and Johannes Müller that lay back of the theory of specific nerve energies.

Grassmann, Fechner, Mach, Hering and G. E. Müller were all analytical in their approaches to this problem. They dealt with single attributive dimensions of sensation and stimulus. It seemed at the time the best way to proceed. By limiting sensation to a linear series and specifying the corresponding stimulus-series, it was possible to control variation in the one and to observe the corresponding variation in the other. Out of such analysis grew the false belief that every simple dimension of sensation is correlated to a simple dimension of the stimulus—brightness or loudness to energy, quality to wave-length or frequency. We know better now (see pp. 25 f.). Any attribute of sensation is likely to be a joint function of several dimensions of the stimulus, yet the simpler correlation of dimension to dimension has persisted for a long time,

only because it began thus and seemed fully as axiomatic as the psychoneural relationship itself.

When Köhler participated in the founding of Gestalt psychology (1920), he made over Müller's axioms in accordance with the new unanalytical dynamic conceptions. It was he, indeed, who applied the term *isomorphism* to this psychoneural relation, he and his colleagues who made the concept so important in Gestalt psychology that it is not always possible in their writings to distinguish between the phenomenal field and the correlated brain field. He was, nevertheless, explicit. The relationship is one of topological order, not of identity of size or shape. "All experienced order in space," he wrote in 1929, "is a true representation of a corresponding order in the underlying dynamical context of physiological process." "All experienced order in time," moreover, "is a true representation of the corresponding concrete order in the underlying dynamical context." And the law for phenomenal organization is similar: "to a context, experienced as 'one thing' belonging together, there corresponds a dynamical unit or whole in the underlying physiological processes."

The criticism of psychoneural isomorphism has come from the physiologists and the positivistic psychologists. The physiologists said that the brain is, in general, a net-work of connections, not a field where dynamic forces, such as the Gestalt psychologists find in perception, can exist. The future will decide that point. Even the physiologists know little as yet about the action of the brain. The positivists said that a mere statement of correspondence between mind and body is not enough, that they wished to know how the one affects the other, and that operational analysis of the nature of the available evidence for isomorphism shows that the axioms should be rewritten so as to state relationships between neural events—between events in the brain and the other physiological events involved in the description of experience. In this contention they are expressing a taste in scientific logic, and again it is the future that will decide whether their preferences will be fruitful enough to prevail.

Notes

Nerve Conduction

For the modern facts and theory of nerve conduction, see the modern physiologies, or more specifically, A. Forbes, *The mechanism of reaction*, *A Handbook of General Experimental Psychology*, 1934, 155-203 (bibliography of 187 titles); or, more briefly, E. G. Boring, *The Physical Dimensions of Consciousness*, 1933, 36-61; also Adrian, Fulton and Lucas, *op. cit. infra*.

On the *animal spirits* and the *vis viva* of the ancients, see J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 1926, 3-55, also bibliography of 1066 titles; *Physiology of the Nervous System*, 1938, historical notes at the beginnings of the chapters, also bibliography of 1361 titles; F. Fearing, *Reflex Action*, 1930, *passim*, esp. 35-58, also bibliography of 554 titles. Less satisfactory is M. Foster, *Lectures on the History of Physiology*, 1901, 255-300 (Vesalius to Haller; bibliography of 40 titles). The discussions in Fulton, 1926, and Fearing are excellent, and the present text is, in comparison, only a brief abstract. In view of their complete bibliographies, we may content ourselves here with the mere listing of nine of the more important books mentioned in the text: R. Descartes, *Les passions de l'âme*, 1650; *Traité de l'homme*, 1662, or *De homine*, 1664; T. Willis, *De motu animalium* (part of *Affectionem*, etc.), 1670; H. Boerhaave, *Institutiones medicae*, 1708; F. Glisson, *Tractatus de ventriculo et intestinis*, 1677 (irritability); G. A. Borelli, *De motu animalium*, 1680 (*succus nervus*); J. Swammerdam's (d. 1680) *Versuche*, published posthumously in H. Boerhaave's *Biblia naturae*, II, 1738; R. Whytt, *An Essay on the Vital and Other Involun-*

tary Motions of Animals, 1751, 2 ed., 1763; J. A. Unzer, *Medicinisches Handbuch*, 1770; 5 ed., 1794; Eng. trans., 1851 [n.v.], (*vis nervosa*).

On the history of *electricity* in the 18th and early 19th centuries, see A. Wolf, *A History of Science, Technology and Philosophy in the Eighteenth Century*, 1939, 213-273.

On *animal electricity*, see first L. Galvani, *De viribus electricitatis in motu musculari*, *De Bononiensi Scientiarum et Artium Instituto atque Academia Commentarii*, (which is a part of *Accad. Sci. Inst. Bologna*), 7, 1791, 363-418. A. Volta's contributions were mostly in letters, which have been brought together in German trans. in his *Briefe zur thierische Elektricität* (1792-1795), 1900 (against Galvani); *Galvanismus und Entdeckung des Saülenapparates* (1796-1800), 1900 (invention of the battery); see also his letter, On the electricity excited by the mere contact of conducting substances of different kinds, *Phil. Trans.*, 90, 1800, 403-431 (Voltaic pile, galvanic chain). On Galvani and Volta, see Wolf, *op. cit.*, 257-267; on Galvani, see Fulton, *op. cit.*, 1926, 34-38.

For Johannes Muller's opinions about *nerve conduction*, see his *Handbuch der Physiologie des Menschen*, Bk. III, Sect. 1, Chap. 3; and Sect. 2, introd., 1834 (or Eng. trans.). The paper which Muller showed du Bois-Reymond in 1841 is C. Matteucci, *Note sur les phénomènes électriques des animaux*, *C.R. Acad. Sci. Paris*, 13, 1841, 540 f. E. du Bois-Reymond began his work in electrophysiology with *Vorläufiger Abriss einer Untersuchung über sogenannten Froschstrom und über die elektromotorischen Fische*, *Ann. Phys. Chem.*, 134, 1843, 1-30. His classical work is *Untersuchungen über thierische Elektricität*, I, 1848;

II (1), 1849; II (2) was published 1860–1884. On du Bois' work, see Fulton, *op. cit.*, 1926, 39–41.

On the *velocity of the nerve impulse*, see H. Helmholtz, Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven, *Arch. Anat. Physiol.*, 1850, 276–364 (Helmholtz's *Wissenschaftliche Abhandlungen*, II, 1883, 764–843); Messungen über Fortpflanzungsgeschwindigkeit der Reizung in den Nerven, *ibid.*, 1852, 199–216 (*Wiss. Abhl.*, II, 844–861). See also E. G. Boring, *A History of Experimental Psychology*, 1929, 42–46, for the story—how Müller and von Humboldt at first refused to believe, how du Bois stood by Helmholtz and persuaded von Humboldt to present the results to the Académie des Sciences, how Helmholtz triumphed.

On the nerve impulse as a *wave of negativity*, see Julius Bernstein, *Die Fortpflanzungsgeschwindigkeit der negativen Schwankung im Nerven*, 1866; *Untersuchungen über den Erregungsvorgang im Nerven- und Muskelsysteme*, 1871.

On *refractory phase*, see H. Kroecker (with W. Stirling), Das charakteristische Merkmal der Herzmuskelbewegung, *Beiträge zur Anatomie und Physiologie als Festgabe Carl Ludwig*, 1874, 173–204; E. J. Marey, *Physiologie expérimentale: travaux du laboratoire de M. Marey*, 2, 1876, 63–86, esp. 72–82, 85 f.; F. Gotch and G. J. Burch, The electrical response of nerve to two stimuli, *J. Physiol.*, 24, 1899, 410–426; Gotch, The submaximal electric response of nerve to a single stimulus, *ibid.*, 28, 1902, 395–416; E. D. Adrian and K. Lucas, On the summation of propagated disturbance in nerve and muscle, *ibid.*, 44, 1912, 68–124.

On the *all-or-none* law, see H. P. Bowditch, Ueber die Eigenthümlichkeiten der Reizbarkeit welche die

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On the *membrane theory*, see W. Ostwald, Elektrische Eigenschaften halbdurchlässiger Scheidewände, *Z. phys. Chem.*, 6, 1890, 71–82; Julius Bernstein, Untersuchungen zur Thermodynamik der bioelektrischen Ströme, *Arch. ges. Physiol.*, 92, 1902, 521–562; R. S. Lillie, Transmission of physiological influence in protoplasmic systems, especially nerve, *Physiol. Revs.*, 2, 1922, 1–37, and references there cited; The recovery of transmissivity in passive iron wires as a model of recovery processes in irritable living systems, *J. gen. Physiol.*, 3, 1920, 107–143; *Protoplasmic Action and Nervous Action*, 1923, or 2 ed., 1932, Chaps. 14 and 15.

Sensory Conduction

On the *Bell-Magendie law* of the spinal nerve roots, see Charles Bell, *Idea of a New Anatomy of the Brain: submitted for the Observations of his Friends*, 1811, private printing of 100 copies; reprinted in *J. Anat. Physiol.*, 3, 1869, 153–166; and again with German trans. as *Idee einer neuen Hirnanatomie*, 1911; François Magendie, *Expériences sur les fonctions des racines des nerfs rachidiens*, *J. Physiol. exp. path.*, 2, 1822, 276–279, 366–371. See also L. Carmichael, Sir Charles

Bell: a contribution to the history of physiological psychology, *Psychol. Rev.*, 33, 1926, 188-217, esp. 191-198 (favors Bell over Magendie); E. G. Boring, *A History of Experimental Psychology*, 1929, 35-38, 44 (favors Bell); Fulton, *op. cit.*, 1938, 20 f. (favors Magendie).

The modern research on sensory conduction has been excellently summarized by E. D. Adrian, *The Basis of Sensation*, 1928; The messages in sensory nerve fibres and their interpretation, *Proc. roy. Soc., Sect. B*, 109, 1931, 1-18; *The Mechanism of Nervous Action*, 1932.

The discussions and experiments cited in the text are, in chronological order, as follows: E. Steinach, Ueber die elektromotorischen Erscheinungen an Hautsinnesnerven bei adäquater Reizung, *Arch. ges. Physiol.*, 63, 1896, 495-520; F. J. J. Buitendijk, On the negative variation of the nervus acusticus caused by a sound, *Akad. Wettensch. Amsterdam*, Proc. Sect. Sci., 13, 1910, 649-652; A. Forbes and A. Gregg, Electrical studies in mammalian reflexes: the correlation between strength of stimuli and the direct and reflex nerve response, *Amer. J. Physiol.*, 39, 1915, 172-235, esp. 225-232; L. T. Troland, The 'all or none' law in visual response, *J. opt. Soc. Amer.*, 4, 1920, 161-186; E. D. Adrian and Forbes, The all-or-nothing response of sensory nerve fibres, *J. Physiol.*, 56, 1922, 301-330, esp. 315-329; Forbes, C. J. Campbell and H. B. Williams, Electrical records of afferent nerve impulses from muscular receptors, *Amer. J. Physiol.*, 69, 1924, 282-303; E. G. Boring, Auditory theory with special reference to intensity, volume and localization, *Amer. J. Psychol.*, 37, 1926, 157-188, esp. 158-161, 177-183; Adrian, The impulses produced by sensory nerve-endings, *J. Physiol.*, 61, 1926, 49-72; Adrian and Y. Zotterman, The impulses produced by sensory nerve-endings: the response of a single end-organ, *ibid.*,

61, 1926, 151-171; The impulses produced by sensory nerve-endings: impulses set up by touch and pressure, *ibid.*, 61, 1926, 465-483; Adrian, The impulses set up by sensory nerve-endings: impulses from pain receptors, *ibid.*, 62, 1926, 38-51; Forbes, R. H. Miller and J. O'Connor, Electric responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 80, 1927, 363-380; Adrian and R. Matthews, The action of light on the eye: the interaction of retinal neurones, *J. Physiol.*, 65, 1928, 273-298; Adrian and K. Umrath, The impulse discharge from the Pacinian corpuscle, *ibid.*, 69, 1929, 139-154; E. G. Wever and C. W. Bray, The nature of the acoustic response: the relation between sound frequency and frequency of impulses in the auditory nerve, *J. exp. Psychol.*, 13, 1930, 373-387; Present possibilities of auditory theory, *Psychol. Rev.*, 37, 1930, 365-380; B. H. C. Matthews, The response of a single end organ, *J. Physiol.*, 71, 1931, 64-110; The response of a muscle spindle during active contraction of a muscle, *ibid.*, 72, 1931, 153-174; Adrian, McK. Cattell and H. Hoagland, Sensory discharges in single cutaneous nerve fibres, *ibid.*, 72, 1931, 377-391; Cattell and Hoagland, Response of tactile receptors to intermittent stimulation, *ibid.*, 72, 1931, 393-404; H. Davis and L. J. Saul, Action currents in the auditory tracts of the midbrain of the cat, *Science*, 74, 1931, 205 f.; Saul and Davis, Electrical phenomena of the auditory mechanism, *Trans. Amer. otol. Soc.*, 22, 1932, 137-145; A. J. Derbyshire and H. Davis, The action potentials of the auditory nerve, *Amer. J. Physiol.*, 113, 1935, 476-504. For further discussion of auditory action potentials and references to the literature, see Chap. 11.

Specific Nerve Energies

The topics of this section have, for the most part, been more fully

considered in E. G. Boring, *A History of Experimental Psychology*, 1929, as cited below.

On the doctrine of specific nerve energies in general (antecedents, Bell, Muller, extension, Helmholtz), see Boring, *op. cit.*, 77-96 and references there cited. See also R. Weinmann, *Die Lehre von den Sinnesenergien*, 1895; W. Nagel, *Handbuch der Physiologie des Menschen*, III, 1905, 1-15; A. J. McKeag, *The Sensation of Pain and the Theory of Specific Sense Energies*, 1902. Bell's argument for the principle is in his brochure of 1811, *op. cit.*, preceding section of these notes. For Johannes Müller on the doctrine, see his *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, 44-55; *Handbuch der Physiologie des Menschen*, II, 1838, Bk. 5, introductory section (or Eng. trans.).

On *adequate stimulation* (specific irritability), see A. v. Haller, *De partibus corporis humani sensibilibus et irritabilibus*, *Comment. Soc. reg. Sci. Göttingensis* (Gesell. Wiss. Göttingen), 2, 1752, 114-158; Müller, *loc. cit.*; E. Hering, in L. Hermann's *Handbuch der Physiologie*, III (2), 1880, 430-433; C. S. Sherrington, in E. A. Schäfer's *Text-Book of Physiology*, 1900, II, 950-954, 960 f.; *The Integrative Action of the Nervous System*, 1906, 12 f.; McKeag, *op. cit.*, 47-50. The unfortunate word *inadequate* is used by McKeag (1902) in double quotation marks, as if (incorrectly) Müller used it. Sherrington (1906) does not use it, but it seems to have drifted into modern texts. In 1911 Luciani used it but Ladd and Woodworth did not.

On the *extension* of the doctrine to specific qualities within the senses, see Natanson, *Arch. physiol. Heilk.*, 3, 1844, 515-535; A. W. Volkmann, in R. Wagner's *Handbuch der Physiologie*, II, 1844, 521-526; H. Helmholtz, *Ueber die Theorie der zusammengesetzten Farben*, *Ann. Phys. Chem.*, 163, 1852, 45-66, esp. 47-49 (reprinted in his *Wissenschaftliche*

Abhandlungen II, 1883, 2-23, esp. 6 f.); *Handbuch der physiologischen Optik*, II, 1860, sect. 17 (same in 3 ed., and Eng. trans.); also 2 ed., 1896, 349, where Helmholtz notes that his use of Müller's principle is an extension of it.

On *phrenology*, see Boring, *op. cit.*, 47-57 and references there cited. F. J. Gall's classics are *Anatomie et physiologie* (full title in the text), 1810-1819 (Spurzheim was collaborator in the first two volumes); *Sur les fonctions du cerveau et sur celles de chacune de ses parties*, 1825. Opposed to Gall was M. J. P. Flourens, *Examen de la phrénologie*, 1842. On the antecedents of phrenology in the faculty psychology of the Scottish School, see H. D. Spoerl, *Faculties vs. traits: Gall's solution*, *Character and Personality*, 4, 1936, 216-231, esp. 219-225.

On *localization of brain function* in general, see J. Soury, *Cerveau*, in C. Richet's *Dictionnaire de Physiologie*, II, 1897, 547-670, 788-952; Boring, *op. cit.*, 58-76, 559 f., 568 f. (Rolando, Flourens, Broca, Fritsch and Hitzig, Franz, Lashley). In particular, see M. J. P. Flourens, *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés*, 1824; *Expériences sur le système nerveux*, 1825; 2 ed. of both under title of first, 1842; C. Broca, *Remarques sur le siège de la faculté du langage articulé, suivi d'une observation d'aphémie (perte de la parole)*, *Bull. Soc. anat.*, 2 ser., 6, 1861, 330-357; G. Fritsch and G. Hitzig, *Ueber die elektrische Erregbarkeit des Grosshirns*, *Arch. Anat. Physiol.*, 1870, 300-332; K. S. Lashley, *Brain Mechanisms and Intelligence*, 1929, esp. 23-26, 86-89, 157-174; W. S. Hunter, *A consideration of Lashley's theory of the equipotentiality of cerebral action*, *J. general Psychol.*, 3, 1930, 455-468.

On *sensory centers*, see Soury, *op. cit.*, pp. 898-952; also the excellent

elementary discussion as of its date in W. James, *Principles of Psychology*, 1890, I, 41-62. For the original experiments and discussion of the three men especially mentioned in the text, see D. Ferrier, *The Functions of the Brain*, 1876, 2 ed., 1886; F. L. Goltz, *Ueber die Verrichtungen des Grosshirns*, 1881 (4 papers, 1876-1881); H. Munk, *Ueber die Functionen der Grosshirnrinde*, 1890 (17 papers, 1877-1889). The text makes no attempt to treat adequately the history of research and opinion in this complicated and disputatious field. As an introduction to that history, see the discussion of it and the 52 references cited by J. F. Fulton, *Physiology of the Nervous System*, 1938, 340, 347 f., 365, 376 f., 397 f.

On *specificity of afferent somatic fibers* (size of fiber, speed of impulse), see J. Erlanger, H. S. Gasser and G. H. Bishop, The compound nature of the action current of nerve as disclosed by the cathode ray oscillograph, *Amer. J. Physiol.*, 70, 1924, 624-666; Gasser and Erlanger, The role played by the sizes of constituent fibers of a nerve trunk in determining the form of its action potential wave, *ibid.*, 80, 1927, 522-547; Erlanger and Gasser, The action potential in fibers of slow conduction in spinal roots and somatic nerves, *ibid.*, 92, 1930, 43-82. On the possibility that the small slow fibers may mediate pain, see E. D. Adrian, The message in sensory nerve fibres and their interpretation, *Proc. roy. Soc.*, Sect. B, 109, 1931, 1-18; *The Mechanism of Nervous Action*, 1932, 22-60.

Projection

In a large measure this section is an anticipatory summary of many matters which are discussed in detail later in this book. See, therefore, the cross-references in the text and then, for the appropriate citations of the literature, the notes for topics to which cross-reference is made.

Julius Bernstein's theory of projection and irradiation is in his *Untersuchungen über den Erregungsvorgang im Nerven- und Muskelsystem*, 1871, 165-202.

For the modern status of the anatomy and physiology of projection, see J. F. Fulton, *Physiology of the Nervous System*, 1938, 291-398. On auditory projection, see S. S. Stevens and H. Davis, *Hearing*, 1938, 413-418, 431-433; and, in particular, H. W. Ades, F. A. Mettler and E. A. Culler, Effect of lesions in the medial geniculate bodies, *Amer. J. Physiol.*, 125, 1939, 15-23.

On the possibility of functionally precise point-to-point projection in spite of cortical end-arborizations of the neurons, see the discussion in C. T. Morgan's forthcoming *Physiological Psychology*.

Isomorphism

On the views of four of the men mentioned in this section, see E. G. Boring, *A History of Experimental Psychology*, 1929, and references there cited, as follows: Descartes, dualism and interactionism, 158-164, 177; Leibnitz and parallelism, 165-168, 177 f., Hartley,^a associationism and parallelism, 195-202, 206; Fechner, psychophysics, parallelism, *Nachtsansicht* and *Tagesansicht*, 265-287.

For the history of psychophysical parallelism, see R. Eisler, *Wörterbuch der philosophischen Begriffe*, 2 ed., 1929, II, 377-386 (art. *Parallelismus*). For a representative presentation of the view as of the late nineteenth century and influenced by the theory of the conservation of energy, see A. Bain, *Mind and Body*, 1872 *et seq.*, esp. Chap. 6; also his The correlation of nervous and mental forces, in Balfour Stewart, *The Conservation of Energy*, 1874, 205-236. For a fairly late defense of interactionism, see W. James, *Principles of Psychology*, 1890, I, 128-144.

The classical papers on the mechanical equivalent of heat (Joule) and the laws of the conservation of energy and of entropy (Kelvin) are J. P. Joule, On the mechanical equivalent of heat, *Phil. Trans.*, 140, 1850, 61-82 (read June 21, 1849); William Thomson (Lord Kelvin), On the dynamical theory of heat, *Trans. roy. Soc. Edinburgh*, 20, 1851, 261-298, 475-482.

For the development of the psychophysical axioms into the isomorphic principle, see R. H. Lotze, *Medizinische Psychologie*, 1852, 206-232, esp. 230-232; H. Grassmann, Zur Theorie der Farbenmischung, *Ann. Phys. Chem.*, 165, 1853, 69-84 (Eng. trans., *Phil. Mag.*, 4 ser., 7, 1854, 254-264); E. Mach, Ueber die Wirkung der räumlichen Vertheilung des Lichtreizes auf der Netzhaut, *Sitzungsber. Akad. Wiss. Wien*, math.-naturw. Cl., 52 (2), 1865, 303-322, esp. 320; E. Hering, *Zur Lehre vom Lichtsinne*, 1878, 74-80, esp. 77; F. C. Donders, Ueber Farbensysteme, *Arch. Opthal. Berlin*, 27 (1), 1881,

155-223; G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Z. Psychol.*, 10, 1896, 1-82, esp. 1-25; M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *ibid.*, 61, 1912, 161-265, esp. 246-252; W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, 173-195, esp. 193; *Gestalt Psychology*, 1929, 58-69; *The Place of Value in a World of Facts*, 1938, 185-232; K. Koffka, *Principles of Gestalt Psychology*, 1935, 61-67.

One criticism, as well as a positivistic restatement of isomorphism, is to be found in E. G. Boring, Psychophysical systems and isomorphic relations, *Psychol. Rev.*, 43, 1936, 565-587, esp. 579-586; A psychological function is the relation of successive differentiations of events in the organism, *ibid.*, 44, 1937, 445-461, esp. 454 f.; An operational restatement of G. E. Müller's psychophysical axioms, *ibid.*, 48, 1941, 457-464. The issue finally reduces itself to nativism vs. empiricism.

Chapter 3

VISUAL SENSATION: BEGINNINGS

AS WE turn now to the history of the psychology of sensation, we see how recent has been the emergence of the separate sciences from natural philosophy. The formal distinction between psychology, physiology and physics is but a product of the scientific specialization of the nineteenth century. Only recently could it be said that psychology deals with immediate experience and physics with mediate experience, for originally all science bore directly the stamp of its own empiricism. The natural philosopher was interested simply in the natural world as he knew it or could come to know it, and he knew it first, of course, in immediate experience. Thus *light* is a concept invented to explain vision, since it was perception that set the first problems for physics. In fact, three of the classical chapters of physics—light, heat and sound—were fixed by the nature of perception; it was only much later that imperceptibles like electricity, magnetism, invisible light, radiant energy and wave-motion became equally important. For this reason, then, the first important knowledge of color and its stimulus was contributed by Newton, of visual space perception by Kepler, of the tonal stimulus by Galileo. These problems, which the psychologists now claim, had meaning and importance for the great natural philosophers of the seventeenth century. They were neither physics nor psychology then; they were philosophy.

Before Newton

The early knowledge about light consisted of certain simple laws of its behavior and control. The Greeks knew that burning glasses—usually glass spheres filled with water—magnify the objects seen through them. Euclid laid down the laws of optics—that light travels in straight lines, that the angle of reflection is equal to the angle of incidence. Although refraction was known, philoso-

phers erroneously believed that the angle of refraction equals the angle of incidence; only in 1621 did Snell formulate the exact law. Lenses could be ground, however, and Roger Bacon in using them as an aid to vision may be said to have invented spectacles (1266). By the sixteenth century spectacles were being made commercially, and it appears to have been a Dutch spectacle-maker who, in 1608, made the first telescope as a curiosity. Galileo seized at once on this principle, built a better telescope in 1609, and, by use of it, observed the satellites of Jupiter early in 1610. Indeed, the practical scientific need for the perfection of the telescope did more than anything else to stimulate an interest in optics at this time. Thus Kepler, the astronomer, published in 1604 and 1611 two important works on optics, dealing with the refraction of light, the properties of lenses and the theory of vision.

Meanwhile there was no adequate conception of the nature of light. The ancients had thought of light as an emanation from the eye, one which mingles with another emanation from the object—a partly subjective view which is natural enough when one considers how voluntary is vision. The Epicureans, for example, believed that objects emit images which enter the eye and give rise to the perception of the objects. It was this view, as we have seen, which Johannes Müller was combating (1826, 1838) when he argued that the sensorium perceives, not the transmitted properties of objects, but the specific energies peculiar to the excited nerves. These ideas show us how the physical problem was originally psychological: the concept of light came into existence to explain the correct perception of objects at a distance. Aristotle came nearer the modern view in supposing that light is a quality of a medium intervening between the object and the eye. Descartes (1630 *et seq.*) held light to be a thrust or pressure of the seen object upon the eye through the intervening medium.

During the seventeenth century the two classical theories of light became established as alternatives, the one holding light to be a wave-motion of an all-pervading medium (undulatory theory) and the other asserting light to be the projection of particles by luminous bodies (corpuscular theory). The corpuscular theory was consistent with the traditional view that objects give off something which enables them to be perceived. The undulatory theory, on the other hand, seemed better to agree with the speed of light and the fact that bodies do not become exhausted by remaining

visible. Though there are said to be hints of the undulatory conception in the writings of Leonardo da Vinci and Galileo, the theory was first definitely laid down by F. M. Grimaldi (1665). Huygens too (ca. 1678) effectively promoted the theory; but, since he and the others of his time thought of the waves as longitudinal—for that is the natural way in which a wave could exert a thrust or pressure upon the eye—he could not explain double refraction and the facts of polarization when these phenomena came to his attention. Newton, however, after weighing both views, finally threw the weight of his opinion to the corpuscular hypothesis. He could reconcile it better with the phenomena of rectilinear transmission of light, of polarization, and of the colors of thin films.

With such uncertainty about the nature of light, there was, of course, no assurance about the rate of its propagation. Tradition held the transmission of light to be instantaneous, a conception which has the advantage of according with the common belief that we see objects as they exist at the moment, not as they were in the immediate past. (The same belief was to lead physiologists before the middle of the nineteenth century to believe that the transmission of the nervous impulse is so rapid as to be practically instantaneous, else perception would lag behind its perceived event. It seemed evident to common sense that there is no such lag.) In this matter of the velocity of light Kepler held to tradition. Descartes, however, who thought of light as a pressure-thrust in the intervening medium, could conceive of its speed as infinite, and it did not seem to him that time would be required for a pressure to be transmitted when no body had to be actually displaced. It was Römer (ca. 1676) who found a difference in the time of the eclipse of one of Jupiter's moons according as the earth was moving toward or away from Jupiter, and who thus computed a finite velocity for light at a value which is about two-thirds of its true value.

The development of the physiological optics of the eye is the necessary prelude to the psychology of visual space perception. Before Kepler there seems to have been no adequate conception of the functioning of the eye. The traditional view was derived from the Arab, Alhazen (d. ca. 1039 A.D.), who considered the lens as the sensitive perceptive organ. Kepler (1604, 1611) noted, as had Leonardo da Vinci before him, the similarity of the eye to a *camera obscura*. Concluding that the lens of the eye forms a small in-

verted image of a perceived object upon the retina, he decided, therefore, that "sight is a sensation of the stimulation of the retina." In addition, he noted the fact of after-image as showing that the retinal image persists after the stimulation has ceased, and he espoused a chemical theory of stimulation by suggesting that light affects the retina in the way that light from a burning glass affects a combustible substance. Accommodation he explained as due to change in the relative displacement between the lens and the retina, noting too that beyond the retina it is necessary for the image to be transmitted by a "spiritual current" to the seat of the faculty of sight in the brain. Thus he raised, and for the first time, that constantly recurring question as to why, when the retinal image is inverted, we nevertheless see the world as upright. All told, Kepler may be said to have formulated the essentials of physiological optics as they still existed for Johannes Müller two centuries later.

Descartes helped to establish Kepler's view. Having demonstrated the formation of retinal images in an eye from a bull, he described the fact of varying convergence in the perception of objects at different distances. It was only a little later (1668) that Mariotte discovered the blind spot, and localized it at the point of emergence of the optic nerve from the retina. This spectacular discovery was even demonstrated by the Royal Society to Charles II. It clearly established the retina, and not the lens, as the perceptive surface. *

The physics of light, however, had still not caught up with the obvious facts of visual perception. There was before Newton no satisfactory theory of color. The infinite variety of the visually perceived world was up to that time explained only as to its spatial characteristics: light travels in straight lines and, following the refractive laws of the ocular optical system, forms an image on the sensitive retina. Yet without color the world would lose many of its distinctive perceptual characteristics. The ancient theory was that color results from a mixture of light and darkness—as indeed might be said to be true of the grays, though not of the chromatic qualities. Kepler tried unsuccessfully to ascribe color to different properties of the perceived object. Descartes for his part suggested that color depends upon different rates of rotation of the particles of the transmitting medium. That bodies modify light in accordance with their structure, in the way that different bodies give off dif-

ferent musical notes, was the proposal made by Grimaldi. Finally Hooke (1665) held that there are two primary colors, red and blue, that the other colors are intermediate between these two, and that the color depends upon the obliqueness with which the ray strikes the retina—one oblique angle inducing red, the opposite angle blue, and intermediate angles intermediate hues. He came to this view from a study of the colors produced by thin plates. Although these last three theories all suggest that color is a special property of light, they are vague, speculative and unsatisfactory. It took the genius of Newton to discover that color is simple and fundamental, and white light a special case (a mixture) of colors.

Isaac Newton

The formulation of the first correct theory of color is due to Sir Isaac Newton (1642–1727). Newton, whose name is one of the most distinguished in the history of science, had already entered upon almost all of his important lines of thought before he was thirty. In the years 1665 and 1666, the years of the Great Plague, Newton, then in his early twenties, a student but not yet a Fellow at Trinity College in Cambridge University, achieved the following ideas: (1) he discovered the binomial theorem; (2) he invented (except for an anticipation by his teacher, Isaac Barrow) both the differential and the integral calculus; (3) he conceived his theory of gravitation, and applied it to the behavior of the moon, being prevented for twenty years from public announcement of the law only because his calculations, based upon an incorrect measure of the size of the earth, were not consistent; and (4) he purchased at the Stourbridge Fair a prism for the purpose of studying the refraction of light. The experiments with the prism, postponed by his absence from Cambridge at the time of the Plague, he took up shortly thereafter. It was by way of these researches on optics that Newton first came to scientific fame.

Newton's discovery was that the refraction of white light by a prism breaks it up into the different spectral colors which can be differentiated from one another, each by its specific degree of refraction. At first this discovery was primarily important to him because of its bearing on the improvement of the telescope. That spherical lenses entail an aberration due to their shape had been shown by Descartes. Now Newton, seeing that lenses must also in-

duce a chromatic aberration, decided, because of his erroneous conclusion that all glasses have the same refractive power, that it would be forever impossible to correct for this distortion. Thus he was led—since there is no chromatic dispersion in the reflection of light—to invent the reflecting telescope. He constructed with his own hands, polishing the mirror himself, a small model which was presently shown to the Royal Society and caused him to be elected to its membership in 1672.

Surprised at the favor with which his telescope was received, he offered to present to the Society a discovery which, as he put it, "I doubt not will prove much more grateful than the communication of that instrument [the telescope], being in my judgment the oddest if not the most considerable detection which hath hitherto been made into the operations of nature." This odd and considerable detection was his research on color, which he presented in a paper in 1672, when he was twenty-nine. The paper drew great attention and also much criticism, especially from Robert Hooke. Controversy about facts, which, being hardly more than the description of experimental results, seemed scarcely controvertible, lasted for three years. Newton hated the controversy. He said: "I was so persecuted with discussions arising out of my theory of light that I blamed my own imprudence for parting with so substantial a blessing as my quiet to run after a shadow." This bitterness about scientific quarrels lay heavy upon him for over twenty years, leading him to avoid and delay publication. The full account of his experiments on light he withheld until after all his other important work, for it was thirty-two years after his youthful paper before the Royal Society in 1672 that, as president of the Society, he published in 1704 the *Opticks*, with three later editions following on (1717, 1721, 1730).

Newton's contributions to the theory of color may be enumerated as follows.

1. He showed that every color has its specific degree of refrangibility, and that every degree of refrangibility has its specific color. Thus he established refraction as the fundamental operation for chromatic analysis.

His simple experiment is shown in Fig. 7. A beam of white light from the sun in being refracted by a prism, *P*, was dispersed on the screen, *S*, into the spectral band of colors, *vibgyor*. There was no dispersion in the direction of the axis of the prism: the band of

colors was just as wide as the round hole, A, through which the light came. In the direction of refraction the band was, however, never less than five times as long as the diameter of the hole, and, of course, the colors always fell in the same order, with the violet on the side of the base of the prism. Newton complicated this experiment in various ways, always with the same general conclusion. For instance, he introduced (Fig. 7) a second prism after the prism, P, and at right angles to it, so that the light was refracted

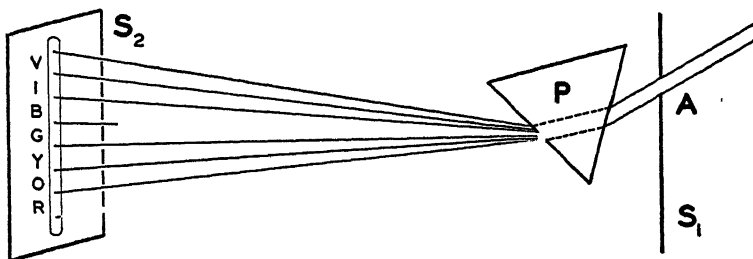


FIG. 7. NEWTON'S EXPERIMENT ON THE PRODUCTION OF COLORS BY REFRACTION (1704)

A beam of white light, passing through the small circular aperture, A, in the shutter, S_1 , is refracted by the prism, P, producing the spectrum, VIBGYOR on the screen, S_2 . The spectral band is as wide as the aperture, A, but much longer, because of the different refrangibility of the different colors. Newton thought that only seven colors could be distinguished in the spectrum.

horizontally as well as vertically on the screen, S_2 . He wondered whether he would obtain thus a square spectrum, but the effect was that the band on S_2 was rendered diagonal on the screen instead of vertical. In other words, the "homogeneous" violet at v was not dispersed, being homogeneous, but was refracted most to the side, and the "homogeneous" red at the other end, r , was not dispersed but was refracted least to the side. This experiment showed that pure light, while still obeying its law of refraction, cannot be split up further by refraction, but is bent an amount that is proper for its color.

2. Thus Newton also showed that the white light of the sun is not simple but is compounded of all the colors.

While this conclusion follows from the experiment of Fig. 7, it is shown more convincingly by the experiment diagrammed in Fig. 8. Here the beam of white light, W_1 , through a round hole, A, is

dispersed by the prism, P_1 , into the spectrum, vr , which falls upon the lens, L . The lens collects the dispersed colors and focuses them upon the prism, P_2 , which is placed so as to reverse the refractive action of P_1 . Thus at W_2 we get a beam of white light again, exactly like the beam at W_1 . If a third prism, P_3 , is introduced, then the beam at W_2 is again refracted and dispersed to give the spectrum, rv , on the screen, S_2 . In this particular arrangement, the spectrum at S_2 is reversed from the spectrum at L , since red is

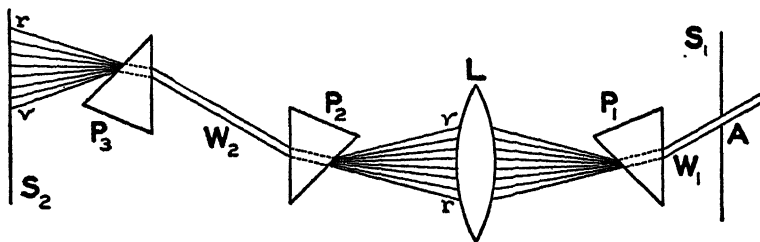


FIG. 8. NEWTON'S EXPERIMENT ON COLOR MIXTURE (1704)

W_1 is a beam of white light which passes through the aperture, A , in the shutter, S_1 , and, being refracted by the prism, P_1 , has its color dispersed as a spectrum, vr , upon the lens, L . The lens brings the colors together again on the prism, P_2 , which, reversing the refraction at P_1 , recombines the colors into a beam of white light, W_2 , which is like the original W_1 . The white light of W_2 can again be refracted and dispersed by the prism, P_3 , so that the spectrum, rv , is thrown on the screen, S_2 .

above violet in the one case, and below in the other. That the light could be so completely mixed up and then sorted out again by refraction must have been to Newton one of those "oddest if not the most considerable detections" which he said his early researches had accomplished.

The optical system of Fig. 8 also showed Newton that white light is a particular combination of colors. If he eliminated one or several colors by intercepting them near L , the light at W_2 was in general no longer white, but of a hue corresponding to a mixture of the remaining colors.

- ✓ 3. Newton concluded erroneously that there are only seven separate homogeneous colors: red, orange, yellow, green, blue, indigo and violet. To account for this error we have to consider the psychology of the scientist. White light had always been regarded as simple. To ask natural philosophers to believe that it is a combination of even as few as seven colors was to strain credulity, as in-

deed the bitter opposition to Newton's theory showed. To propose a very large number or an infinitude of colors would have seemed an absurdity; in asking for seven colors Newton was already asking for more adjustment of thought than he could easily get. Nor was Newton himself ready to find a finer analysis. He became convinced, in fact, that there ought to be some similarity between the colors and the musical intervals of an octave. There are seven different tones in an octave; why not seven colors in the spectrum? If the spectrum, he said, were spaced off by divisions placed so as to be proportional to the numbers 1, $\frac{3}{8}$, $\frac{5}{8}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, and $\frac{1}{2}$, the spacing representing "the Chords of the Key, and of a Tone, a third Minor, a fourth, a fifth, a sixth Major, a seventh and an eighth above that Key," then "the Intervals will be the Spaces which the several Colours (red, orange, yellow, green, blue, indigo, violet) take up."

In view of Newton's great caution in drawing conclusions and his careful use of experiment to constrain opinion, it is natural for us to wonder how he arrived at this correlation between the spectrum and the octave. Newton wrote: "I delineated therefore in a Paper the Perimeter of the Spectrum, . . . and I held the Paper so that the Spectrum might fall upon this delineated Figure, and agree with it exactly, whilst an Assistant, whose Eyes for distinguishing Colours were more critical than mine, did by right lines drawn cross the Spectrum, note the Confines of the Colours. . . . And this Operation being divers times repeated both in the same, and in several Papers, I found that the Observations agreed well enough with one another." Did the assistant just chance upon seven divisions as convenient with the spectrum that was presented to him? Did he have the nature of the musical octave in mind when he decided where to put his lines? Did Newton suggest any principle of division to him? We shall never know, but we seem here to have a case of what has since been called the effect of 'laboratory atmosphere.' The observation was much more sensible than the truth.

Newton could not test this part of the theory further by refractive analysis. With enough refraction he might have expected to divide the spectrum into seven separate patches. A spectrum spread so long, however, becomes too faint to observe. The same thing happens if the aperture, *A* (cf. Fig. 7), is made very small. The spectral band, *vr*, consists of a series of overlapping images of

the hole in the screen, S_1 . If the hole were very small, one might expect to get seven small circular spots for a spectrum; but actually, if the hole is very small, the spectrum is too faint to be observed clearly. Newton tried a triangular hole, which gave a bright continuous spectrum on the side corresponding to the base of the triangle, but only a very faint band on the side corresponding to the apex. Even though he never got seven spots of color, it was to be more than a century before anyone realized that the spectrum includes an infinitude of wave-lengths.

4. Newton formulated two of the laws of color mixture.

(I) "Colours may be produced by Composition which shall be like to the Colours of homogeneous Light as to the Appearance of Colour, but not as to the Immutability of Colour and Constitution of Light. And those Colours by how much they are more compounded by so much are they less full and intense, and by too much Composition they may be diluted and weaken'd till they cease, and the Mixture becomes white or grey. There may be also Colours produced by Composition, which are not fully like any of the Colours of homogeneous Light."

(II) "Whiteness and all grey Colours between white and black, may be compounded of Colours, and the whiteness of the Sun's Light is compounded of all the primary Colours mix'd in a due Proportion."

The greater number of experiments supporting these laws was made with the optical system of Fig. 8. By allowing only certain colors to pass at L , the resultant mixture can be seen by placing a screen at W_2 . Newton found that, with proper combinations of lights, fewer than all the primaries can produce white. He found too that, the more complex the mixture, the lesser the saturation of the color. The rapid succession of colors, he showed, produces the same result as their simultaneous mixture. This last discovery he made by fashioning a cardboard comb with teeth as wide as the spaces between them and moving the comb rapidly up and down at the lens, L (Fig. 8). In this case some of the colors were always intercepted at any moment but the rapid succession gave white at W_2 . His reference to mixed colors, "which are not fully like any of the Colours of homogeneous Light," was, of course, to the purples.

He also showed that gray is simply a less "luminous" white. After rubbing gray powder on the floor of his room and letting the sunlight fall upon it, he placed a piece of white paper of the same

size as the patch of gray alongside the gray but in the shadow. When he viewed these two stimuli at a distance too great for him to identify the objects by their texture, the gray powder looked whiter than the white paper. Then he adjusted the intensity of the light until they looked alike. He checked the observation. "A Friend coming to visit me, I stopp'd him at the Door, and before I told him what the Colours were, or what I was doing; I asked him, Which of the two Whites were the best, and wherein they differed? And after he had at that distance viewed them well, he answer'd, that they were both good Whites, and that he could not say which was best, nor wherein their Colours differed."

5. Finally we must note that Newton by these experiments was able to advance what was at once the first and the correct theory as to why objects are differently colored. The color of objects, he decided, is due to their selective reflection of the homogeneous lights. "They become colour'd by reflecting the Light of their own Colours more copiously, and that of other Colours more sparingly." For instance, he found that cinnabar, placed at W_2 (Fig. 8), became more red when the blue and green light was intercepted at L , and that it appeared not red at all when the red light was intercepted, but a dull yellow-green.

It was in these ways that Newton discovered and established what is in its essentials the correct theory of color. The discovery is an instance of the unusual case where an apt experiment under the attention of a keen mind establishes a positive body of fact where only vague and incorrect guesses had existed before. Newton's theory of color had no anticipators. There has been no argument about priority in respect of it. It was all too absurd a truth to have been guessed—that fact that white or gray, which appears as primary, fundamental and prior to the colors, should be, in contradiction of immediate experience, nothing but a mixture of all the colors instead of the absence of all of them. Of the advantage of experiment over intuition there could be no better illustration.

Thomas Young

During the eighteenth century there was very little advance in the psychophysiology of vision. Research in optics had mostly to do with the development of the reflecting telescope, which avoids chromatic aberration, and of the achromatic lens, which corrects

chromatic aberration in the refracting telescope. Huygens had sponsored the wave-theory of light, but Newton had come over gradually to the corpuscular theory, and so indelibly was Newton's view impressed upon the scientists of the century that an editor could assert in 1792 that the wave-theory was not held by a single physicist of importance—although actually Euler and Benjamin Franklin had supported it. The shift back to the wave-theory was brought about by Thomas Young in England, by Fresnel and Arago on the Continent. Young, working with the colors induced by thin plates, developed the theory of interference, which requires a wave-hypothesis of light for its validation.

There was during the century some interest in the mechanism of the eye, and William Porterfield published in 1759 *A Treatise on the Eye, the Manner and Phaenomena of Vision*. Particularly was there an interest in the means by which the eye accommodates its focus to different distances. There were three chief theories of accommodation: (1) that the cornea changes its curvature; (2) that the eye changes its length so that the distance between the lens and the retina is altered; and (3) that the lens changes its curvature. (See pp. 272-279.) A fourth implausible view was (4) that the refractive index of the media of the eye might be altered in accommodation. Thomas Young, as we shall see presently, performed experiments that led him to the correct theory, viz., that the lens changes its shape.

Although the chief interest in color lay in getting rid of it in telescopes, still there had been a gradual change of view since Newton. It became clear that, although the number of perceived colors may be limited (cf. Newton's seven), there is nevertheless an infinitude of degrees of refraction within the spectrum, that is to say, the spectrum is continuous and there are many more than seven kinds of homogeneous light. The discovery by W. H. Wollaston in 1802 of fixed black lines in the solar spectrum (the lines described more fully by J. v. Fraunhofer in 1814 and named after him) served to differentiate successive parts of the spectrum much more finely than could be accomplished by any process of immediate observation and color naming. (See p. 177.)

This distinction between the color of light and the color of sensation was enhanced by the discovery of color-blindness, or Daltonism as it came to be called. John Dalton, the distinguished chemist who discovered the law of multiple proportions, described

in 1794 his own case of color-defect and several other similar cases that had come to his attention. The discovery aroused a great deal of scientific interest. (See pp. 184-188.)

Into this scene Thomas Young (1773-1829) entered. By training and subsequent profession he was a physician, but his temperament and interests made him one of the brilliant natural philosophers of his day. His scientific contributions, according to the list drawn up by one biographer, covered "fields as diverse as the physiology of the human eye, hydrodynamics, music, paleography, atmospheric refraction, theory of tides, tables of mortality, and theory of structures." His greatest contribution, as we have seen, lay in the reestablishment of the wave-theory of light. For us he becomes important because of his theory of ocular accommodation and his theory of color, both of them theories that failed to gain proper recognition until Helmholtz established them half a century later.

Thomas Young sought to prove experimentally that the *accommodation* of the eye is due to a change in the curvature of the crystalline lens. His first paper on this subject, presented to the Royal Society in 1793, resulted in his election to its membership only a few days after his twenty-first birthday. Ocular accommodation was a suitable topic for a medical student who wished to undertake research. His principal paper on this subject, however, was read to the Society in 1800 and published in 1801. In it he described a simple optometer by which he could measure the focal length of the eye. He also presented computations to show the refractive power of the ocular media, and concluded that the ratio of the refractive power of the lens to the refractive power of water is about $2\frac{1}{2}_0$.

He then undertook to dismiss the alternative theories of accommodation. That the cornea does not change its curvature in accommodation he decided on the basis of two pieces of evidence. First, he failed to find, even with microscopic observation, any change in the distance between the images of two candles reflected from the cornea while accommodation was being altered. Secondly, he found that his power of accommodation was in no way impaired by attaching a small glass lens by wax to his cornea in front of the pupil and filling the space between the lens and the pupil with water. With water next to the cornea, the curvature of the cornea could have little refractive power, and the glass lens

really acted as an artificial cornea, the shape of which could not be changed. It is doubtful if the eye-ball could be elongated for near fixation (thus bringing the lens farther from the retina) without the cornea's being altered in curvature, but Young supplemented the argument against this theory of accommodation by another experiment.

Turning his own eye as far as possible inward toward the nose, he pressed an oval iron ring against the front of the eye-ball so that he could look through the ring, and then pressed the "ring of a key" at the outside of the socket in behind the eye-ball as far as he could without too much pain, so that the key was "wedged between the eye and the bone." He assumed that in this manner the eye was constrained mechanically so as to prevent its elongation, and yet he found accommodation unimpaired, although the pressure upon the retina created 'phantoms.'

There remained the possibility that the refractive power of the ocular media might change. This theory he ruled out by the observation that persons from whose eyes the lens has been removed lose the power of accommodation. Thus there seemed to be left no possible mechanism for accommodation other than a change in the shape of the lens. Young made computations as to the form of the change in the shape of the lens by studying the aberration of rays of light that enter at the extreme sides of the pupil and fall upon the edges of the lens. In this way he arrived at a solid conclusion concerning the nature of accommodation, although it remained for Helmholtz to establish the theory and to show how the ciliary process affects the change.

Thomas Young's theory of *color sensation* appears in four paragraphs incidental to his general discussion of the physical theory of colored light. Inasmuch as the theory is purely speculative, it is doubtful if it could have interested Young so keenly as his experimental demonstrations of fact. He first presented his views in 1801, putting them forth as a modification of Newton's theory.

Newton had suggested in a communication to the Royal Society in 1675—before he had accepted the corpuscular theory of light—that the various rays of light excite vibrations in the retinal terminations of the optic nerve, "the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power"; and these vibrations, he had added,

“will run . . . through the optic nerves, into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all with white—much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses to generate sounds of divers tones, for the analogy of nature is to be observed.”

Newton was thinking about seven colors analogous to seven musical tones in the octave. Young, on the other hand, had to consider a very large number of wave magnitudes in the spectrum. Moreover, Newton was thinking in terms of the traditional view of perception, to the effect that properties of objects (in this case, vibrations) get themselves transmitted to the sensorium. Young, however, saw that some physical mechanism must be interposed at the retina, if only because it would not have been possible for a single retinal point to transmit all the infinitude of vibrations that make up the white light which is perceptible at a single point.

With the suggestion that the taking up of the vibrations by the retinal substance “must be dependent upon the constitution of this substance,” Young created a necessity for separate retinal particles with a nervous connection for each. This assumption led to further consequences. He said: “As it is almost impossible to conceive of each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as light composed of those two species; and each sensitive filament of the nerve may consist of three portions, one for each principal colour.” This was in 1801. A year later new measurements of the spectrum by Wollaston led Young to modify these remarks “respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet,

for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6." These numbers are, of course, approximately proportional to the now known wave-lengths of their respective colors.

Young's theory is really a resonance theory of color, which, by appealing to Newton's laws of color mixture and their corollary that all colors can be got from a mixture of three widely separated hues, was able to take account of the sensitivity of a single retinal point to a very large variety of colors. Thus naturally did Young espouse a theory in which color is represented physiologically by the particular nervous elements excited. In so doing he abandoned the traditional theory that the properties of objects are conducted to the sensorium, and anticipated Johannes Müller's theory of specific nerve energies (1826, 1838)—a theory which holds that the sensorium perceives, not the exciting objects, but the state of the excited nerves. Had Young realized the importance of his conception, instead of leaving it for Müller and Helmholtz to discover, he might have had more effect upon the history of the psychophysiology of sensation. As it was, he received credit eventually, but not until long after his death.

Goethe

The poet Goethe (1749–1832) enters this picture because of his intense and aggressive interest in the theory of color, an interest that caused him to publish a book of two volumes, *Zur Farbenlehre*, in 1810. Goethe was one of the great Germans of his generation, a man of exceptional genius, ambition, power, vanity and influence. A keenly intuitive observer of life and the world, he advocated observation as the primary means of gaining insight into truth. His faith in the validity of intuition, an appropriate characteristic for a poet, stamps him as more nearly the philosopher by temperament than the scientist, making him impatient, as it did, of the intricacies of an experimental method which may yield results at variance with the data of intuitive observation. Science adopted the experimental technique because it mistrusted the adequacy of immediately intuitive observation, but to that view Goethe placed himself vigorously in opposition. His *bête noir* was Newton, whose theory of color he reviled in bitter tirade. Presently we must speculate upon the reasons for Goethe's aversion to Newton's theory and experiments.

To some extent Goethe made good his claim to the scientific value of intuitive observation. He contributed to biology the doctrines of metamorphosis and homologous parts, views that gained greater significance later in connection with the theory of evolution. His concept of metamorphosis was that parts of an organism change, according to need, so as to assume other functions. Almost all vertebrates except man have two upper jaw bones. Goethe in 1786 had discovered that the upper human jaw shows traces of sutures, as if it had been formed from two bones. The doctrine of homologous parts is closely related. Goethe's intuitive observation here was to note that stamens, petals, sepals and some other parts of plants bear the same relation to the stem as do the leaves, so that they can all be regarded as forming a single class of appendages, and may have been created by metamorphosis. Wings, forelegs and arms are homologous. Goethe, seeing a broken sheep's skull on the beach, perceived it as the modification of several vertebra. Much later Helmholtz remarked that there is very little exact meaning to such a vague concept, for there is not much more to the doctrine of homologous parts—apart from metamorphosis and evolution—than the statement that plants have many lateral appendages of various forms. Yet Goethe, the poet, was never shaken in his belief that the fundamental scientific method is this intuitive observational insight.

Zur Farbenlehre consists of three parts. The first half of the first volume includes the description of many subjective phenomena of color and brightness, observations which before had neither been made so systematically nor set down so fully. They represent Goethe's useful contributions to the psychology of vision. The second part of the volume is devoted to a long unreasonable polemic against Newton, and also, incidentally, to Goethe's color theory, if the result of this intuitive method can be called a theory. The second volume is a full and detailed history of optics.

Let us see what were the *subjective phenomena* that Goethe described under the caption "Physiological Colors." To avoid circumlocution we use modern terms. Goethe particularized the phenomena of *irradiation*, the spreading of white upon black, and its application in practical life. He described light and dark *adaptation*, but not chromatic adaptation. He gave accounts of *positive after-images* and of the flight of colors, along with statements about the times of both of these phenomena. He stated conditions

for *negative after-images* of black and gray and colors, commenting on the nature of complementary hues and the complementary halo that is seen about the stimulus-object during fixation. He also noted the complementary after-effect of general exposure to dark or light or color. He suggested the effect of illumination of background upon positive and negative after-images, in a case where he viewed both images after exposure to a single stimulus. He described *contrast* effects of black and gray and colors. He discussed the coloring of shadows in relation to after-images and contrast. Finally he gave accounts of cases of *color blindness*. It is, of course, in such a marshalling of simple data that intuitive observation is at its best; thus we may note that the addition of Goethe's facts to Newton's gives us some correct information for almost every topic about color that is nowadays worth putting into an elementary textbook of psychology.

In Goethe's *theory of color*, on the other hand, observational intuition appears at its worst. Goethe was struck by the fact that all colors are darker than white. Thus it became immediately apparent to him that colors must be a mixture of dark with light. Noting that blue is darker than yellow, he established as his main thesis the principle that blue and yellow are two primary colors to which all the rest may be related. Furthermore, knowing only about the mixture of pigment colors, he pointed out that green is a mixture of blue and yellow. Red (*Purpur*) he described as a "condensation" of yellow, the way yellow looks when it is very dense, like the sun through a mist or smoke or the atmosphere at sunset. Although Goethe elaborated this theory with further observational and argumentative detail, he added nothing that simplifies or clarifies his view. The theory boils down to a two-color hypothesis based on yellow and blue. To illustrate the polarity of yellow and blue Goethe compiled the following list of comparable dichotomies:

PLUS	MINUS
Yellow	Blue
Brightness	Darkness
Light	Shadow
Warmth	Cold
Active	Passive
Force	Weakness
Repulsion	Attraction
Proximity	Distance
Acid	Alkali

Goethe's violent and unreasonable opposition to Newton was ostensibly based on his belief that Newton's theory contradicts observation. That white is not a mixture of colors was so obvious to Goethe that he was shocked at the absurdity of any experiment leading to an opposite conclusion; such a conclusion, he could tell in advance, must be based on fallacy. Goethe's behavior, nevertheless, needs more to explain it than his faith in observation and his mistrust of experiment. He had got interested in color quite early. Struck with the absurdity of Newton's view he borrowed a prism in order to test the theory, neglected the test, and then, when the owner wanted the prism back, held it up opposite a white wall to find, to his astonishment and delight, that the wall still looked white except at the boundaries of dark objects upon it. At once he was certain that Newton was wrong. So shocked was he when a physicist assured him that Newton's theory would indeed require the expanse of the wall to appear white through a prism, that he set to work at once to prove Newton wrong. Hence he kept the prism and performed various experiments of viewing through it small black objects on white grounds and small white objects on black grounds. Finding colors only at the boundaries and different colors at opposite boundaries, he concluded that both white and black yield colors on refraction, and thus arrived at his notion of polarity which we have just noted. Newton's experiment he never tried! Instead he published two brochures on the matter in 1791 and 1792, and then 1411 pages of discussion in 1810. Though Helmholtz tried to explain this strange behavior as a natural consequence of the poetic temperament, that reason can account only for Goethe's original distrust of Newton's theory. The intense drive, which closed Goethe's eyes to the logic so obvious to every physicist whom he consulted, and which led him into this huge undertaking in observation and publication, must have been founded upon the wounded pride of an irrepressible ego.

Even when he was known to be wrong in his main commitment Goethe was too important a man to be ignored. His sterile theory of colors was cited during the first half of the nineteenth century, while the fertile theory of Thomas Young remained unknown. It was, moreover, Goethe's careful observation of subjective phenomena that opened the way for Purkinje and Johannes Müller.

When Goethe was rebuffed by the physicists, he found a friend for his theory in Schopenhauer, who both accepted his views about

the polarity of colors and shared his dislike of Newton. Presently Schopenhauer evolved a somewhat different theory of his own, one which he published in 1816. He asserted that the complementaries—red-green, orange-blue, and yellow-violet—show the polarity of colors, and that red and green are the principal colors upon which the others are based. This theory was admittedly intuitive, and it had little influence upon scientific thought.

From Goethe to Helmholtz

While the excursions of a poet into science would seem almost preposterous in the twentieth century, Goethe's adventures with color were not so absurd in the Germany of the early nineteenth century. In the German culture of that day philosophy and literature, not science, held the center of the stage. Idealism dominated philosophy, and the poets Goethe and Schiller were half philosophers. In such an environment Herbart could write a *Psychologie als Wissenschaft* (1824–1825) founded upon experience but not, as he explicitly insisted, upon experiment. To the modern person outside of Germany it seems impossible that a *Wissenschaft* could be held to be non-experimental, but then *Wissenschaft* never has meant exactly *science*; it is a broader term for a field of systematic knowledge. Perhaps the lack of a German equivalent for the word *science* explains in part why intuitive empirical disciplines exist without the benefit of precise experimental control more readily in Germany than in America. At any rate, the more one knows of the times, the more one sees that Goethe did no violence to the *Zeitgeist* in addressing his own self-confident, egoistic insight to the problems of evolution and of color.

Goethe may thus be said to head a *phenomenological tradition*. His immediate successor was Purkinje, followed shortly by Johannes Müller. Perhaps all German psychologists—Fechner and Wundt, for instance—belong somewhat in this line, yet the great phenomenologist of the latter half of the century was Hering. Stumpf followed him, and finally Wertheimer brought phenomenology to the world as Gestalt psychology. The whole phenomenological tradition, from Goethe to Wertheimer, is characterized by a belief in the value of the direct observation of experience and a mistrust of the mediation of psychological fact by elaborate experimentation.

Thus experimentalism finds itself opposed to phenomenology. The two are not incompatible, for all recent phenomenologists have believed in experimentation and have experimented too. The tempers of the two are, nevertheless, opposite. Thus in Germany itself a growing experimentalism largely, though not entirely, replaced phenomenology. Johannes Müller, who began his career with some phenomenology of vision, was also an experimentalist from the start. Fechner was mostly an experimentalist in his founding of psychophysics, although his motivation was philosophical (he wanted to relate materialism to spiritualism by psychophysics), but he also published considerable phenomenological observation of scientific significance. Helmholtz, the greatest scientist to contribute importantly to experimental psychology, is representative of experimentalism at its best and purest. The differences between the visual researches of Hering and of Helmholtz are typical of the differences between phenomenology and experimentalism. Where phenomenology is egoistic, asserting the validity of individual observation and insight, experimentalism is diffident, mistrusting individual observation and relying upon controls, procedures without knowledge, and the other techniques that have been devised to achieve assurance in the face of the unreliability of human observation.

Nowhere is the changing balance between phenomenology and experimentalism better illustrated than in the history of visual sensation.

Johannes Evangelista Purkinje (1787–1869), an Austrian physiologist, represents the phenomenology of vision in its best and most effective form. He wrote in 1819 a first volume, *Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht*, and then in 1825 a second volume, *Neue Beiträge*. This second volume contains his appreciation of Goethe, the phenomenologist. After two paragraphs eulogizing *Zur Farbenlehre*, he dedicated his own work to Goethe: "Nehmen Sie, grosser Mann, diese treue Huldigung von einem Ihrer kleinsten, aber innigsten Verehrer." That, from the lesser to the greater man, but from the better to the poorer scientist!

Purkinje's observations of visual sensation "in its subjective aspect" supported and supplemented Goethe's. He described the phenomena that result from 'inadequate' stimulation of the retina by pressure on the eye-ball and by electric currents, thus supplying data for Johannes Müller's doctrine of specific nerve energies. He

examined the effect of dazzling light and its results in after-images and the flight of colors, the effects of dark adaptation, the facts about the blind spot, the relation of fixation to the size of the pupil, the phenomena of single and double vision with the two eyes, and the general facts of after-images and of the images of imagination and memory. All this is in his first volume. Although his second volume is somewhat more physiological, it includes phenomenological accounts of the changes in color sensitivity in indirect vision from 0° to 90° from the fovea, of true and apparent movement in the visual field and why the moving eye does not produce the perception of movement, and of the effect of strong light upon colors. His observations on indirect vision were potentially important, although as a matter of fact nothing much was made of this problem until Aubert mapped the color zones in 1857.

In the second volume (1825) there are three paragraphs that describe the change of hue and of relative brightness of the different colors as illumination increases from very faint light to normal vision. This observation, one among hundreds and not regarded as especially important by the observer himself, is the one that perpetuates Purkinje's name. Today every psychologist knows what is meant by the *Purkinje phenomenon*. So familiar is the term that the pronunciation of the proper name has even been anglicized. In general, however, phenomenology does not make for fame. The greatest discoveries are linked with hypotheses that go beneath the level of immediate phenomenological observation.

It was the growth of experimental physiology that forced experimentalism upon the study of the sense of vision. Even a hundred years after Goethe, to be sure, there were psychologists, like Titchener, who thought that trained observation is more important than carefully devised experiments; nevertheless the big generalizations that make up the body of scientific knowledge wait in general upon the complication of experimental technique. The most distinguished German physiologist of the early nineteenth century, Johannes Müller (1801-1858), began his professional career by attacking problems of vision. In 1826 he published a large book on the comparative physiology of vision, a book in which he considered the general problems of human and animal vision, and in which he also gave his first formulation of the famous doctrine of the specific energies of nerves. Later in the same year he published a small phenomenological book, *Ueber die phanta-*

stischen Gesichterscheinungen (with a quotation from Goethe printed before the preface).

There were half a dozen important texts in the field before Helmholtz. Treviranus published an elaborate comparative anatomy of the eye (1828) with exact data as to the formation of retinal images in a wide variety of animal eyes. Heermann had a text on visual perception and sensation (1835), and A. W. Volkmann published a general physiology of vision (1836). In 1838 came Johannes Müller's chapter on vision in his *Handbuch der Physiologie des Menschen*, a chapter that rounded out the information as of that time and cited Volkmann extensively. There were in addition Burow's physiology of the eye (1841), the book that considers the mechanisms of accommodation and convergence, and Szokalsky's study of color sensations (1842). After that came Volkmann's and Listing's general accounts of vision in Wagner's *Handwörterbuch der Physiologie* (1846, 1853). Helmholtz's researches began early in the 1850's.

Helmholtz (1821-1894), as we have just noted, represented experimentalism at its best. His career as a physiologist was launched by his measurement of the rate of the nervous impulse (1850), that preposterous fact which the intuitionists of his day protested could not be true, just as the intuitionist Goethe had denied Newton's fact that white is heterochromatic. Helmholtz accepted Müller's principle of specific nerve energies as a fundamental psychological law of paramount importance, extending it to account for different qualities within a single sense. Quite early (1852) he brought forth and championed Thomas Young's theory of color, developing it as a theory of three specific nerve energies. Then too he reviewed the literature as well as accomplishing a great deal of technical research in physiological optics. The outcome was the publication of the three volumes of his classical *Handbuch der physiologischen Optik* in 1856, 1860 and 1866. The value of this book is attested by its translation into English almost sixty years later (1924-1925)—in celebration of the centenary of Helmholtz's birth.

After Helmholtz

Because the great expansion of the psychophysiology of vision from Helmholtz to the present time prevents the subject from being traced as a whole, we must study—in the next chapters—the

separate histories of some of the principal problems. We may first, however, note briefly a few of the major trends.

(1) There has been a huge multiplication of fact and research. To the second edition of Helmholtz's *Handbuch* (1896) König added a bibliography of 7,833 titles, and no bibliography was considered any longer possible in the English edition of 1924-1925. Evidence of the eventual wide expansion of interest may be seen in the formation of the Optical Society of America and the many investigations published in its *Journal*. There has also been some support from American industry for research in psychophysiological optics.

(2) The phenomenological attitude in visual research was maintained by Ewald Hering (1834-1918). Hering and Helmholtz, representing phenomenology and experimentalism, were the two greatest names in visual science of the latter half of the century. Although later G. E. Müller came to hold Hering's attitude toward visual problems, phenomenology in the twentieth century has been lost in a mass of technical experimentation. It exists only in the problems of visual perception that the Gestalt psychologists consider, and they owe much of their orientation to Hering.

(3) The point of view toward problems of vision has thus shifted from the phenomenal to the physical. Hering, G. E. Müller and Titchener sought to study the attributes of immediate visual experience, and then to find out what dimensions of the stimulus are correlated with these attributes. Nowadays we consider first the dimensions of the stimulus, and then seek to discover what phenomenal consequences they yield. We used to inquire into the physical cause of hue; now we ask about the effects of monochromatic light. We used to be concerned with the criteria of visual depth; now we study the properties of binocular vision. Since we know that the correlations between stimulus and phenomenon are not simple (hue depends on more than wave-length, brightness on more than energy), this shift to classification in terms of the physical properties of stimulus sometimes leads to a radically new grouping of facts.

(4) Quantification has led to calibration. Many facts are so surely and so accurately established that norms for them can be used in the computation of other results. The luminosity curve for the brightness of spectral colors and the differences between cone vision and rod vision are facts which no one questions, and which

modern investigators like Hecht use as the basis for quantitative theorizing.

(5) In the late nineteenth century the chief interest in vision was centered in the theories of color, like the Young-Helmholtz theory, Hering's theory, Ladd-Franklin's theory. Almost dwindled away now, this interest has been supplanted by a concern with the experimental establishment of functional relations which show the dependence of a phenomenon upon its conditions in the stimulus. A graph or an equation is the law and takes the place of a theory.

(6) The stimulus has, in general, migrated from the external world to the retina. In the nineteenth century the stimulus was usually some variable object, like the papers on a color-mixer. In the present century the optical properties of the eye are so well understood that the nature of the proximal stimulus at the retina can be predicted and used as the independent variable in experimentation. Thus Troland took as a measure of stimulus intensity the photon, which is a unit of intensity at the retina after pupillary accommodation has been taken into account.

In the next chapters we shall consider the histories of specific problems of visual sensation and perception.

Notes

Before Newton

Here follow the more important names, dates and works mentioned or implied in this section of the text.

Alhazen is the Latin name of the Arab, Ibn al Haitham (ca. 965-ca. 1039).

Johann Kepler (1571-1630), *Ad vitellionem paralipomena*, 1604; *Dioptrice*, 1611.

Willebrord Snell (1591-1626) formulated the law of refraction in 1621. He did not publish it.

René Descartes (1596-1650), *La dioptrique*, published with *Discours de la méthode*, 1637; *Principia philosophiae*, 1644; *Le monde*, written ca. 1630, published posthumously in 1664.

Francesco Maria Grimaldi (1618-1663), *Physico-mathesis de lumine,*

coloribus, et iride, 1665 (posthumous).

Robert Hooke (1635-1703), *Micrographia*, 1665.

Ole Christensen Römer (1644-1710) determined the velocity of light in Paris in 1672-1676 and communicated his results to the Académie des Sciences in 1676.

Christiaan Huygens (1629-1695) communicated his researches to the Académie des Sciences in 1678 and published them in *Traité de la lumière*, 1690 (Eng. trans. 1912).

Edmé Mariotte (1620-1684) described the blind spot in 1668.

There was also a book on color by the distinguished scientist, Robert Boyle (1627-1691), *Experiments and Considerations Touching Colours*, 1664, a date which is two years before Newton bought the prism which

started his experiments. Goethe said (1810) that only Theophrastus (but perhaps he meant Aristotle) and Boyle had attempted before himself to describe and classify the phenomena of colors. Boyle, however, although presenting a great mass of casual information and opinion, seems to have had no great influence, presumably because he lacked a central idea that would give significance to his facts.

The histories of physics detail the history of optics and thus the beginnings of the psychology of vision. Excellent brief English accounts are E. Mach, *Principles of Physical Optics*, [1913], Eng. trans. 1926, Chaps. 4 and 7; F. Cajori, *History of Physics*, 2 ed., 1929, 83-97; and esp. A. Wolf, *A History of Science, Technology, and Philosophy in the 16th and 17th Centuries*, 1935, Chap. 11; *A History of Science, Technology, and Philosophy in the 18th Century*, 1939, Chap. 7. See also Thomas Young's lecture on the history of optics, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 1807, I, 472-483; W. Whewell, *History of the Inductive Sciences*, 1847, II, 371-425. On the ancients' views of light, see H. Crew, in F. Maurolycus, *Photisme de lumine*, Eng. trans., 1940, vii-xvii.

Newton

Sir Isaac Newton's *Opticks* was published in 1704; 2 ed., 1717, and, with additions and corrections, 1718; 3 ed., 1721; 4 ed. (posthumous), 1730; reprint, 1931.

Much has been written about Newton and his work. The classical biography is D. Brewster, *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 2 vols., 1855. See also L. T. More, *Isaac Newton*, 1934; the symposium of the History of Science Society, *Sir Isaac Newton: 1727-1927*, 1928; Cajori, *op. cit.*,

89-93; Wolf, *op. cit.*, 1935, 264-271; Mach, *op. cit.*, Chap. 6.

Porterfield

Some mention must be made here of William Porterfield, *A Treatise on the Eye, the Manner and Phaenomena of Vision*, 1759, 2 vols., 450 and 435 pp., respectively. This book was the standard and most complete work on the physiology and psychology of vision in the eighteenth century. Thomas Young frequently referred to it as summarizing the knowledge at that time and contributing new experiments. The author made, however, no new historically important discovery and urged no new significant view. The book punctuates the gap between Newton and Young, but was not crucial. The more important items of its psychophysiology can be abstracted as follows:

1. *Retinal image.* The optics of its formation are discussed. Kepler is cited. The problem of the inversion of the image is dismissed on the ground that the mind does not see the retina but concludes as to the nature of objects on the basis of all the information furnished it.

2. *Accommodation.* The "adaptation" of the eye to clear vision at different distances is due not to elongation of the eye-ball nor to change in "the figure of the crystalline," but to contraction of the ciliary ligament which moves the crystalline back and forth, being maximally contracted for near vision.

3. *Convergence.* The two eyes judge distance in terms of "the Angle that the Optic Axes make at that point of the Object to which the Eyes are directed."

4. *Perception of distance.* Six criteria are given: (a) the disposition of the eye (accommodation), (b) the angle (convergence), (c) the apparent size of objects, (d) the "force" or strength of color, (e) the confusion of small parts (acuity),

and (f) the occurrence of intermediate objects.

5. *Single field of binocular vision.* The phenomenon is not due (Porterfield thought incorrectly) to a "mixture" of the optic nerves, but rather to the fact that the mind localizes an object, seen double, in a single place in space. This result does not come about by custom and experience: Porterfield opposed Berkeley.

6. *Adaptation.* The eyes "accustom" themselves to light, darkness and color, although to be noticed the change must be considerable and sudden. The after-effect for light and darkness is sensitivity to the opposite illumination, an effect which may be due to change in the size of the pupil.

7. *Color.* Colors depend upon vibrations in the retina set up by light. This is the Newtonian theory, which Thomas Young took up.

Thomas Young

Sir Thomas Young's papers have been collected by George Peacock in the *Miscellaneous Works of the Late Thomas Young*, 3 vols., 1855. These first two volumes include the scientific memoirs. In 1802 and 1803 Young, appointed as Professor of Natural Philosophy at the Royal Institution, gave lectures which were published four years later in the large volumes of his *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 2 vols., 1807. These lectures are not included among the *Miscellaneous Works*. There is a detailed biography: G. Peacock, *Life of Thomas Young*, 1855. There is also an incomplete reprint of three papers on the wave theory: H. Crew, ed., *The Wave Theory of Light*, 1900, 45-78.

The early paper on accommodation is: Observations on vision, *Phil. Trans.* 83, 1793, 169-181; *Miscellaneous Works*, I, 1-11. The important later paper is: On the mecha-

nism of the eye (read Nov. 27, 1800), *Phil. Trans.*, 91, 1801, 23-88; *Miscellaneous Works*, I, 12-63. On the eighteenth century theories and researches on accommodation, see the citations in the second paper. While Young used the word *accommodation* in this sense, he more often spoke of the "focal length of the eye" or used some similar phrase. The text employs *accommodation* to avoid constant circumlocution.

There are four references to the color theory: (1) On the theory of light and colours (read Nov. 12, 1801), *Phil. Trans.*, 92, 1802, 20 f.; *Miscellaneous Works*, I, 146 f.; (2) An account of some cases of the production of colours not hitherto described (read July 1, 1802), *Phil. Trans.*, 92, 1802, 395; *Miscellaneous Works*, I, 176 f.; (3) *Lectures* (delivered 1802-1803), I, 1807, 440; (4) *Lectures*, II, 617. The reference to Newton's theory of retinal mediation of the vibrations is transcribed as of 1675 by T. Birch, *History of the Royal Society*, III, 1677, 262; it is also quoted in Young (1), *supra*.

On specific nerve energies, *vide supra*, pp. 68-74; also E. G. Boring, *A History of Experimental Psychology*, 1929, 77-94.

Goethe

Johann Wolfgang Goethe's chief work on color is *Zur Farbenlehre*, 2 vols. and plates, 1810. There is an Eng. trans. of the first part of the first volume by C. L. Eastlake, *Goethe's Theory of Colours*, 1840. The data as to how Goethe went to work, borrowed the prism, was rebuffed by physicists, etc., come from Goethe's "Confession" in *op. cit.*, II, 666-692. Goethe's two early papers on color were separate brochures, *Beyträge zur Optik*, I, 1791, II, 1792. These and *Zur Farbenlehre* can be found in various editions of his collected works.

For H. v. Helmholtz's analysis of

Goethe's scientific vision and obstinacy, see his lecture of 1853 On Goethe's scientific researches, reprinted in Eng. trans. in *Popular Lectures on Scientific Subjects*, 1873, 33-59.

Vol. I, pt. i, of *Zur Farbenlehre* consists of 920 consecutively numbered dicta. It is not easy without lengthy quotation to show how dictatorial these dicta are. Thus: "839. In dress we associate the character of the color with the character of the person. . . . 840. The female sex in youth is attached to rose-color and sea-green, in age to violet and dark-green. . . . 841. People of refinement have a disinclination to colors." Or this: "Lively nations, the French for instance, love intense colors, especially on the active side; sedate nations, like the English and Germans, wear straw-colored or leather-colored yellow accompanied with dark blue. Nations aiming at dignity of appearance, the Spaniards and Italians for instance, suffer the red color of their mantles to incline to the passive side."

Arthur Schopenhauer's (1788-1860) 93-page monograph on color is *Ueber das Sehn und die Farben*, 1816; later eds., 1854, 1870; Latin ed., 1830.

From Goethe to Helmholtz

A fuller general description of this period is to be found in E. G. Boring, *A History of Experimental Psychology*, 1929, 97-105, 293-295.

The books mentioned in the text are the following:

J. E. Purkinje, *Beobachtungen und Versuche zur Physiologie der Sinne*, 2 vols. The first is *Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht*, 1819; 2nd unaltered and more frequent ed., 1823. The second volume is *Neue Beiträge, etc.*, 1825, and contains the brief account of the "Purkinje phenomenon," pp. 108-110. On Purkinje, the man, *vide infra*.

Johannes Müller, *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Tiere nebst einem Versuch über die Bewegungen der Augen über den menschlichen Blick*, 1826; *Ueber die phantastischen Gesichtserrscheinungen*, 1826; *Handbuch der Physiologie des Menschen*, II, 1838, Bk. v, sect. i.

G. R. Treviranus, *Beiträge zur Anatomie und Physiologie der Sinneswerkzeuge des Menschen und der Thiere*, 1828.

Heermann, *Die Bildung der Gesichtsvorstellungen aus Gesichtsempfindungen*, 1835, [n.v.].

A. W. Volkmann, *Neue Beiträge zur Physiologie des Gesichtssinnes*, 1836; and in R. Wagner's *Handwörterbuch der Physiologie*, III, i, 1846, 264-351.

C. A. Burrow, *Beiträge zur Physiologie und Physik des menschlichen Auges*, 1841.

V. Szokalsky, *Ueber die Empfindungen der Farben in physiologischer und pathologischer Hinsicht*, 1842.

J. B. Listing, in R. Wagner's *Handwörterbuch der Physiologie*, IV, 1853, 451-504.

H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, 3 vols. (1856, 1860, 1866), 1867; 2 ed. (1885-1894), 1896; 3 ed., with editorial additions, 1909-1911; Eng. trans., with further additions, 1924-1925. See also E. G. Boring, *A History of Experimental Psychology*, 42 f., 45 f., 288-309.

For Purkinje's life and work, see O. V. Hykes, *The life of J. E. Purkyne*, *Osiris*, 2, 1936, 464-471; F. K. Studnicka, J. E. Purkinje's "physiology" and his services to science, *ibid.*, 2, 1936, 472-483; V. Robinson, Johannes Evangelista Purkinje, *Sci. Mon.*, 29, 1929, 217-229. Purkinje's scientific contributions to physiology were noteworthy; these papers on the phenomenology of vision are only the first publications of a man who had not yet attained recognition.

There is a story that Goethe, impressed by his first monograph, helped him to his first position, and it is certain that later personal acquaintance between the two reenforced a bond which at first had been purely scientific.

The text depicts Helmholtz and Goethe as at opposite scientific poles. That Helmholtz also had this view is indicated by his criticism of Goethe, cited above.

After Helmholtz

E. Hering wrote extensively on the problems of visual sensation, but his two most important books are *Zur Lehre vom Lichtsinne*, 1872-1874; reprinted, 1878, and *Grundzüge der Lehre vom Lichtsinn*, 1920 (posthumous).

G. E. Müller's chief contribution to the psychology of vision is *Zur Psychophysik der Gesichtsempfindungen*, *Z. Psychol.*, 10, 1896, 1-82, 321-413; 14, 1897, 1-76, 161-196. In 1930, when he was eighty years old, Müller published the two large volumes of *Ueber die Farbenemp-*

findungen: psychophysische Untersuchungen; but he wrote from the point of view of his own generation, and the book made little impression upon the changed world of psychophysiological optics.

For an example of modern visual psychophysiology as classified in accordance with the dimensions of the stimulus and as consisting of statements of functional dependence, see L. T. Troland, *The Principles of Psychophysiology*, II, *Sensation*, 1930, 51-205. On the photon, see *ibid.*, 62 f., 110 f.

The reason that phenomenology perpetually loses the battle to experimentalism is that the development of science is always toward complication and a subsequent simplification by abstraction. Goethe's or Hering's insightful apprehension of the visual world might yield a conviction that there are four principal colors, but it could never show that relative intensive sensitivity is minimal at an intermediate intensity and increases for greater and lesser intensities, i.e., that the Weber function is so-and-so and not as Fechner thought.

Chapter 4

COLOR AND ITS STIMULUS

IN this and the next four chapters we shall consider the histories of the more important problems of visual sensation, distinguishing the problems as they were understood in the early part of the present century. At that time phenomenal experience was believed to be the primary subject-matter of psychology, and physiology a secondary consideration useful only later for the purpose of explanation. For this reason classifications were based upon phenomenal characteristics, with only subordinate consideration for the dependence of the sensations upon their stimuli. In this attempt the psychologists were not, it must be admitted, successful; no laws of visual sensations were ever formulated solely in terms of consciousness without reference to the stimuli or the nervous system. Nevertheless, phenomenal color and brightness remained for a long time the important terms of visual sensation; and to determine the laws of dependence of these two aspects of color upon the different dimensions of variation of the visual stimulus was the aim of most research.

Color and its Dependence on the Stimulus

We have seen in the last chapter how an understanding of the nature of color suddenly emerged from Newton's discovery that different colors are immutably associated with different degrees of the refrangibility of light, and that white is, in this sense, not a color, but the consequence of a mixture of lights of many different refrangibilities. By a false analogy with tones Newton, however, supposed that there were but seven colors, a view which seems to imply that he believed in but seven refrangibilities. Mach thought later that Newton must have known that the seven colors are correlated with an infinitude of degrees of refraction—as indeed the seven musical notes are related to an infinitude of vibration frequencies even within the single octave. At any rate, Newton did

not express himself clearly, and the matter went over for a century.

In 1801, however, Thomas Young, as we have also seen, realized that there must be an infinitude of kinds of light and thus, as he thought, of colors too. He conceived the necessity, therefore, of reducing the perception of colors to a few (three) nervous processes. It was later, about 1815, that Fraunhofer described accurately the spectral lines which now bear his name. He measured the position of more than five hundred of these lines, naming the most important ones by the letters A to G. Since the refraction of such lines can be accurately determined, he used them to study the variation of refraction—knowledge essential in the manufacture of achromatic lenses. After Fraunhofer it was not possible to think of the kinds of light as less than an 'infinitude'; yet it was still not clear how many seen colors correspond with the light continuum that is the spectrum.

There are really three different ways of answering this question about the number of colors, although the three were not clearly distinguished until much later. (1) One may ask how many distinguishably different colors lie within the limits of the spectrum. Here is the problem of the number of *just noticeable color differences*. (2) One can, on the other hand, equally well inquire as to how many pure colors can be seen in the spectrum. This question implies the existence of *pure* or *elementary colors* (e.g., red, yellow), and intermediate colors (e.g., orange) that are mixtures of the pure colors. (3) Finally, one may argue as to how many elementary physiological processes in the retina or optic nerve are necessary for the mediation of all the discriminably different colors. This is the problem of *physiological elements*. The confusion of these three problems results from the supposition, taken for granted by the early investigators, that phenomenally pure colors would be the correlates of elementary physiological processes and also the most noticeably different colors in the spectrum.

Newton thought, as we have seen, that there were just seven pure colors, corresponding to seven kinds of light. Although he knew about the intermediary colors, as his laws of color mixture show, he also knew that the prismatic spectrum consists of manifold overlapping images of the aperture through which the light comes, and thus it was natural for him to suppose that the intermediate colors are due to mixed light.

W. H. Wollaston in 1802 was the first person to observe and

describe the black absorption lines in the solar spectrum. He measured the position of seven of these lines, and subscribed to a belief that the three most prominent divide the spectrum into four fundamental colors: red, yellowish green, blue and violet. He seems to have thought of the black lines as breaks between the colors, although it is strange that "yellowish green," a name which indicates a mixture, should have seemed to him a simple color—for Wollaston knew that the spectrum presents an infinitude of refrangibilities, and he was discussing perceived color, not light.

It was in 1802 also that Thomas Young argued for three primary physiological processes. Wollaston thought of his theory of four primaries as contradicting Young's theory of three; nor should we expect at that time to find the physiological analysis of colors distinguished from the phenomenological analysis. Even a century later there were those who believed that Young's view of three physiological primaries must be incompatible with the introspective observation of four fundamental hues. In fact, most psychophysiological theories assume that the mind perceives only what the nervous system brings either to it or to its part of the brain, and thus that there could not be four primaries in perception if there are only three in the optic nerve. (Cf. pp. 69-72, 206-209.)

Goethe's phenomenological theory of color appeared in 1810. It is, according as one interprets it, a two-color (yellow-blue) or a four-color (red-yellow-green-blue) theory. It posits only two elementary colors, yellow and blue, from which the other two colors, red and green, are derived. Thus it has come to be regarded as the forerunner of the phenomenological four-color theories, of which Hering's is the outstanding example. Goethe's phenomenological four, at any rate, helped to obscure Young's physiological three.

About 1815, when Fraunhofer rediscovered the black spectral lines that Wollaston had first observed, he named nine prominent, well-separated lines by the first letters of the alphabet, charting the positions of 565 others. His determinations meant that there are more than five hundred kinds of light for each of which the refrangibility is specific and accurately measurable, with presumably many more degrees of refrangibility in between. Thus it became necessary to admit that the number of perceptible colors is less than the number of differently refrangible lights.

In the latter half of the century the problem of physiological primaries became the central problem of color theory. Helmholtz

(1852), in reviving Young's theory, threw the weight of his opinion toward three physiological processes, a principle accepted by most theorizing today as fundamental. Hering (1874), on the contrary, believing that the number of physiological processes must be the same as the phenomenally primitive colors, argued for four, that is to say, two pairs, yellow-blue and red-green. Ladd-Franklin (1893), also for phenomenological reasons, hypostasized four retinal molecules, existing at two stages of chemical decomposition, to explain the four principal colors. After Helmholtz, however, the theories of the perception of color and of brightness became related and many other special phenomena were considered as bearing upon them. As a consequence we must defer this consideration until a later chapter (pp. 210-214).

The problem of the phenomenally simple colors achieved importance because Hering based his theory on the distinction. Hering knew, for instance, that pure red is not spectral, that the best red in the spectrum is somewhat yellowish, and he determined (1885) the proportions of spectral red and spectral violet lights that would have to be mixed to give pure red. Donders (1884) had already placed pure yellow at 582 m μ , a mean of determinations for 111 eyes that ranged from 572 to 594 m μ . Hess (1889) determined the wave-lengths for the other pure spectral colors: yellow, 575 m μ ; green, 495 m μ ; blue, 471 m μ . The standard determinations were made much later by Westphal (1910): red, non-spectral; yellow, 574.5 m μ ; green, 505.5 m μ ; blue, 478.5 m μ . These are average values, subject to considerable individual variation. The capacity of observers, however, to make such accurate 'absolute' judgments of the purity of the four crucial colors has been taken to mean that these colors have a special place in the color system and that special account should be taken of them in any complete physiological theory.

The problem of the number of discriminable color differences did not arise until the psychophysical era when Fechner had made differential sensitivity the fundamental problem of mental measurement. Actually the number of colors could not be determined by inspection of the spectrum; psychophysical methods were required for it. Mandelstamm (1867), the first person to experiment upon differential thresholds for color, was followed in 1872 by Dobrowolsky who made determinations which were cited as standard for many years. Before the end of the century there were at

least four other determinations. The data of L. A. Jones (1917) are standard today. All the determinations have shown that discriminability of hue is a variable function of the wave-length of light, that differential sensitivity is poor at the extremes of the spectrum, and that there are several maxima in the middle region. Jones found three maxima at 588, 507 and 489 m μ , where the differential limen reaches a minimum of 1 m μ .

Their interest aroused by the charting of the differential limen throughout the course of the spectrum, psychologists immediately proceeded to count the total number of liminal steps throughout the spectral range. This number of sensations, which are just discriminably different under optimal conditions of observation, came to be regarded as the total number of sensations. Von Kries (1882) computed that the results which are usually accepted from Dobrowolsky's work imply the existence of 208 discriminably different spectral hues. The limens found by König and Dieterici (1884) yield a count of 235. Külpe (1893), however, came nearer the truth in his estimate of 150. The high values of the early investigators undoubtedly include some discriminations due to differences in brightness, since in some parts of the spectrum the change in brightness is more rapid than the change in hue. Jones' figures of 1917 show 128 discriminably different hues for the spectral scale.

That the concept of a simple color had become ambiguous is obvious. It is one thing to say that there are seven different colors or four pure colors or three elementary colors, and quite another to say that there are 150 discriminably different colors. Out of this discrepancy there arose the question as to whether the colors that lie between the principal or pure colors are themselves simple or compound. Is an orange from homogeneous light a psychological mixture of red and yellow, or is it psychologically as elementary as red and yellow? There was a long controversy on this matter. Külpe (1893), Titchener (1896) and Ebbinghaus (1897) held that the intermediate colors are as simple and elementary as the four principal colors (*Hauptfarben*), which form a frame of reference to which the others are referred when arranged in order according to their similarities. Mrs. Ladd-Franklin (1893) and later Brentano (1907), on the other hand, were among the more articulate sponsors of the complexity of the intermediate colors. Bentley (1903) argued that an orange is similar to red and yel-

low but not composed of red and yellow. Much later, at a meeting of the American Psychological Association, Ladd-Franklin appealed to the consensus of expert opinion by exhibiting disks and asking for the judgments of psychologists. The problem was never settled. It simply disappeared. From the perspective of the present it is clear that the proposition was never clearly formulated. Since simplicity and complexity were supposed to be immediately apparent to introspection, no rigid criteria for either concept were ever put forth so that the issue could be determined, or at least be given sense with reference to definitions of *analysis*, *complexity* and *simplicity*.

An even more famous controversy of this kind had its inception in Brentano's contention (1907) that green is phenomenally a mixture of yellow and blue. Yellow and blue *light* do not give green when mixed; Helmholtz had already explained why yellow and blue *pigments*, because of their physical properties, actually do give off, when mixed, a predominance of green light. Both Goethe and Sir David Brewster, however, had held that the mixture of yellow and blue is immediately obvious in a green—a view which Brentano defended, supported later by E. B. Holt (1912). This special problem has gone the way of the more general problem without solution. With the essential concepts undefined, no *experimentum crucis* could be arranged. The author was once present in a group of psychologists which included Titchener and Holt when this question of the complexity of green came up for discussion. These two men flatly disagreed as to the evidence of introspection, and there was nothing that anyone could propose to bring about agreement.

In nearly all of the many researches upon color, from Helmholtz to the present time, it has been assumed that for a given eye the hue is determined when the wave-length is specified. That generality, however, has to be modified in accordance with the principles of the Bezold-Brücke phenomenon. When illumination is diminished, the spectral hues, if wave-length is kept constant, tend to shift away from yellow or blue, and toward red or green. Small wonder, then, that the wave-length correlates of hue, for the principal colors and for the number of discriminable colors, vary, since they have been determined without strict control of the intensity of the spectral light.

Helmholtz noted incidentally that hue alters with intensity when

wave-length is constant. Bezold in 1873 described the phenomenon but briefly, since his chief interest lay in the determination of the primary colors (*Grundfarben*) by noting which hues remain when the spectrum is darkened. At low intensities of the spectrum it is obvious that yellow ordinarily disappears. Brücke a little later (1878) made a thorough study of the phenomenon for the same purpose. Both these studies, by emphasizing the fact that there can be no very dark yellow, told against yellow as physiologically elementary. The best modern determination of that variation of hue which is the Bezold-Brücke phenomenon is Purdy's (1929).

Brightness and Intensity

Newton, as we have seen (pp. 102-107) identified whiteness with intensity. The gray powder, rubbed on the floor of his chamber and lying in the direct sunlight, looked whiter than the white paper in the shadow near it, provided the two were viewed at such a distance as to render impossible their identification as objects. By adjusting the illumination the two could be equated. Newton was thus entitled to group together in his laws of color mixture ✓ "Whiteness and all grey Colours between white and black," for he had shown that position in the series black-gray-white depends on the intensity of the light, and that all parts of this series are alike in respect of dependence upon the mixture of colors.

The first effect of Newton's analysis of white light into colored light was to diminish the interest in white for those who were concerned with the account of the nature of visual experience. It soon became obvious, however, that, since a great deal of visual experience is colorless, there are laws to be worked out for the black-gray-white series. Fechner established a general interest in intensity, including the visual intensity which Newton had identified with the series of grays. We find Helmholtz (1860) talking separately about light sensations and color sensations. We find Hering (1874) talking about a light sense and a color sense, and nearly all of his *Zur Lehre vom Lichtsinne* was, as its title implies, about the light sense. That black is a sensation, not the absence of sensation, was plain to Helmholtz; Hering too was at pains to establish the positive sensory nature of black, since black in his theory is functionally symmetrical to white. The status of black, indeed, was crucial. If black is nothing, then white is only a mix-

ture—that is the verdict of Newton and of modern physics. But if black is a sensation, as introspection seems to assert—if there can be even such a thing as an intense black—then the light sense, the achromatic visual sensations, possess a claim for independent psychological status.

The very arguments, however, that established the blacks, grays and whites as a qualitative series, told against the status of visual intensity as an independent attribute. If the grays form an intensive series, like faint-medium-strong, then black must be zero intensity; it must be a visual silence. On the other hand, if the grays constitute a qualitative series, like red-orange-yellow, then what becomes of intensity? Do visual sensations have no intensity? ✓ Of this matter common sense could not judge. So much seemed obvious, however: (a) that one perceives intensity in perceiving the degree of illumination, (b) that an extreme black is as positive an experience as an extreme white or a color, and that black may even appear intense, and (c) that these two statements are contradictory.

Though Helmholtz made no attempt to solve this problem, Hering met it by the denial of intensity (in the usual meaning of the word) to visual sensations, a view which was accepted by Külpe and others. To avoid the apparent absurdity that a class of sensations should exist without any degree of intensity whatsoever, G. E. Müller (1897) proposed that visual intensity exists, but is not recognized because it is constant and never varies. Müller had modified Hering's theory by suggesting that black and white are given respectively by the action of antagonistic retinal processes, and that added to them there is a constant gray, generated in the brain, which is seen alone when the black and white processes are in exact equilibrium (pp. 212–214). As a consequence, if intensity were to be distinguished from the quality gray, it would be necessary for intensity and quality to vary independently. Müller argued, however, that a gray could increase in intensity without change of quality only if the amount of the black and white components in it can be increased together. Since there is no way of increasing both of two antagonistic processes simultaneously, and since the brain gray is constant, the intensity, although extant, cannot be recognized. Modern science, of course, would not countenance the argument that a necessarily unobservable datum can nevertheless exist, but forty years ago such an assumption was

not absurd. Titchener rejected it, not because it was unreasonable, but because it was a matter of physiological speculation rather than a direct datum of introspection (1908).

Titchener seems at that time (1910) to have believed, without much evidence, that intensity and the quality of the gray series exist and vary together from black to white. He even put Gates (1915) on to determining the differential thresholds for intensity and quality for the same stimulus variables (different illuminations of a white surface) in hopes that different limens for the two attributes would be found for the same illumination, thus showing a change in one without a change in the other. The results, however, were inconclusive.

Then Stumpf (1917) suggested that intensity is found to increase with any excitation of the dark-adapted eye; in other words, the zero-point is the gray of dark adaptation, and a change to a well-saturated color or to white is an increase in intensity, whereas qualitative change is represented by the deviation among the colors. After that Dimmick found experimentally (1920) that absolute thresholds for black and for white can be determined in the middle of the black-gray-white series, so that this series may be regarded as having a zero at middle gray with increasing black in one direction and increasing white in the other, just as the blue-gray-yellow line of saturations lying between these two complementaries has a zero in its middle. In the series of grays 'less white' does not mean 'more black,' nor conversely.

Thus Titchener (1923) was able to propose as a new solution that Müller's constant gray is the minimum (arbitrary zero) of the visual system, and that all lines from gray to white, black, red, orange, yellow, or any other hue represent intensive increases at the same time that they show qualitative changes. If gray could be taken out of the color solid (pyramid), Titchener said, then the figure would become a hollow surface oriented with respect to the six principal colors—black, white, red, yellow, green and blue. The center could then be thought of as zero-intensity, and intensive lines could be imagined drawn radially from it in every direction within the solid, like a pin-cushion with pins on top, underneath and all around. This view almost amounts to a resolution of both saturation and brightness into intensity, except that Titchener clung to his belief in a covariant quality which appears because the constant gray is added to the intensive series.

After Titchener the problem evaporated. The analytical phenomenologists lost faith in introspection as a means for determining the nature of the sensory attributes, and more technical methods came into use. Most psychologists, surrendering phenomenology and coming nearer to Müller's point of view, regarded intensity simply as the correlate of degree of excitation. For a three-color theory the intensity of a white would be the sum of the intensities derived from the three excitatory processes. In a four-color theory, an orange would depend for hue on the relative intensities of red and yellow excitatory processes, and for saturation on the total intensity of the two processes.

While all this was going on, the German phenomenologists were coming to a different solution of the problem by turning their attention from the simpler problems of sensation to the broader field of perception. Katz (1911), working in Müller's laboratory, brought out the distinction between surface colors and film colors, along with the fact that surface colors designate an object and tend to remain constant in the proper character for the object in spite of changing illumination. The observer, Katz showed, may also be aware of the intensity (or the color) of the illumination; in short, he may perceive an object of one brightness and hue illuminated by another intensity and hue. This dual perception comes about when there are sufficient additional cues to give the observer object-knowledge and to throw him into the attitude which helps to maintain constancy of perceived color. When, however, he loses these cues by viewing the object through a small hole ("reduction screen") so that the perceptual datum is no longer an object or a surface but only a patch of film-color, then the separation of constant color and intensity of illumination fails and the perception is unitary. Seen through a reduction screen, the illuminated black lies above the shaded white in the series of grays, as Newton had found when he destroyed the object-character of his gray and white by viewing them at a distance. Bühler (1922) achieved a very convincing validation for this kind of perceptual analysis.

The phenomenologists did not, of course, bring intensity back to vision as an attribute of sensation. They had little use for attributive analysis. They showed, nevertheless, why it was never possible for most psychologists to accept Hering's dictum that there is no visual intensity. Intensity of illumination is actually

perceived, and so are blacks and whites. It is possible to see a shaded white and an illumined black, in which the shade is darker than the illumination and the white lighter than the black. Such a double perception depends on the attitudes and knowledge of the observer and other factors which enter into perceptual constancy. The attributive duality of quality and intensity that Titchener tried to save lies really, not at the sensory level, but at the perceptual level—the level concerned less with immediate sensory experience than with the organism's awareness of its environment and its adjustment to it.

Weber's Law

While the phenomenologists were fussing over the question as to whether visual sensations have any intensity, the psychophysicists were busy measuring visual intensity and deciding whether Weber's law of intensive discrimination applies to vision. Weber's law by its essential nature implies a quantitative scale that has zero at one end, and that is irreversible in respect of 'more' or 'less.' Such a statement is not true of a quantitative scale like the oranges, where what is called 'more yellow' could equally well be called 'less red.' For the grays it was not doubted that the direction of white is the direction of 'more,' and that the zero end of the series must be in the blacks. It was, at any rate, easily established that the differential threshold increases as one proceeds from black to white or from less illumination to more, so that the essential principle of Weber's law can be seen to apply. Even casual observation serves to show that one candle added to two makes more change in the illumination than one candle added to ten. Thus it was natural enough, with the great interest that Fechner had aroused in Weber's law, that the psychophysicists should take the intensity of visual sensation for granted and inquire into the validity of the law.

The initial research on differential sensitivity to brightness, however, antedates Weber's law. Bouguer in 1760 was the first to perform the shadow experiment. Two candles project shadows of a rod upon a white screen. One candle is moved away from the screen until the shadow that it projects is only just noticeable against the background of the screen which is illuminated by both candles. Since the difference between the illumination of the

shadow and the illumination of its surroundings is, of course, the illumination given by the more distant candle that projects the shadow, it is easy thus to express the just noticeable difference as the ratio between these two illuminations. Bouguer discovered that this ratio is approximately constant for any pair of distances at which the two candles are adjusted, and his observation set the ratio at $\frac{1}{64}$; that is to say, the shadow was just noticeable when the far candle was eight times as far from the screen as the near

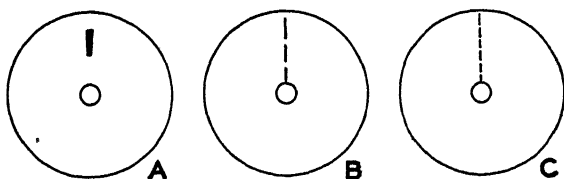


FIG. 9. MASSON DISKS

A. As used by Masson (1845). The sector as drawn is 6° and would make a ring that differed from its background by $\frac{1}{60}$.

B. As used by Helmholtz (1860). The three radial lines would, as drawn here, give Weber fractions of about $\frac{1}{40}$, $\frac{1}{100}$ and $\frac{1}{60}$. The outside ring would be invisible; the inside ring should be clear under most circumstances.

C. As recommended by Titchener (1901). The disk has more rings, more finely scaled.

candle. He concluded: "the sensibility of the eye is independent of the intensity of the light," thus anticipating Weber's law by seventy-four years.

Fechner (1858), repeating this experiment with Volkmann, made the fraction only $\frac{1}{100}$. He also described the cloud experiment, in which the observer finds a just noticeable difference between a bit of cloud and its background and then observes it through various densities of gray glass to find that what is just noticeable at one intensity is likewise just noticeable at another. With a fixed ratio for the stimuli the differences in sensation remained the same. The astronomer Arago had also repeated the shadow experiment (1850), finding under optimal conditions, which involved a moving shadow, a fraction of only $\frac{1}{133}$.

Another experiment had been made by Masson in 1845 with the disk that is named after him. (See Fig. 9.) Helmholtz later designed this disk with an interrupted black radial line. The parts of this line, mixing with the white of the disk, darken the field slightly.

The effect is greatest near the center, where the width of the black line is greatest in relation to the circumference with which it is mixed. If the conditions are right, the observer finds the outer ring invisible, and some inner ring that is just visible. With the

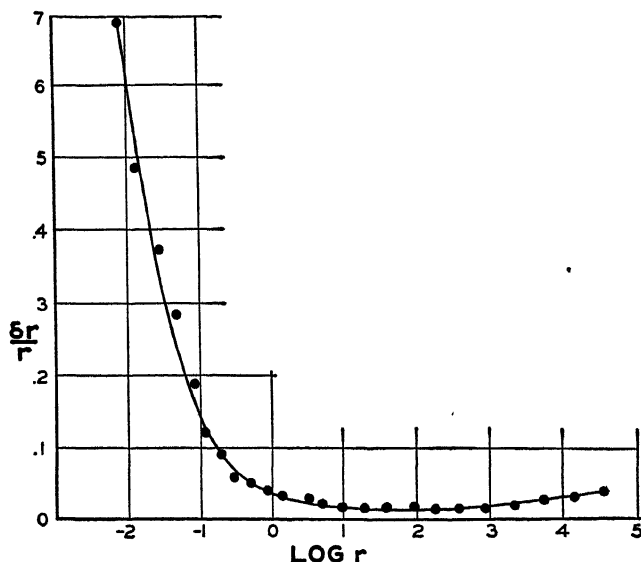


FIG. 10. KÖNIG'S DATA FOR DIFFERENTIAL SENSITIVITY TO BRIGHTNESS
(1888)

The graph is plotted from the data for König's eye for just noticeable differences, δr , for intensity of white light, r . The values of r run from 0.0082 to 34580. meter-candles and are plotted against $\log r$ in order to get them within the compass of a graph. Weber's law requires a horizontal straight line and is approximated between 1 and 3 for $\log r$, and roughly between 0 and 5. It breaks down entirely between -2 and 0 , and we now know that the slight rise between 3 and 5 is to be expected with many different kinds of data.

disk of Fig. 9A Masson found that sensitivity varied from $\frac{1}{50}$ to $\frac{1}{120}$ according to illumination and other conditions of visibility. Helmholtz (1860) "on bright summer days near a window" used the disk of Fig. 9B to get, "for a single instant" on shifting his gaze to the ring in question, a fraction of only $\frac{1}{167}$. More consistently he could see at the window the ring for which the ratio was $\frac{1}{133}$, or in the interior of the room the ring for which the ratio was

$\frac{1}{117}$. It was plain that Weber's law does not hold exactly, since difference in illumination alters the ratio.

Aubert in 1865 showed how great the variability in the ratio can be, when he obtained values from $\frac{1}{3}$ at low intensities to $\frac{1}{146}$ at high intensities. Much later (1888) König and Brodhun undertook a very thorough study for white light and various monochromatic lights, in which the average ratios vary from a little more than $\frac{1}{2}$ for minimal intensities to about $\frac{1}{60}$, a minimum fraction that occurs at less than the maximal intensity. For greater intensities the ratio increases slightly again. (See Fig. 10.) In general these data have been shown to agree surprisingly well with Aubert's and also with the recent investigation by Blanchard (1918). The shape of this function has led to the common statement that Weber's law does not hold for the extremes of intensity.

The most recent view, however, is that Weber's law does not hold at all: the middle portion of the function which, being horizontal, approximates Weber's law, is after all a consistent part of the total curve, which in no way could be said to approximate horizontal linearity.

Color Mixture

The facts of color mixture assumed importance in the 1850's because Helmholtz and Maxwell made them the basis for the theory of color vision.

As we have seen (pp. 103-106), Newton established the fundamental principles of color mixture—that every color has a complementary, with which, when mixed in the right proportions, it gives white (gray); that the mixture of other colors gives an intermediate; that the intermediate is poorer in saturation the farther the mixed colors are separated in the color series. As a schema for determining mixtures Newton constructed a color circle (Fig. 11). The seven colors are arranged about this circle subtending distances proportional to the musical intervals of the octave. The letters A . . . G are analogous to the musical notes of the octave. Newton said that the non-spectral purples lie at the end of the violets and the beginning of the reds, a remark which shows the confusion in his thought as to whether there were only seven colors or more than seven. White (gray) lies at the center, O, from which the radii measure the 'strength' of the colors. Newton thought of the pure spectral colors as concentrated at P, Q, R, . . .

If a red is to be mixed with a green, a circle proportional to the amount of red should be drawn at *p*, and another circle proportional to the amount of green at *s*; then the center of gravity of the two circles on the line *ps* represents the color of the mixture.

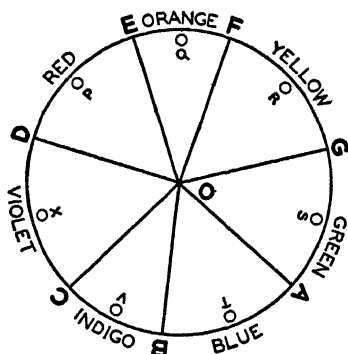


FIG. 11. NEWTON'S COLOR CIRCLE (1704)

The letters, A . . . G, correspond to the notes of the musical octave, and the seven colors are proportional to the seven intervals in the octave. The purples near D are not in the spectrum. The center, O, is white or gray. In mixture the amounts of the different components *are represented by circles of proportionate sizes at *p* . . . *x* respectively, and the resultant is supposed to lie at the center of gravity of the set of components.

some faint anonymous Colour. For I could never yet by mixing only two primary Colours produce a perfect white. Whether it may be compounded of a mixture of three taken at equal distances in the circumference I do not know, but of four or five I do not much question that it may." And there Newton left the matter.

With these remarks of Newton's in mind, Helmholtz in 1852 undertook to test the laws of color mixture, thinking that he could thus establish a basis for a physiological theory of color along the lines suggested by Thomas Young. He arranged to view through a telescope two complete spectra, at right angles to each other and superposed one upon the other. Such an arrangement gives a

In this way the schema summarizes both of Newton's propositions: the more remote the colors mixed, the weaker (nearer white) the resultant mixture; and, when the colors are maximally remote (complementaries, opposites in the circle), they may be "weaken'd till they cease and the Mixture becomes white or grey."

Newton conceived of this diagram as "accurate enough for practice, though not mathematically accurate." He was, in fact, doubtful about getting an exact white from two complementaries. He said: "If only two of the primary Colours which in the circle are opposite to one another be mixed in equal proportion, the point [for the mixture] shall fall upon the center O, and yet the Colour compounded of those two shall not be perfectly white, but

field which includes every possible paired mixture of all monochromatic lights, and, except for the fact that relative intensities are not controlled, it might have been expected to show a plot of the complementaries as a gray curve in the field. Helmholtz varied the intensity and, by removing his eye some distance from the eye-piece of the telescope, was able to limit inspection to a very small bit of the entire field. In this fashion he observed that a yellow and an indigo, both exactly localized with respect to the Fraunhofer lines, give a white, although all the other dual mixtures that he examined seemed tinged with color.

Although this experiment failed to establish the law of complementaries, the discovery that yellow and indigo mix to make white brought up the question as to why the mixture of yellow and blue paints makes green, a fact which had been known to painters for centuries and which was an outstanding example of the inexactness of Newton's laws, for Newton had not distinguished between mixtures of pigments and of lights. In this paper Helmholtz gave for the first time the correct reason why yellow and blue paints mix to give green. He noted that, in the mixture, most of the light penetrates a little way below the surface passing through particles of each of the pigments both before and after reflection. The yellow particles let through red, yellow and green light, whereas the blue particles let through green, blue and violet light, with the result that the blue particles eliminate the red and yellow from the yellow particles and leave the green, and the yellow particles eliminate the blue and violet from the blue particles and leave the green. Since the green is the only light that will pass through both kinds of particles, there is a great excess of green, diluted by whatever white is produced by the small amount of yellow and blue that finally get reflected. This conclusion led Helmholtz to lay down the general rule that the laws of color mixture cannot be demonstrated by pigments but only by lights, a rule that for the first time made possible the establishment of Newton's laws with exactitude. In his first paper, however, Helmholtz found only this one pair of complementaries, yellow and indigo.

The next year, 1853, Grassmann pointed out that Helmholtz's data implied the existence of exact complementaries for all colors, even though Helmholtz himself had experimentally established only one pair, yellow and indigo. Grassmann showed that Helmholtz had gotten mixtures according to the principles expressed

in Newton's color circle, and, that although Helmholtz did not ordinarily get exact whites, still his results indicated the direction of change in the mixture which would have been necessary to make the mixture pass through white. Grassmann posited as a law this: "that, if one of two mixed lights is steadily changed while the other remains unchanged, the impression of the mixture itself is also steadily changed." Thus, without any new experiments, he could apply a principle of limits to Helmholtz's data to establish Newton's law of complementaries.

Helmholtz, accepting Grassmann's criticism, in 1855 worked out the first table of exact complementaries (see also Fig. 12):

Color	Wave-length	Complementary	
		Color	Wave-length
Red	2425	Green blue	1818
Orange	2244	Blue	1809
Gold yellow	2162	Blue	1793
Gold yellow	2120	Blue	1781
Yellow	2095	Indigo blue	1716
Yellow	2085	Indigo blue	1706
Green yellow	2082	Violet	from 1600 on

The figures for wave-lengths are in millionths of a Paris inch. The millimicron as a unit came into Germany with the metric system a few years later. In the second edition of his *Optik* (1896) Helmholtz could show that these original determinations of his were very close to the later and supposedly more accurate ones by von Frey and von Kries (1881) and by König and Dieterici (1887). In fact, there is little difference in these results from the most recent figures founded by Sinden (1923). Differences between observers are greater than any error which there may have been in Helmholtz's measurements of 1855.

Various authors have stated different sets of the laws of color mixture. In general, however, there is agreement that the important laws are the two of Newton's: (1) the law of complementaries and (2) the law of intermediates. What is sometimes called the third law is the principle that (3) colors enter into composition no matter how they are composed. This law is due to Grassmann (1853): "Two colors, each of which has a constant color tone [hue], a constant color intensity [brightness] and a constant intensity of intermixed white [saturation], also give constant mixed colors, no matter of what homogeneous colors they are composed." Helmholtz said, less generally, "Colors that look alike

mix to give mixtures that look like them." Titchener said, "The mixture of mixtures that match will match either of the original mixtures." The law is important since the stability of the world of

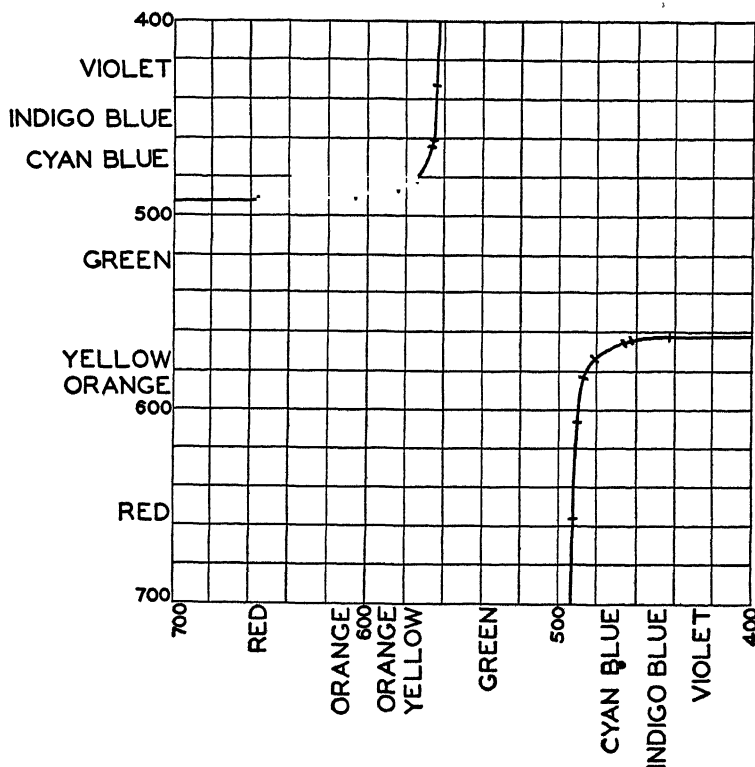


FIG. 12. HELMHOLTZ'S GRAPH OF COMPLEMENTARY COLORS (1855)

The curves show the relation between pairs of complementary spectral colors. The scales are in millimicrons, the later unit of wave-length. The complementaries of the greens are the purples which, being non-spectral, do not appear on the chart.

colored objects depends upon it, although its validity is, of course, limited by the Purkinje phenomenon.

Grassmann also laid down what might have been called a fourth law of color mixture: (4) "The total light intensity of the mixture is the sum of the intensities of the mixed lights." This law is recognized as valid today to the extent that the addition of any

colored light to a mixture always increases—never decreases—the brightness.

Although Helmholtz's name stands out in the researches upon color at this time, very important is the work of the brilliant and young J. Clerk Maxwell (1831–1879), who confirmed Helmholtz's findings on color mixture (1855, 1860), repeated Helmholtz's argument about the mixture of yellow and blue pigments to make green, espoused with Helmholtz the three-color theory of Thomas Young, worked out a practical system for predicting color mixtures, and developed the method of mixing colors by the rotation of colored sectors on disks.

Altogether there were at that time three principal methods of mixing colors. The best method, advocated by Helmholtz, consisted of mixing pure spectral lights. An optical system with a prism, similar to the one shown for Newton in Fig. 8 (p. 104) was so arranged that most of the colors could be interrupted, the chosen colors be left, allowed to recombine, and viewed projected upon a screen or through a telescope. This is the improved method that Helmholtz used in 1855 to correct his first results, the method that was used in the better quantitative researches by König and others, and the best method today.

There was also the method of mixture by transmission and reflection, a method originated by J. H. Lambert in 1760. The observer, viewing one color obliquely through a glass plate, sees another color reflected from the glass, the two superposed. Helmholtz, when he described the method, pointed out that it could be used with colors obtained by white light passed through liquid filters. This technique is common today with plate glass or half-silvered mirrors in a wide variety of situations which require the combination of two visual fields. The half mirror is set at 45° to the line of sight, so that one field can be seen directly through it and the other, at right angles to the line of sight, as reflected from it.

Of course the simplest method of mixture is the combination of colors as sectors of a disk which rotates so rapidly that the colors fuse. This method was first employed by P. van Musschenbroek in 1768. J. Plateau (1829) and H. F. Talbot (1834) used it for photometric purposes. Maxwell (1855) invented the colored disks with the radii cut so that the disks can be fitted together and the resultant sectors varied at will—the device familiar to every student of elementary experimental psychology. For years they were called

Maxwell's disks. He also invented a top that would carry the disks and spin for a long time. Later color-mixers were spindles driven rapidly by a belt from a large wheel turned by hand. Nowadays the electric motor does the work.

That this method of the mixture of colors by rapid succession should be called in question was natural. Helmholtz had shown that the laws of mixture break down for mixed pigments, and succession is not even a true mixture. To settle this matter an appeal was made to what came to be called the *Talbot-Plateau law*. Plateau it was who had really first laid down its essential principle in an obscure paper in 1829. Next, Talbot had established it for use in making photometric matches in 1834, after which Plateau had discussed the matter and formulated the law in 1835. Plateau said: "If an illuminated object affects the eye with regular intermittance, and if the successive moments of its appearance lie so close to one another that the eye can no longer discriminate among them but receives an uninterrupted sensation, then the apparent brightness of this object is weakened as the sum of the durations of the presence and absence of the phenomenon is to the duration of the phenomenon alone." The principle involved is briefly this. The effect of a brightness or color, thus briefly presented, is proportional to the intensity and also to the time of presentation, so that there is no change if an intensity, for example, is halved but maintained twice as long. The general formula is: light of an intensity a , acting through the time t , produces the same effect as light of the intensity a/n , acting through the time nt . Mrs. Ladd-Franklin noted in 1902 that this is a familiar law of chemical action. It had, nevertheless, been accepted with considerable hesitation until Lummer and Brodhun (1889) showed photometrically that the error at most is not more than two per cent.

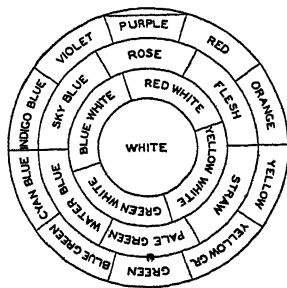


FIG. 13. HELMHOLTZ'S
COLOR CIRCLE (1860)

Color Diagrams

Newton constructed his color circle (Fig. 11) to summarize the facts of color mixture. Although it also showed the relations of

similarity among the colors, its primary purpose was the prediction of the results of mixtures—an approximate prediction only, since Newton did not regard this diagram as working with exactitude.

It was Thomas Young (1801) who fixed attention upon that im-

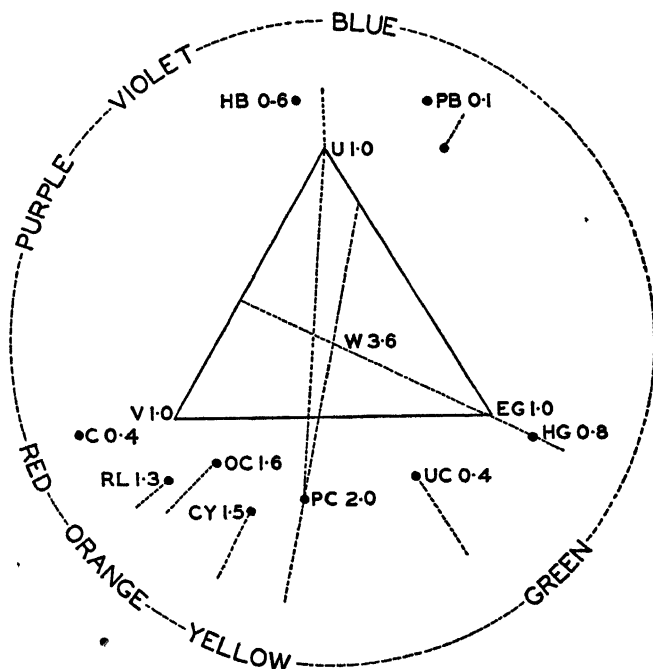


FIG. 14. MAXWELL'S COLOR TRIANGLE (1855)

Maxwell showed the color circle with the triangle within it. The positions of various pigment colors are indicated, and the coefficients indicate the strengths of the colors, thus implying a third dimension to the system.

portant consequence of Newton's discoveries, the fact that all the colors can be had from a mixture of three. Thus Young thought of the color figure as a triangle rather than a circle, for, if three colors be chosen as primaries within Newton's circle, then all their mixtures will lie within the triangle formed upon them.

J. Clerk Maxwell (1855) was the first person to undertake a quantitatively exact formulation of the laws of mixture. Having worked out equations for the mixtures of colors, he represented the mixtures within a triangle, each lying at a center of gravity on a line between the colors mixed. He discovered here that he had to

take account of the intensity of the colors by attaching a numerical coefficient to each. In this way he implied the fact, recognized by everyone shortly thereafter, that the system of colors, being tridimensional, cannot be represented by a plane figure. See Fig. 14.

With the triangle coming to be accepted as the figure for mixtures and with quantification being recognized as the ideal, the question arose as to where the spectrum lies in the color diagram. In Newton's figure, the spectrum was the circle. Helmholtz (1860), recognizing that the spectral colors lie at different distances from central white—that a little yellow, for instance, will cancel a great deal of violet—drew the spectral diagram as in Fig. 15.

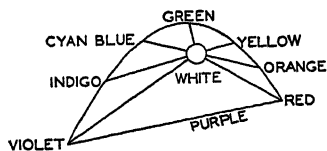


FIG. 15. HELMHOLTZ'S COLOR TRIANGLE (1860)

The forerunner of König's triangle in Fig. 16.

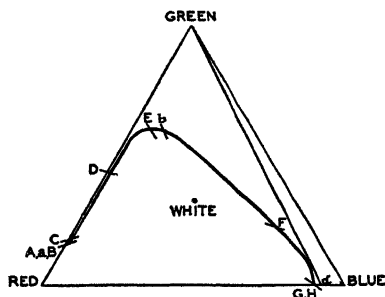


FIG. 16. KÖNIG'S COLOR TRIANGLE (1892)

The heavy line shows the locus of the spectrum within the large theoretical triangle. The letters on the spectral line are for the positions of the Fraunhofer lines. Since the figure is accurate for mixtures, the scaling off of wave-lengths along the spectral line is very irregular.

König and Dieterici made the most careful measurements of spectral mixtures in 1892, and König's triangle has since been accepted as standard. See Fig. 16. In this diagram, where the spectral wave-lengths have been carefully plotted, the locus of the spectrum is shown as lying within an ideal larger triangle of more than spectrally saturated colors. The shape of the spectral line shows how much oversimplified was Newton's generalization that mixed colors are less saturated the more remote their components in the color series.

Paralleling this interest in the making of a diagram to express accurately the facts of color mixture, there was a trend toward the construction of a figure to show the phenomenal relations of the colors to one another. Actually Newton's circle shows these relations, yet with the omission of important facts.

There is the fact that red, yellow, green and blue occupy crucial positions in the color system, and the further fact that black and gray need to be included as well as white. The first fact could be

taken into account by substituting a square for the circle or triangle; the second could be represented by extending the figure in the third dimension.

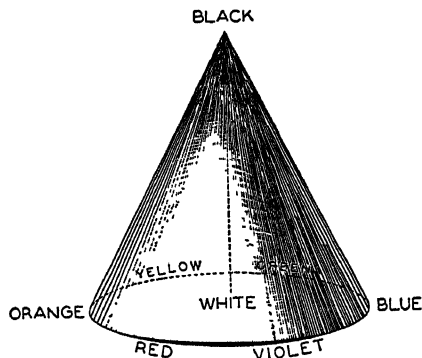


FIG. 17. WUNDT'S COLOR CONE (1893)

J. H. Lambert in 1772 represented the colors in a pyramid with white at the apex and the colors around the base. Wundt once (1893) showed the system as a cone, with black at the apex and the color circle as the base (Fig. 17). These

figures indicate the fact that the number of colors falls off as the system is made brighter in approaching white or darker in approaching black. Thus Wundt's more usual diagram (1874 et seq.) was a sphere (Fig. 18) with white at the top, black at the bottom, the grays falling along a vertical axis, and a horizontal bisecting plane as the color circle.

The first figure to take account of the crucial position of the four principal colors was constructed by the painter, P. O. Runge, in 1810 (Fig. 19). His diagram was the forerunner of the double pyramid or octahedron so familiar in textbooks today. The modern double pyramid was introduced by Ebbinghaus (1902). He made the base square, but tilted it so that the best yellows, being relatively bright, should be near white, and the best blues, being relatively dark, should be near black. He also blunted the six corners for the reason that

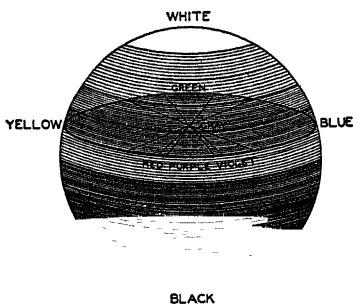


FIG. 18. WUNDT'S COLOR SPHERE (1874)

points of change are not sharply defined. See Fig. 20. Such a diagram does not serve to predict mixtures; it is a purely phenomeno-

logical representation. Red and green, although not complementaries, lie opposite each other. Nor do the lines represent the number of discriminable colors that lie along them, nor (as in König's triangle) the loci of two-color mixtures. Thus, because they represent different sets of facts, the double pyramid and the triangle have both persisted in systematic psychology.

Except as pedagogical devices the diagrams no longer seem important. The facts of color-mixture are better expressed by graphs of functions plotted against standard physical measures than by

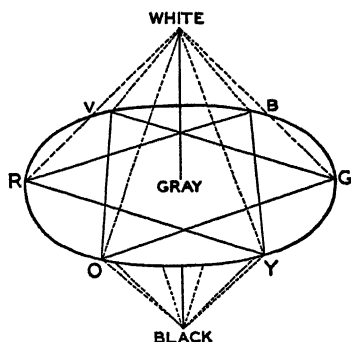


FIG. 19. RUNGE'S COLOR PYRAMID (1810)

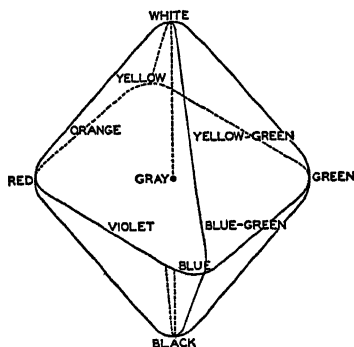


FIG. 20. EBBINGHAUS' COLOR PYRAMID (1902)

König's triangle, where wave-length and energy vary in accordance with no simple spatial rule. For a while Ebbinghaus' double pyramid represented the last stand of the phenomenologists against the encroachments of the nervous system upon psychology: here in the color pyramid, it was argued, there is at least one fact that is independent of both the stimulus and of physiology. That there are but few psychologists any longer to cherish such a last leaf on the tree of mentalism goes to show how phenomenology perpetually, in the development of psychology, loses the battle to experimentalism.

Notes

Color and Its Dependence on the Stimulus

For references to Newton, Young and Goethe, see the text and notes of the preceding chapter.

W. H. Wollaston's paper is: A method of examining refractive and

dispersive powers, by prismatic reflection, *Phil. Trans.*, 92, 1802, 365-380. It was published between Young's first and second papers on color theory. The observation about the black absorption lines separating the four fundamental colors is on pp. 378 f.

J. Fraunhofer's paper is: Bestimmung des Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glassarten, in Bezug auf die achromatischer Fernröhre, *Denkschr. Acad. Wiss. München*, math.-naturwiss. Cl. (1814-1815), 5, 1817, 193-226. See esp. on the black lines, pp. 202-204, and Plate II.

Because the idea was incorrect and sterile, the text makes no mention of Sir David Brewster's view that sunlight is composed of three kinds of light: red, yellow and blue. Brewster showed that glasses of these three colors might completely absorb the spectrum, and that all the visible colors were the result of different combinations of these three lights. The theory is a noteworthy instance of the tendency to refer the characteristics of physiological phenomena to the nature of the physical stimulus, but it ignored the essence of Newton's discovery. See D. Brewster, *A Treatise on Optics*, 1831, Chap. 7. In criticism, see Helmholtz, Ueber Herrn D. Brewster's neue Analyse des Sonnenlichtes, *Ann. Phys. Chem.*, 162, 1852, 501-523, which also gives the other references to Brewster's theory.

On H. L. F. v. Helmholtz's espousal of the 3-color theory, see his Ueber die Theorie der zusammengesetzten Farben, *Ann. Phys. Chem.*, 163, 1852, 45-66. (reprinted in his *Wissenschaftliche Abhandlungen*, II, 3-23), or the *Handbuch der physiologischen Optik*, II, (1860), 1867, (or the reprint as 3 ed., 1911, or the Eng. trans., 1924), sect. 20. For E. Hering's 4-color theory, see his *Zur Lehre vom Lichtsinne*, 1874, Mittheilung 6. For Christine Ladd-Franklin's 4-color theory, see her Eine neue Theorie der Lichtempfindungen, *Z. Psychol.*, 5, 1893, 211-221. On her theories of light-sensation, *Mind*, n.s. 3, 1892, 473-489; or better, the various papers reprinted in her *Colour and Colour Theories*, 1929, which reprints on pp. 219-230,

72-91 the first two papers mentioned here.

The problem of the elementary colors has been still further confused by its terminology. We have in this literature the following words and meanings. *Urfarbe* = *primal color* = *primitive color* was used by Hering for the physiological elementary colors, but unfortunately used by Westphal and others in the sense of *Hauptfarbe* (*infra*), and hence so defined by H. C. Warren in his *Dictionary of Psychology*, 1934. *Hauptfarbe* = *fundamental color* is practically the same as *Prinzipalfarbe* = *principal color*, and is generally used for the four (or three) 'pure' colors which form the frame of reference in a phenomenological system of color similarities, like the color square. *Grundfarbe* = *primary color* is a term used by Helmholtz, Mach and Ebbinghaus for any one of the three colors which are basic to a system of mixtures and which can thus give all the colors.

Hering's reference of 1874 to the yellowishness of spectral red is in *loc. cit.* (p. 108 of 2 ed., 1878). For his determination of the amounts of spectral red and blue in pure red, see his Ueber individuelle Verschiedenheiten des Farbensinnes, *Lotos*, 6, 1885, 142-198, esp. 162. For F. C. Donders' determinations of *Urgelb*, see his *Farbengleichungen*, *Arch. Physiol. Leipzig*, 1884, 518-552, esp. 533. For C. Hess's values of the Hauptfarben, see his Ueber die Farbensinn bei indirectem Sehen, *Arch. Ophthal. Berlin*, 35 (4), 1889, 1-62, esp. 20 f. The standard determinations at the present time are from H. Westphal, Unmittelbare Bestimmungen der Urfarben, *Z. Sinnesphysiol.*, 44, 1910, 182-230. Westphal discusses the history of this problem, pp. 182-187.

The references to the researches on differential color sensitivity and the discussion of the number of discriminable colors are as follows: E. Man-

delstamm, Beitrag zur Physiologie der Farben, *Arch. Ophthal. Berlin*, 13 (2), 1867, 399-406; W. Dobrowsky, Beiträge zur physiologischen Optik, *ibid.*, 18 (1), 1872, 53-103, esp. 66-74; J. v. Kries, Die Gesichtsempfindungen und ihre Analyse, *Arch. Physiol., Leipzig*, 1882, Suppl. Bd., 57; A. König and C. Dieterici, Ueber die Empfindlichkeit des normalen Auges für Wellenlängen-Unterschiede des Lichtes, *Arch. Ophthal. Berlin*, 30 (2), 1884, 171-184 (reprinted in König's *Ges. Abhandl.*, 1903, 23-33); O. Külpe, *Outlines of Psychology* (1893), trans. 1895, 127; L. A. Jones, The fundamental scale of pure hue and retinal sensibility to hue differences, *J. opt. Soc. Amer.*, 1, 1917, 63-77.

The discussion of simplicity vs. complexity for colors that are not *Hauptfarben* is widespread, but the following references show its nature and its dates. It is a problem that belongs peculiarly to the period 1890-1910, when introspection was generally supposed to be the sole method of psychology. On the side of simplicity, there were Külpe, *op. cit.*, 126; E. B. Titchener, *Outline of Psychology*, 1896, 55 (cf. his *Text-Book of Psychology*, 1910, 60-64); H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1897, 187-191 (sect. 14.2). On the side of complexity, there were Ladd-Franklin, *opp. cit.*, and presently F. Brentano, *Untersuchungen zur Sinnespsychologie*, 1907, esp. 51-79. G. F. Stout, *Manual of Psychology*, 1899, 148 f., is practically on the side of complexity, and M. Bentley, The simplicity of color tones, *Amer. J. Psychol.*, 14, 1903, 92-95, is practically on the side of simplicity, although both discuss inadequately the meaning of analysis in this case. For the only record of Ladd-Franklin's exhibit of complex and unitary colors, see her Determination of the psychically unitary color-sensations, *Psychol. Bull.*, 12, 1915, 62 f. For experiments

directed upon the solution of the problem and further discussion of it and its history, see E. M. Alspach, Simplicity of color hues, *Amer. J. Psychol.*, 27, 1916, 273-282.

On the problem of phenomenal green as a psychological mixture of yellow and blue, see Brentano, *op. cit.*, esp. 1-50, 129-161, although the entire book is really about this matter. E. B. Holt supported Brentano, *New Realism*, 1912, 332-335. Again see Alspach, *op. cit.*, esp. 280-282.

On the Bezold-Brücke phenomenon, see W. v. Bezold, Ueber das Gesetz der Farbenmischung und die physiologischen Grundfarben, *Ann. Phys. Chem.*, 226, 1873, 221-247, esp. 237-239; E. Brücke, Ueber einige Empfindung im Gebiete des Sehnerven, *Sitzungsber. Akad. Wiss. Wien, math.-naturwiss. Cl.*, 77 (3), 1878, 39-71; D. M. Purdy, The Bezold-Brücke phenomenon and contours for constant hue, *Amer. J. Psychol.*, 49, 1937, 313-315. The problem as to whether there is no such color as dark yellow troubled Hering and others. Purdy's research was first reported in 1929, but the fullest printed account is the note of 1937.

Brightness and Intensity

On E. Hering's phenomenological distinction between a light sense and a color sense, see his *Zur Lehre vom Lichtsinne*, (1874) 1878, and compare Mittheilung 5 with Mittheilung 6.

Is black a sensation? See H. L. F. v. Helmholtz, *Physiological Optics*, III, (1866) trans. 1925, 204 f.; Hering, *op. cit.*, Mittheilung 4; J. Ward, Is 'black' a sensation?, *Brit. J. Psychol.*, 1, 1905, 407-427; A further note on the sensory character of black, *ibid.*, 8, 1916, 212-221; E. B. Titchener, A note on the sensory character of black, *J. Phil.*, 13, 1916, 113-121; A further word on black,

ibid., 649-655; and references cited by the last two authors.

The following references deal with the problem of intensity vs. quality in the black-gray-white series. Hering denied intensity for the black-whites: *op. cit.*, sect. 21. G. E. Müller attempted a psychophysiological resolution: *Zur Psychophysik der Gesichtsempfindungen*, *Z. Psychol.*, 10, 1896, 30-33; 14, 1897, 40-46, 60-64. E. B. Titchener rejected Müller's view and discussed the problem citing other references (q.v.): *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 20-22, 324 f. Titchener argued explicitly for both attributes: *Text-Book of Psychology*, 1910, 205 f., 217, 219. E. J. Gates tried to show that the limens for intensity and quality are different: On intensive and qualitative judgments of light sensations, *Amer. J. Psychol.*, 26, 1915, 296-299. C. Stumpf gave his view: *Die Attribute der Gesichtsempfindungen*, *Abh. preuss. Akad. Wiss., philos.-hist. Kl.*, 1917, no. 8. F. L. Dimmick found that white and black do not overlap in the gray series and determined their thresholds: A note on the series of blacks, greys and whites, *Amer. J. Psychol.*, 31, 1920, 301 f. Titchener revised his view: Visual intensity, *ibid.*, 34, 1923, 310 f. Dimmick clarified his: A reinterpretation of the color-pyramid, *Psychol. Rev.*, 36, 1929, 83-90. Finally S. S. Stevens has shown that the question of one or two attributes depends, not on a direct introspective apprehension of the attributes, but on experimentally derived laws of dependence of the introspective judgments of equality upon the values of the stimulus: The attributes of tone, *Proc. nat. Acad. Sci. Wash.*, 20, 1934, 457-459; see also E. G. Boring, The relation of the attributes of sensation to the dimensions of the stimulus, *Phil. Sci.*, 2, 1935, 236-245. See also pp. 25 f.

The dual perception of quality and intensity comes about because ob-

jects tend to keep quality constant in a varying illumination which can also be perceived. The classical work on this subject is D. Katz, *Die Erscheinungsweisen der Farben*, 1911 (*Z. Psychol.*, Ergbd. 7), esp. 79-245. An early summary of the problem is K. Bühler, *Die Erscheinungsweisen der Farben*, 1922, esp. 72-141. For the history, see R. B. MacLeod, An experimental investigation of brightness constancy, *Arch. Psychol. N. Y.*, 21, 1932, no. 135, 19-57. For still more recent discussion, see K. Koffka, *Principles of Gestalt Psychology*, 1935, esp. 240-264.

It should be noted that the shift from sensation to perception, from simplicity to complexity, from a simple retinal system to the total organic system, from immediate experience to meaningful awareness, from Titchener to Koffka—the list gives different aspects of the same shift—is crucial. To be aware of the intensity of the illumination as something different from the brightness of the object illuminated is to respond adequately to each aspect of the situation but not necessarily to have two differentially unique experiences. The difficulty here is that many psychologists are coming to doubt whether immediate experience, as Titchener believed in it, actually exists for any plausible scientific purpose, or whether this discriminatory awareness of knowledgeable data is not all there is for the psychologist to use.

Weber's Law

The researches cited on Weber's law applied to visual intensity are as follows: P. Bouguer, *Traité d'optique sur la gradation de la lumière*, 1760, 51 f.; V. Masson, *Étude de photométrie électrique*, *Ann. Chim. Phys.*, 3 ser., 14, 1845, 129-195, esp. 148-154; D. F. J. Arago, Un mouvement modéré rend la visibilité des objets plus facile (1850), in *Oeuvres complètes*, X,

1858, 255-260; G. T. Fechner, Ueber ein psychophysisches Grundgesetz und dessen Beziehung zur Schätzung der Sterngrößen, *Abh. sächs. Ges. Wiss., math.-phys. Cl.*, 4, 1859, 457-532, esp. 457-459, 465-483; *Elemente der Psychophysik*, 1860, I, 139-175; H. L. F. v. Helmholtz, *Physiological Optics*, II, (1860) trans. 1924, 172-203; H. Aubert, *Physiologie der Netzhaut*, 1865, 52-82; A. König and E. Brodhun, Experimentelle Untersuchungen über die psychophysische Fundamentalformel in Bezug auf den Gesichtssinn, *Sitzungsber. preuss. Akad. Wiss.*, 1888, 917-931; 1889, 641-644; J. Blanchard, The brightness sensibility of the retina, *Phys. Rev.*, ser. 2, 11, 1918, 81-99.

Another phase of the early establishment of Weber's law for vision was Fechner's study of stellar magnitudes. The astronomers Herschel and Steinheil had shown, by photometric measurements, that the accepted scale of stellar magnitudes is a logarithmic function of the intensities of the stellar light. Fechner thought that this fact supported Weber's law. See Fechner and Helmholtz, *loc. cit.*

Although there is a considerable literature of Weber's law in the field of vision, most of it gets nowhere. Fechner 'established' the law in 1860, and Aubert disestablished it in 1865 by showing how the Weber fraction varies with intensity. Since then no one has ever supposed that the Weber fraction remains constant at all intensities, but there have been many attempts to explain its departure from constancy, like the paper of H. Ebbinghaus, Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen, *Arch. ges. Physiol.*, 45, 1889, 113-133. The point is that Weber's law won belief because it is obvious that the just noticeable difference increases as the stimulus increases and that it represents more nearly a constant proportion of the stimulus than a constant

absolute amount. Thus theorizing took the form of explaining—or even explaining away—the deviation of the actual results from the Weber law, instead of explaining the form of the Weber function without relation to the 'law' which the results did not confirm. The study of this literature bears more upon the psychology of scientific belief than upon that vision which is mediated by the retina. Thus recently L. T. Troland spoke as if Weber's law might be the true function for part of the intensive range: "The exact value of the relative threshold varies rather widely with conditions, but is of the order of magnitude of 1/100th part, or 1%, for the intensity range within which the logarithmic relationship holds," *Principles of Psychophysiology*, II, 1930, 79. Against this view that the Weber function is fundamentally logarithmic in spite of certain deviations from the true law, see H. Hoagland, The Weber-Fechner law and the all-or-none theory, *J. gen. Psychol.*, 3, 1930, 351-372. An excellent summary of results, showing that the fraction tends to be minimal for some middle intensity, is A. H. Holway and C. C. Pratt, The Weber-ratio for intensive discrimination, *Psychol. Rev.*, 43, 1936, 322-340.

Color Mixture

For Isaac Newton's laws of color mixture, see the preceding chapter, pp. 102-106. For his further discussion of color mixture and his color circle, see his *Opticks*, 1704 et seq., Bk. I, Pt. 2, prop. 4-7.

H. L. F. v. Helmholtz's two most important papers on this subject are Ueber die Theorie der zusammengesetzten Farben, *Ann. Phys. Chem.*, 163, 1852, 45-66; and Ueber die Zusammensetzung von Spectralfarben, *ibid.*, 170, 1855, 1-28; which are reprinted in his *Wissenschaftliche Abhandlungen*, 1883, II, 1-23 and 45-70 respectively. See also his

discussion in his *Handbuch der physiologischen Optik*, II, 1860, sect. 20, reprinted in Germ. 3 ed. and Eng. trans.; and the later discussion in *idem*, 2 ed., 1896, sect. 20.

H. Grassmann's paper is *Zur Theorie der Farbenmischung*, *Ann. Phys. Chem.*, 165, 1853, 69-84. It is translated into English in *Phil. Mag.*, 4 ser., 7, 1854, 254-264.

For J. Clerk Maxwell's more important papers on this topic, see his *Experiments on colour*, as perceived by the eye, with remarks on colour-blindness, *Trans. roy. Soc. Edinb.*, 21 (2), 1855, 275-298; On the theory of compound colours, and the relations of the colours of the spectrum, *Phil. Trans.*, 150, 1860, 57-84; reprinted in *The Scientific Papers of James Clerk Maxwell*, 1890, I, nos. 7 and 21. See *ibid.*, I and II for Maxwell's other papers on col'cr.

The later determination of the wave-lengths of complementaries, determinations which confirmed Helmholtz's values of 1855, are: M. von Frey and J. von Kries, *Ueber die Mischung von Spectralfarben*, *Arch. Physiol. Leipzig*, 1881, 336-353; A. König and C. Dieterici, *Die Grundempfindungen in normalen und anomalen Farbensystemen und ihre Intensitätsverteilung in Spektrum*, *Z. Psychol.*, 4, 1892, 241-347, esp. 287 (Helmholtz gives this reference incorrectly as *Wied. Ann.*, 1887, and Nagel has twice copied him); R. H. Sinden, *Studies based on spectral complementaries*, *J. opt. Soc. Amer.*, 7, 1923, 1123-1153.

For the origin of color-mixing by transmission and reflection, see J. H. Lambert, *Photometria, sive de mensura et gradibus luminis, colorum et umbrae*, 1760, 527, [n.v.]. For the first use of rotating sectors for color mixture, see P. van Musschenbroek, *Introductio ad philosophiam naturalem*, 1768, II, 728, sect. 1820.

On the origins of the Talbot-Plateau law, see J. Plateau, *Dissertation sur quelques propriétés des im-*

pressions produits par la lumière sur l'organ de la vue, 1829, [n.v.]; *Betrachtungen über ein von Hrn. Talbot vorgeschlagenes photometrisches Princip*, *Ann. Phys. Chem.*, 111, 1835, 457-468; H. F. Talbot, *Experiments on light*, *Phil. Mag.*, 3 ser., 1834, 321-334, esp. 327-334. The validity of the law was pretty well established for photometric purposes by O. Lummer and E. Brodhun, *Photometrische Untersuchungen*, I, *Ueber ein neues Photometer*, *Z. Instrumentenkunde*, 9, 1889, 41-50 [n.v.].

Color Diagrams

For H. L. F. v. Helmholtz's circle, see *Handbuch der physiologischen Optik*, 1 ed. (or 3 ed.), II, 1860, sect. 20, Fig. 16; for his triangle, *ibid.*, Fig. 19.

For J. C. Maxwell's triangle, see *op. cit.*, 1855, Plate I, Fig. 1.

König's triangle was developed in König and Dieterici, *op. cit.*, Figs. 7 and 8.

For W. Wundt's cone, see his *Vorlesungen über die Menschen- und Thierseele*, 2 ed., 1893, (or Eng. trans.), Lect. 6, Fig. 13. Wundt did not give the cone in the first edition of 1863, and it is not clear why he introduced so inferior a diagram after he had given the sphere in 1874.

Wundt cites the reference to J. H. Lambert's pyramid as *Beschreibung einer mit dem CALAU'schen Wachs ausgefalteten Farbenpyramide*, 1772 [n.v.]. P. O. Runge's *Farbenkugel*, 1810, gives the double pyramid as Fig. 4, and is reprinted in his *Hinterlassene Schriften*, 1840, I, 112-137, esp. 118.

For Wundt's sphere, see his *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, 395, or any of the five succeeding editions.

For H. Ebbinghaus' double pyramid, see his *Grundzüge der Psychologie*, 1 ed., 1902, I, 184. This figure is the best known of them all.

Chapter 5

VISUAL PHENOMENA

THE scientific interest in color began, as we have seen, with the objective physical problem, the problem of the stimulus to color, not the problem of seen color itself. Interest centered in the nature of light. Newton's discovery that white light is a mixture of colored lights was really the discovery of the subjective fact that white, a simple color showing no evidence of components, is an illusion, a phenomenon that depends less upon the light (for white has no unique stimulus, there are many different light mixtures that will give white) than upon the constitution of the perceiving organism. Newton's interest, however, was chiefly focused upon the nature of the stimulus. His laws of color mixture were laws of the ways in which light combines to make various colors; no one at the time realized that Newton was actually investigating the properties of the percipient person.

On the other hand, there was to be found an incidental interest in phenomena that were recognized as subjective. The careful historian can note the phenomenological observations of Aristotle and Ptolemy; he can cite Alhazen's *Opticae thesaurus* (ca. 1100) and Leonardo da Vinci's *Trattato della pittura* (ca. 1519); he can quote half a dozen other ancients who wrote before the seventeenth century. In the seventeenth century there were more than a score of men who interested themselves in incidental subjective phenomena of vision: Kepler (1604), Boyle (1663), Mariotte (who discovered the blind spot in 1668), Newton. There was a considerable literature by the astronomers, who had discovered irradiation, the fact that the apparent size of a celestial object depends upon its brightness. The eighteenth century began with Newton's *Opticks* (1704). By 1772 there was enough known for the chemist, Joseph Priestley, to write, as an 812-page book, *The History and Present State of Discoveries Relating to Vision, Light, and Colours*, but a book which dealt much more with the physics than with the psychology of color. G. L. L. Buffon, the French naturalist, wrote a

Dissertation sur les couleurs accidentelles in 1743, but for the most part the phenomenological writings in this century were by less famous men: C. Scherffer (1761-1775), N. de Beguelin (1767-1771), de Godart (1778), C. G. Kratzenstein (1781), R. W. Darwin (1786). These papers, however, were but a beginning. Plateau, who wrote in 1876 a history of visual sensation and wrote from the phenomenological point of view, cited less than sixty references for the eighteenth century and over seven hundred for the first seventy-five years of the nineteenth. Thus Goethe may be counted as the real head of the phenomenological tradition in the study of vision, for, even though all the important topics had already been touched upon, there was before him relatively little careful systematization of the field.

Plateau, in writing this historical account, classified the hundred-odd papers that had been published before 1800 under six rubrics, which show more or less the nature of the subjective problems recognized in the early days. (1) Plateau's first section dealt with "the persistence of impressions on the retina," a topic which must have been especially interesting to one of the authors of the Talbot-Plateau law. (2) His second section covered the "ordinary accidental colors of succession." Buffon (1743) had coined this phrase, "accidental colors" (*couleurs accidentelles*, *zufällige Farben*), to cover these subjective phenomena which seem somewhat capricious in occurrence. In English the phenomena were also often referred to as *spectra* (or in French, *spectres*). The word *spectrum* in those days meant a visual appearance, as distinguished from a corporeal reality, and was thus similar to *spectre*. It was Newton who applied the term *spectrum* to the colored image formed by a prism. (The French *spectre* means both a ghost and the band of colors.) Plateau in this section brought together the data for the flight of colors and for those after-images in which the form is not a clear-cut objective image. (3) His third section took up the well-defined after-images: "images which follow the contemplation of objects of great brilliance or even of well-illuminated white objects." (4) "Irradiation" was his fourth topic. Although Leonardo da Vinci had noted that whites tend to be larger than blacks, it was for the most part the astronomers who were interested in the spatial relations of dark and light objects in eclipses and occultations. (5) The fifth topic, "the ordinary phenomena of contrast," presented the essential facts of simultaneous contrast,

but it lacked definiteness because the fundamental importance of paired complementariness among the colors was not recognized so early. (6) Plateau ended his résumé with summaries of the literature on "colored shadows," for which the technique of production had been worked out without a clear understanding of the fact that the shadows are instances of complementary color contrast.

Such is the background from which the modern laws of after-image, adaptation and contrast emerged.

After-Images

There are certain visual after-effects so easily obtained and so striking that they were bound early to find their way into the literature of casual observation. (1) The circle of light that is seen on whirling a live coal indicates a persistence of visual sensation. So too does the confusion of the rapidly moving parts of an object. Ptolemy remarked that a disk, painted with various colors, appears to be but a single color when it is turned rapidly. (2) Looking at the sun even briefly leaves a succession of colored appearances for the closed eye, or else after-images of the sun's disk upon any field of projection. Of this observation there are numerous early instances, although a systematic account of the phenomenon seems not to occur before Boyle's description (1663). (3) Gazing fixedly at a window and then looking away gives a negative after-image of the pattern of brightness—the dark frame and the bright field next it are reversed. So natural is it to look at windows that one ought to expect early descriptions of this phenomenon of reversal, and such descriptions were indeed given by Aguilonius in 1613 and by N. C. F. de Peiresc in 1634. (4) The fact that these after-effects may recur periodically led Aristotle and others after him to consider dreams as the recurrences of perceptual images of waking life.

The early interest in after-images was frivolous. A priest, Kircher, told (1646) how a friend had wagered that he could make the priest see in the dark, and had won the wager by having him look steadily at a drawing by a window in a dark-room and then gaze 'at' a piece of white paper when the room was suddenly thrown into complete darkness. The paper, as Helmholtz noted later, was unnecessary; but Kircher's theory was that the light from the object, having entered the eye, was again projected by

the eye upon the paper—a theory illustrative of the seventeenth century's uncertainty about the nature of light and of visual perception.

Newton (1691), queried by John Locke about Boyle's account of the after-effects of gazing at the sun, looked himself at the mirrored image of the sun three times in close succession, and then had to shut himself up for three days in a dark room in order to get rid of the after-images. Newton was of the opinion that he had also to divert his mind from the after-images and that they would still have returned after a very long time had he not resisted seeing them.

In the eighteenth century there was some systematic study. Jurin (1738) put forth the theory that the reversal of brightness in the negative after-image is due to the fact that the process in the eye continues but is opposite in effect—a theory which is somewhat advanced over the fatigue theory that presently came into vogue, because it accounts for an after-image in complete darkness when there is no stimulation to the eye at all. Buffon (1743), who gave a full and circumstantial account of the occurrence of "accidental colors," was the first to describe in detail negative after-images of colors, noting that blue is the cause of 'accidental' yellow and conversely, that red is the cause of 'accidental' green and conversely, and even that the consequence of red is a green that is somewhat bluish. Thus Buffon actually sought laws for these effects; he did not regard them as truly accidental. Benjamin Franklin (1765) noted how the positive after-image appears on the field of the closed eyes and the negative on a field of white paper. This phenomenon, called thereafter the "Franklin experiment," was discussed by Godart (1776), along with an elaborate account of the flight of colors.

In 1765 Father Scherffer published a long discussion of these after-images. He noted that the color reversals occur between the complementaries of Newton's color circle, and went on to discuss the painting of a picture in the complementaries of the correct hues so that the after-image would assume the natural colors. He proposed the fatigue theory of the negative after-images of both brightnesses and colors, the theory that is essentially part of the Young-Helmholtz theory. The eye, locally fatigued to brightness or specific colors, sees white light with a diminution of the previous excitation, so that the former light regions become relatively dark,

and the colored regions appear the complementary color (for white less some of the color for which the eye is fatigued gives a complementary color). This theory, though more specific than Jurin's, still does not explain how the negative images occur in complete darkness.

The scientific situation for the after-image at the end of the eighteenth century was set forth in an experimental paper by Robert Waring Darwin, the father of Charles Darwin and the son of Erasmus Darwin, who communicated the paper to the Royal Society in 1786. Robert Darwin undertook first to show that the retina itself is active in vision, since it gives rise to "ocular spectra" when no light is being admitted to the eye. He then laid down the laws of retinal sensibility. "The retina is not so easily excited into action by less irritation after having been lately subjected to greater"; and conversely. In these principles we have what amounts to a law of adaptation, and we see how negative after-effects are related to the differential fatigue of the retina. Darwin's explanation of positive after-images was that retinal excitation tends to continue if the retina is not fatigued, and that it continues spasmodically with recurrent spectra if the original intensity is great enough. When the stimulus is continued and the retina fatigued, however, then the spectra are reversed, in brightness and in color, as might be expected from the principles of sensibility just stated. He paired green (not blue-green) with red, orange with blue, and yellow with violet, being thus less phenomenologically accurate than Buffon. Darwin also noted that the reversed spectra are recurrent when the stimulus has been intense enough, that, when the sun is the stimulus, they may last for days.

We have already seen what impetus Goethe gave to the phenomenological description of visual facts in the nineteenth century (pp. 112-119). Purkinje followed him, and even Johannes Müller wrote a little book on subjective visual phenomena (1826). What was becoming a large literature of conflicting opinions about the after-images was then brought together and ordered, first by Plateau in 1833 and then by Fechner in 1838. Fechner stressed the relation of the reversals to the complementariness of the colors. Helmholtz incorporated the explanation of negative after-images by relative fatigue into his general theory of vision—the Young-Helmholtz theory as it came to be called. For Helmholtz the reversals were complementary because complementaries, when

mixed, give white; hence the diminution of one color (or of its stimulating power) in white yields an increasing effect of the complementary color. Hering, on the other hand, built his theory upon complementaries, supposing that adaptation to one color means sensitization to its opposite. Thus Hering's theory avoided the objection that the negative image would not be seen by a lightless retina. Modern views, of course, take it for granted that excitation disturbs some sort of chemical equilibrium in the retina, which, being recovered in the absence of excitation, excites the complementary quality.

The term *spectra* dropped out of use about the middle of the nineteenth century and *accidental colors* a little later. Fechner (1838), who consistently spoke of *after-images* (*Nachbilder*), seems to have established the word in its technical meaning. Hering later (1872) used the general term, *light-induction*, for both positive and negative and both simultaneous and successive effects. He also used the term *successive contrast* for the negative after-effects.

Adaptation

We have just seen that the phenomenon of negative after-image was explained by such early investigators as Father Scherffer and Robert Darwin as due to the selective fatigue of the retina to continued excitation, such that, with sensitivity differentially diminished, a general excitant, like white light, would produce a residual and hence a reversed effect. Thus it can be said to have been realized throughout most of the eighteenth century that excitation leads to diminished sensitivity.

It was Hermann Aubert, the physiologist who shares with Helmholtz and Hering the distinction of being a chief pioneer in the field of physiological optics, who in 1865 introduced both the concept of adaptation and the term. The words *adaptation* and *accommodation* had both been used to denote the adjustment of the eye for near and far focus, but *accommodation* had received preference for this function since about 1850, and it was that meaning which Helmholtz fixed upon psychology. Aubert in his *Physiologie der Netzhaut* (1865) sought to limit the word *adaptation*, on the other hand, to the adjustment of the eye to the intensity of light, and his usage has held ever since.

Since the problem arose in connection with the diminution of

visual sensitivity as the result of excitation, it was natural that Aubert should undertake to measure adaptation in terms of sensitivity, and not phenomenologically in terms of the level of brightness at which objects are seen. As observer he would go from a light into a dark room, where he would adjust the length of a fine platinum wire connected across a Daniel cell until the wire, hot from the current, was just visible in the dark. The shorter the wire, the less its electrical resistance, the greater the current, and thus the greater the light given off. By the use of an episcotister (an instrument which he both invented and named) he calibrated the brilliance of the wire photometrically to establish the ratio of brightnesses between two lengths of wire shunted across the constant voltage of the cell. In this way he determined the absolute threshold for illumination after periods of dark-adaptation that ranged from no time at all up to more than two hours. The resultant curve showed that there was more adaptation in the first five minutes than in the next 115 minutes, and that adaptation was still not complete after two hours. Aubert's function—plotted actually between sensitivity (reciprocals of the absolute threshold) and time—has the general form and approximate values of the later more careful investigations; and this in spite of a serious error in his method, his neglect to control the degree of light-adaptation with which the subject entered the dark.

This omission was significant. Aubert knew nothing about light-adaptation, although it was of course the more obvious basis for the negative after-images. He thought of the dark as a state of no excitation and, therefore, as the condition of complete rest for the eye, a state in which the retina would gradually reach maximal sensitivity. Like Helmholtz he conceived of the zero of the visual system as lying in black and at no illumination. It took Hering to argue that black is a sensation, that the visual system is symmetrical about middle gray, and that the phenomena of dark-adaptation are matched by the phenomena of light-adaptation. Aubert, it is true, noted the crucial phenomenological fact that the dark field becomes subjectively lighter as the observer continues in the dark room, but he failed to see the implication of this change—that black implies excitation and that the resting retina finds its equilibrium at gray.

Hering, however, saw this point and made it the central principle of orientation for his *Zur Lehre vom Lichtsinne* in 1872-1874.

He argued that black is a sensation and that the series of grays from black to white consists of a constant increase in the weight of white and decrease in the weight of black in the total excitation. Middle gray is the point that is just as white as it is black, or, for that matter, just as blue as it is yellow. We have already seen (pp. 132 f.) the difficulty that such a view brought, the question as to why the middle point of the system is not an invisibility instead of a gray, and we have seen how Müller rescued Hering from this difficulty by his hypothesis of cortical gray. Nevertheless, in spite of its introduction of this new difficulty, Hering's view was an advance over Aubert's. It took cognizance of the phenomenologically positive character of black, which means that black must also be physiologically positive at the level of the retina, although physically negative at the level of stimulus-illumination. Perhaps even then the symmetry of the visual system would not have become apparent had not Hering made it vivid in terms of his theory of retinal excitation, the theory that there are three retinal substances, each capable of undergoing a reversible process—one substance for green-red, one for blue-yellow, and one for black-white. With this picture of adaptation as a balance between two opposing tendencies, the notion of retinal fatigue appeared to be quite inadequate to express the effects of continued stimulation.

The classical researches, after the symmetrical nature of the problem became clear, are Piper's on dark-adaptation and Lohmann's on light-adaptation, both of them done under Nagel's inspiration at Berlin. Piper (1903) improved Aubert's technique. He controlled the initial state of light-adaptation at a high value, subsequently measuring adaptation by the absolute threshold for brightness in the dark. The resultant function is sigmoid in form; during the first nine minutes, there was little adaptation, whereas during the next twenty minutes adaptation went on rapidly and after an hour was beginning to level off. Nagel, however, asserted that even after sixteen hours (an experiment on himself) it is not clear that complete dark adaptation has been reached.

To determine functions of light-adaptation is much more involved, inasmuch as there is no single level of illumination to which adaptation should naturally be made. It was Lohmann (1906) who found the functions for adaptation to various brightnesses, and he got a clear indication that the final level of equilibrium is higher, the higher the intensity to which adaptation is being made.

(There is, of course, no reason why dark-adaptation should not also be determined for various levels of low illumination, but not even yet does the problem seem quite symmetrical to psychologists: darkness appears to be more 'absolute' than lightness.) Lohmann continued to use Aubert's and Piper's criterion of sensitivity: the absolute threshold for brightness in the dark. He had to take his light-adapted observers out of the light, put them in the dark, and determine the threshold quickly (10 secs.). It would seem natural for him to have used the differential intensive limen at the level of illumination to which the observer was being adapted; that would be the true analogy to Aubert's method. With tradition dominant, however, he kept to the absolute threshold as a measure. Lohmann's functions show light-adaptation to be much more rapid than dark-adaptation. There is a tremendous change in the first minute, there is very little change after the first ten minutes, and complete adaptation may be reached in ten minutes for intense light or after only an hour for weaker light—for the rate of change depends upon the intensity to which adaptation is made.

In a sense all this measurement of adaptation is indirect. What the procedures measure directly is a capacity for discrimination. A direct measure would give the curve that represents the phenomenological change—in dark-adaptation the shift of black toward middle gray and in light-adaptation the shift of white toward middle gray. One can note, as Aubert did, that the black of complete darkness gets subjectively lighter as it continues, but the function is difficult to determine with any exactitude because the method would normally involve a comparison of two grays separated by a long interval of time, the comparison of the gray after adaptation with the gray before adaptation. Since the whole of the retina is always exposed to some degree of lightness or darkness, there is no way of comparing the adaptation of one part of the retina with some other part which has no adaptation: every part is in some state of adaptation depending on its previous stimulation.

For colors the situation is different. Although the negative after-images had shown that sensitivity to color diminishes with continued excitation, Aubert did not mention color in connection with adaptation, partly because there is no way in which the absolute threshold of vision changes with color adaptation. The first direct research on retinal fatigue for color was undertaken by S. Exner in Helmholtz' laboratory at Heidelberg in 1868. Exner studied the

fading of colors with continued fixation in an effort to find support for the Young-Helmholtz theory of color vision. His method was to fixate a small patch of color for the desired adaptation time and then to project the after-image on a much larger colored field. Thus fixation of a red patch with the subsequent projection of the after-image on a field of the same red showed the after-image to be an unsaturated red as compared with the rest of the field. Here the field serves as a standard of comparison, because the retina during adaptation is excited by a gray, which is a chromatically neutral field, and thus it comes to the colored field without previous adaptation to color. (In the case of brightness-adaptation there was no way of getting a comparable neutral adaptation field.) Exner also found that the after-image of a green projected on a red field gave a red better saturated than the field. The other colors gave comparable results. In this way he demonstrated clearly the diminution of color sensitivity with continued excitation.

It was Hering who by the symmetry of his theory for colors and for grays brought color fatigue under the general concept of adaptation. Light-adaptation is a function of the change in the retinal 'black-white' substance, he thought; so chromatic adaptation must be a function of changes in the 'blue-yellow' and 'green-red' substances.

There was not much early research on chromatic adaptation. Apparently the first paper after Exner was one by John Aitken in 1872, followed in 1899 by G. J. Burch's description of the temporary color-blindness which he induced by chromatic adaptation.

It is a question as to whether color completely disappears under adaptation. Burch found that it does, and he is supported in a recent study by H. Sheppard (1920). L. T. Troland, on the contrary, has held that equilibrium is reached at different levels under different conditions, and that ordinarily some hue remains at maximal adaptation.

Another controversial problem concerns the constancy of hue in a color undergoing adaptation. Exner found only three (primary) colors that adaptation does not change in hue; and he was supported in this finding by Hess (1890) and Voeste (1898). Troland, however, has contradicted this finding on the ground that the change appears because the Bezold-Brücke phenomenon is involved and not because of chromatic adaptation *per se*.

Contrast

The principle of brightness contrast was known to Aristotle, the fact that the margin of a black on a white ground may seem blacker than the rest of the color, and conversely. Alhazen (ca. 1100) attributed the invisibility of the stars in daytime to the lack of contrast between them and the bright sky. Leonardo da Vinci (ca. 1519) laid down a number of exact rules whereby painters might use or take account of contrast between black and white. Although not always intelligibly, the early writers also mentioned color contrast; frequently, however, in such a way that the point of the remark seems to lie in the brightness contrast of colors. Leonardo, however, stated the correct rule for color, pairing the complementaries correctly almost two centuries before Newton made out the general table of opposition between pairs of colors. He wrote: "In order to attain a color of the greatest possible perfection, one has to place it in the neighborhood of the directly contrary color: thus one places black with white, yellow with blue, green with red."

Almost all the writers whom we have mentioned in this chapter and who wrote before 1800 made at least incidental mention of brightness contrast, and often they spoke of color contrast, although the examples of the latter phenomenon do not always seem correct in the light of our present knowledge. Many of them, like Aristotle, noted the occurrence of a marginal contrast, a halo of special enhancement close to the boundary contour. This phenomenon was, however, undoubtedly due to eye-movement and the reenforcing after-image, as Helmholtz later made clear (1860). There were also, however, many valid examples. Newton cited Halley's remark that the hand of a deep-sea diver appears rose-colored against the green of the sea-water—doubtless because contours are blurred and brightness more nearly equalized in the depths. Buffon (1743) gave instances of both brightness contrast and color contrast as a form of accidental colors. Robert Darwin summed the matter up in 1786: "It was before observed, that when the two colours viewed together were opposite to each other, as yellow and blue, red and green, &c, according to the table of reflections and transmissions of light in Sir ISAAC NEWTON's Optics, . . . the spectra of those colours were of all others the most brilliant, and best defined; because they were combined of the reverse

spectrum of one colour, and of the direct spectrum of the other." Darwin then repeated the implication of Leonardo's thought, that color contrast is an aid to beauty, perhaps in part because the outlines are emphasized by it.

There was also, from Leonardo on down, a great deal of discussion of the cause of colored shadows, especially as to why shadows are blue at sunset. The topic was, of course, of great interest to painters. For the most part it was expected that the explanation would be physical, not a consequence of the properties of the human eye. Subjective explanations were not much in vogue before the nineteenth century. In 1782 there appeared a book of more than 200 pages by an author who signed himself "H. F. T." Called *Observations sur les ombres colorées*, it dealt entirely with an analysis of the physical conditions under which colored shadows arise. The nearest it came to a solution of the problem was to lay down the principle that the shadows can be of various colors and depend upon the relation of two lights to each other. Apparently the first person to get at the true nature of the colored shadows was Count Rumford (B. Thompson), who in 1794 found that he could produce the phenomena with a colored glass and two artificial lights in a dark room. When he put a yellow glass in front of one light, it colored the shadow from the other light yellow; but the objectively uncolored shadow from the first light then looked blue. Thus he synthesized the blue sunset shadows. Rumford found, however, that a blue glass reversed the phenomenon, creating a yellow shadow in the absence of any yellow light. Unfortunately he had no red and green glasses wherewith to complete the generalization; but he said: "I began to suspect that the colours of the shadows might, in many cases, notwithstanding their apparent brilliancy, be merely an optical deception, owing to contrast, or to some effect of the other neighboring colours upon the eye." This suspicion he established to his own satisfaction by showing that the induced color in the gray shadow vanishes when the shadow is viewed through a tube that excludes sight of the surrounding field and the neighboring shadow.

The facts of both brightness and color contrast seem to have been generally accepted from the beginning of the nineteenth century. Goethe and the other phenomenologists described many instances. Johannes Müller (1838) discussed "physiological colors produced by contrast," placing the discussion under the general heading,

"Of the reciprocal action of different parts of the retina upon each other." This phrase anticipates Hering's notion of contrast as showing that the retina acts as a whole. Thomas Young in 1807 had given what might be called a sympathetic theory of color contrast. He suggested that those parts of the retina which are adjacent to a region excited by colored light are affected "by sympathy," so that along with the excited area they lose part of their sensibility to light and are therefore more strongly affected by the remaining constituent parts of white light. Such a theory of the sympathetic adaptation of adjacent regions is invalidated by the fact that contrast colors appear before adaptation can be effective. In 1839 M. E. Chevreul published a book of more than 700 pages, *De la loi du contraste simultané des couleurs*, which is mostly concerned with principles of contrast as applied to paintings and other *objets d'art*, but which shows also that the principle had got itself established in scientific thought. It was the next year (1840) that Fechner's discussion of *Nebenbilder* firmly established the rule that contrast occurs between complementary colors and brightnesses.

In 1855 M. Meyer demonstrated color contrast by a simple use of colored papers. Placing a small patch of gray paper upon a piece of colored paper, he covered both with a piece of thin white paper through which the other papers could be faintly seen. The gray, of course, took on the contrast color that is complementary to its background color. Not only did this demonstration bring the facts of contrast under easy conditions of demonstration; it also related them to the phenomena of colored shadows, for it became clear that contrast is more effective with the shadow-like appearance of the colored papers seen through the white paper than with the stronger undimmed papers.

Helmholtz in 1860 discussed simultaneous contrast at great length. Although he warned against confusing it with that type of successive contrast which, depending upon fatigue and inadaptive eye-movement, seems to put a halo of complementary color about the boundary of a field, he admitted, nevertheless, the existence of simultaneous contrast that appears immediately and without eye-movement. At the same time, he was hard put to it to explain such a phenomenon. In his perplexity he resorted to the theory that the opposite color is seen as an illusion of judgment; a view which he generalized under the concept of unconscious inference (*unbewusster Schluss*), the notion that perceptions are

altered immediately and involuntarily so as to accord with the results of inference based upon past experience.

For all its usefulness, however, unconscious inference is but a negative explanation. It tells nothing positive about the nature of the phenomenon; it is a speculation which is essentially a confession of ignorance. Hering (1873 et seq.) supplied a positive concept: simultaneous light-induction. His *Zur Lehre vom Lichtsinne* is for the most part concerned with the problems of successive and

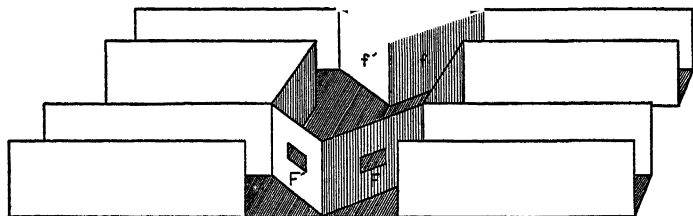


FIG. 21. APPARATUS FOR BRIGHTNESS CONTRAST BY HESS AND PRETORI (1894)

The variably illuminated surface f is seen as a small square field through the window in F ; and F , variably illuminated, becomes a surrounding field that induces a contrast effect in f . Similarly for f' and F' . The standard of comparison is f' , supported by some given value of F' , and f is matched in brightness to f' for different values of F .

simultaneous induction and of contrast. He added no crucial discovery to the facts of contrast, it is true, but he organized those facts under intelligible positive scientific rubrics, and he successfully removed from this chapter of visual psychology the concept of unconscious inference. He devised, in addition, several schemes for the demonstration of contrast effects. Among them there is the "Hering window" which gives remarkably well-saturated contrast colors of shadows cast in a dark room and produced by a double window provided with a gray glass and colored glass which can be changed. This device represents the final perfection of Rumford's old experiment.

The problem of contrast was now ready to pass over into the experimental and quantitative stage. It was Alfred Lehmann of Copenhagen who in 1886 undertook to study the application of the psychophysical method of mean gradations to certain problems of visual brightness. In the course of his study he worked out the relationship between the objective brightnesses of (1) a gray back-

ground that could be varied, (2) a variable gray disk seen against the background, and (3) a second gray disk that was varied to match the other disk whose subjective brightness was undergoing induction by its background. He came out with a simple relation, but this problem was put in more general form in the classical research of Hess and Pretori published in 1894.

They set up the apparatus which is shown in Fig. 21. The observer, looking at this apparatus from the front, sees two large square gray fields, F and F' , and in the center of each, a small gray field, f and f' , showing through windows in F and F' . All four fields are independently varied in brightness by lamps that illuminate them and move back and forth in tunnels running to the right and the left. The task is to equate f subjectively to f' for various brightnesses of the inducing background, F . Since everything is affected inductively by its surroundings, it is necessary also to control F' , the background of f' . With F' and f' set at some value to provide a standard of comparison, F is varied, and then there is determined the illumination of f that makes f and f' subjectively equal in brightness. The resultant generalization can be expressed in an equation. If F and f represent the illuminations of these fields (in hefners), then $f = \frac{1}{2}F + c$, a linear relation in which the induction is one-half, for a change in F induces half as much change in f . The value c depends upon the values of F' and f' , for the same relation holds between F' and f' and $c = f' - \frac{1}{2}F'$. The inductive coefficient, $\frac{1}{2}$, is dependent, of course, upon the particular dimensions of this apparatus. Nevertheless, we have with this research come a very long way from Leonardo's rules for contrast in painting or even from Buffon's observations about accidental colors.

For color contrast the classical study was made by Kirschmann in Wundt's laboratory at Leipzig in 1890. Kirschmann placed a gray ring on a color-mixer so that the ring took on a contrast color. This ring he matched by varying papers on another mixer outside the field of influence of the inducing color. In this manner he determined most of the important laws of color contrast—except the fundamental law that contrast is always in the direction of maximal opposition, for that principle had been clear since Fechner. These are Kirschmann's principal findings. (1) The intensity of simultaneous contrast increases, within the limits of the distinct perception of size of the resting eye, in proportion to the linear extension of the inducing part of the retina; that is to say, the larger

the perceived inducing field, the greater the effect. (2) Simultaneous color contrast is maximal when brightness contrast is absent or reduced to a minimum. In this principle lies, of course, one reason for the slow recognition of the facts of color contrast: the phenomena had seemed to occur adventitiously because they failed to appear in the presence of considerable differences in brightness. (3) Given equality of brightness, then the amount of induced color varies with the saturation of the inducing color, but there are diminishing returns in the induction of color, which Kirschmann found to be roughly proportional to the logarithm of the inducing saturation.

The other reason why the phenomena of color contrast were not understood sooner lies in the fact that induction is greatly favored by the elimination of contours. Here was the real point of Meyer's experiment in 1855. By spreading thin white paper over a piece of gray on a colored background, he tremendously reduced the saturation of the inducing background and yet, for all that, got a more intense contrast color in the gray. The reason escaped Helmholtz. He had to be content with the mere fact that color contrast actually seems best for faint inducing colors. Wundt, however, got the right principle in 1874: he noted that Meyer's thin white paper enhanced contrast, in spite of reducing the intensity of the colors, because it eliminated the contours that make of the gray paper an object separate from its background. In the same way Kirschmann's rotating disks aided contrast because rotation blurs contours. Though no one seems to have made much of this point at first, Titchener in 1901 put down the elimination of contours as the fifth formal condition of contrast. Nowadays Gestalt psychology makes use of this fact to show that contrast phenomena hold better within an integrated field than from one field to another. Hering said of contrast: the retina acts as a whole. Koffka said: contrast depends upon the manner of organization of the perceptual field.

In the present century the only important advances in the knowledge of visual contrast are these indications brought forward by Gestalt psychology to show that the laws hold in relation to the organized structure of the perceptual object, and that the neurological basis of the phenomenon may lie in the brain rather than in the retina. Certainly, if contrast is dependent upon perceptual organization, it is very probable that its laws represent the proper-

ties of central neural action. This view is, actually, a modern and sophisticated return to the original objection of Helmholtz, for he too believed that the laws of simultaneous contrast must depend upon the brain and not upon the action of the retina.

Indirect Vision

Maximal retinal sensitivity in daylight is limited to the fovea, which is not much more than $\frac{1}{1000}$ of the total area of the sensitive retina; yet before the nineteenth century there was almost no important consideration of the nature of vision as it occurs outside of the immediate field of fixation. Even Mariotte's discovery (1668) of the blind spot in this region gave no impetus to systematic investigation.

The first experiment on indirect vision, after Mariotte's, was due to the habitual curiosity of Thomas Young who, in 1801, determined the range of visibility of a light as he moved it away from the visual axis. He reported the limits of its visibility as 90° outwards, 60° inwards, 70° downwards, and 50° upwards, stating further that distinctness of perception begins to decrease at 5° or 6° from the visual axis. Upon the question of color-sensitivity Young did not touch.

In 1804 D. Troxler noted that peripheral images fade out very rapidly so that, in a series of white figures pasted on a blue ground, all but the fixated figure soon disappear. In other words, adaptation in indirect vision is rapid. Troxler seems to have thought that the particular color had something to do with the rate of adaptation, but it is not clear how.

It was Purkinje (1825) who definitely brought color sensitivity into the picture. He showed (1) that the extreme limits of vision are approximately what Young had already shown them to be, (2) that visual sensitivity diminishes continuously from the center to the periphery, (3) that colors tend to change in hue, at different places for different colors, and differently along the different meridians, and (4) that all colors tend to become gray in the extreme periphery. Though he is said thus to have indicated the existence of color zones on the retina, actually all he showed was that color sensitivity varies and diminishes toward the periphery. He coined the term *indirect vision*.

It was really Szokalsky (1842) who had the idea of zones. Work-

ing on the basis of a three-color theory of vision, like most persons after Young and before Hering, he reported that the periphery of the retina gives rise only to black and white sensations, that there is an intermediate zone which mediates blue and yellow as well as black and white, and finally that the central zone gives red in addition to these others. It must have been partly the operation of chance in his choice of stimuli that led him to a conclusion so close to the later requirements of the Hering theory.

The first thoroughgoing investigation of indirect vision was made by Aubert (1857 *et seq.*), who established most of the fundamental truth about this matter. Here is what he discovered.

(1) The range of chromatic sensitivity varies with the brightness of the color, for colors on black backgrounds are seen much farther toward the periphery than the same colors on white backgrounds. This difference is of the order of the difference between 16° (red on a white background) and 30° (red on a black background). (2) The range of sensitivity depends upon the area of the stimulus. The difference here is of the order of an increase from 16° for a red of 1 sq. mm. to 43° for a red of 256 sq. mm., whereas a stimulus of 1024 sq. mm. often appears still in its original color at the extreme boundary of the retina. (3) The rate of change of sensitivity is not uniform and differs upon different meridians. (4) In general, reds and greens pass gradually into yellow and then to gray as stimulation shifts from the center to the periphery, whereas yellow and blue pass directly into gray.

With so many conditions of variability Aubert did not think it possible accurately to map the retina into color zones. Such topography could have meaning only if it were accomplished for a set of colors of the same brightness, and Aubert did not believe that colors can be precisely equated in brightness. In striking certain averages of limits obtained under very various conditions Aubert did not find large differences for the four principal colors. The weight of his discovery thus told against the establishment of retinal color zones. He pointed out, moreover, that the difference between the center of the retina and its periphery is more one of degree of sensitivity than of kind. Even at the fovea colored stimuli are seen colorless if very small, if unintense, if weakened by contrast, or if weakened by adaptation. Thus the laws of the less sensitive periphery also apply to the more sensitive center.

Some years later (1872) E. Landolt demonstrated further the

wide range of color sensitivity for maximally effective stimuli, concluding—with Donders' concurrence—that no part of the retina is insensitive to color of any hue provided the conditions are made sufficiently favorable. (This conclusion also seems to follow from the fact that a large sheet of color, filling the entire field of vision, being large and therefore effective, does not allow the subject to see his own color zones; but this obvious observation appears not to have been made.)

At this time there began to be a great deal of experimental research on the problem of the color zones. Where there had been but a half-dozen papers in the preceding fifty years, now there were a dozen in the decade 1871–1880 and almost as many in each of the two following decades. Actually, as Aubert and Landolt had shown, there are no color zones, not in the sense of fixed boundaries which separate functionally or anatomically different parts of the retina. Sensitivity diminishes continually toward the periphery of the retina, and the zonal boundaries are no more real than the contours on a contour map. What happened was that interest in color theory became acute because of the conflict between Helmholtz's and Hering's theories, and it was thought that the establishment of fixed color zones could settle the argument one way or the other, depending upon how the boundaries lay with respect to one another.

Helmholtz's theory had held the field after 1852. Hering's theory is dated as of 1874. Both alike were founded upon Helmholtz's extension of Johannes Müller's doctrine of specific energies to the problem of the different hues. Both appealed to the laws of color mixture to explain how an infinitude of perceptible colors can be mediated by very few retinal processes. Helmholtz for his part supposed the existence of three specific energies arising respectively from the excitation of three substances in the retina—the 'red,' 'green' and 'violet' substances. All the colors, including white, come, he thought, from different admixtures of the excitations of these three. Hering, on the other hand, supposed that there are six specific energies arising from three substances, each capable of two antagonistic and mutually incompatible processes—the 'red-green,' 'yellow-blue' and 'white-black' substances. To him it seemed that the facts of indirect vision could furnish a decision between these two theories by showing whether red and green are inseparably related, as they should be if they depend upon the

presence of the same substance (Hering theory), or whether they are separable, as they should be if dependent upon different substances (Helmholtz theory).

The proponents of the Helmholtz theory held that the 'red' substance is missing or diminished in the periphery of the retina. Some of the arguments were involved, but the theory was supposed to be supported by Aubert's finding that red and green pass through yellow as they fade out to gray at the periphery, whereas yellow and blue become gray without change of hue. Schelske in 1863 and Woinow in 1870 got experimental results similar to Aubert's and used them to support the Helmholtz theory before Hering's theory had clearly come into the field. The diminution of the 'red' substance would, of course, explain the fading of red to yellow, even though it would not explain in any simple manner why green passes through yellow to gray.

Hering's theory seemed to require that the red and green zones should coincide, and also the blue and yellow zones. Szokalsky (1842) had already reported such a relation. Nevertheless it had become plain that a color-sensitive zone can be any size at all, depending upon the effectiveness of the stimulus used. The only possible meaning that can be given to coincidence between the red and green zones is, therefore, that they should be determined with equally effective stimuli. Aubert, who had realized that point, had thought that the heterochromatic equation of colors could not be established. In 1881, however, seven years after Hering had promulgated his theory, Ole Bull saw that such a hypothesis also requires the existence of four critical invariable colors which do not change in hue from the fovea to the periphery. Not only should yellow and blue change directly to gray, but there should also be a red and a green (the hues associated with the isolated action of the 'red-green' substance) that do not change in hue before they become gray. Bull found two such colors, a bluish green and a purplish red which, with pure blue and pure yellow, he called the "physiologically pure" colors. Hess in 1889 confirmed these results about the invariable hues and determined their spectral wavelengths as follows: blue, 471 m μ ; green, 495 m μ ; yellow, 574.5 m μ ; red, non-spectral. Introspectively this yellow and this blue are pure, whereas the red and the green are bluish. Hegg (1893 *et seq.*) and Baird (1905) obtained similar results.

When these four investigators used what they thought were

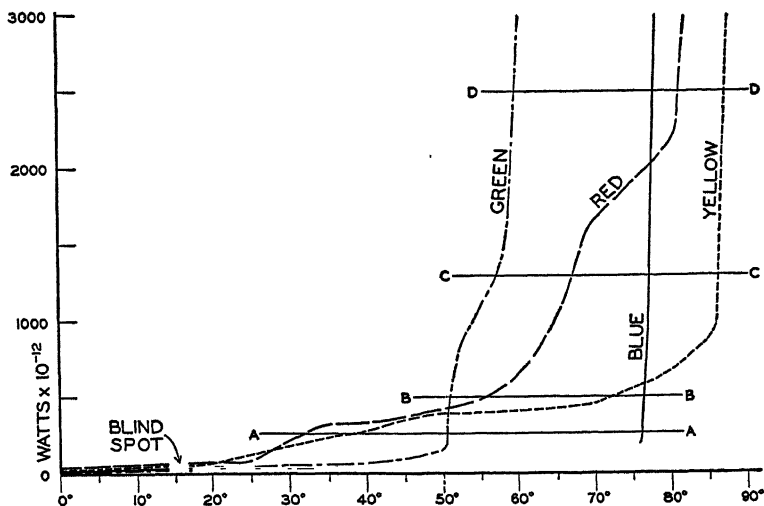


FIG. 22. COLOR ZONES, AS SHOWN BY CHROMATIC THRESHOLDS OF FERREE AND RAND (1919)

The thresholds are plotted between energy (watts) and eccentricity (degrees from the fovea on the nasal meridian). The stimuli are 670 m μ (red), 581 m μ (yellow), 522 m μ (green) and 468 m μ (blue). A horizontal line drawn at any level shows the limits of the color zones (for these particular stimuli) at that level of energy. The order of limits for the four zones varies with the energy level, and in general zones do not coincide for equal energy. Compare the different orders at the levels represented by the horizontal lines AA, BB, CC, DD. All these curves, except the one for green, extend as far out as 90° when they are continued to higher energy levels.

equivalent values of the four invariable colors, they got remarkable zonal coincidences to support the Hering theory. Typical limits (in degrees from the visual axis) occurred for the temporal meridian as follows:

	<i>Red</i>	<i>Green</i>	<i>Yellow</i>	<i>Blue</i>
Bull	20	20	30	29
Hess	21.2	21.6	35.6	35.0
Hegg	20	20	41	42
Baird	32.5	31.0	47.0	48.2

In this table one cannot, of course, compare Hess with Baird, since their stimuli were not equated. The point is that, for each in-

vestigator, the values for red and green are close together, the values for yellow and blue are close together, but the two pairs are far apart.

More recent times have witnessed the evaporation of interest from these classical, naive theories, and with it the disappearance of the problem of zones. No longer is it clear, in fact, just what would be the physiological significance of the coincidence of zones, since any zones can be made to coincide or to differ in any desired order simply by choosing stimuli of the proper relative effectiveness. Twisting the argument around, one can even say that the effectiveness of each stimulus is measured by the degree of retinal eccentricity at which its color still remains visible. At any rate modern research, having largely abandoned the theories, is primarily concerned with the measurement of the phenomenal effects of the carefully controlled variation of the physical dimensions of the stimulus.

An example of the modern work on indirect chromatic vision is the research of Ferree and Rand (1919), who determined chromatic thresholds for certain spectral colors along certain meridians. Fig. 22 shows how these thresholds vary for four colors along the nasal meridian. It will be seen that with increasing level of energy the order of the limits for these four colors alters from *RYGB*, through *GRYB* and *GRBY*, to *GBRY*. The more the retina is studied, the more it is found to vary irregularly and in no such simple manner as the early theories presupposed.

Luminosity of the Spectrum and the Purkinje Phenomenon

The distribution of luminosity in the spectrum changes with the total level of energy in the spectrum, and this fact accounts for the Purkinje phenomenon. Thus the history of the luminosity curve, the Purkinje phenomenon, and the differences between photopic and scotopic vision is single and must be discussed all at once.

That the colors of the spectrum are not equally bright is obvious. Newton wrote: "The most luminous of the Prismatic Colours are the yellow and orange. These affect the Senses more strongly than all the rest together, and next to these in strength are the red and green. The blue compared with these is a faint and dark Colour,

and the indigo and violet are much darker and fainter, so that these compared with the stronger Colours are little to be regarded." The full import of this statement Newton could not see because he thought of light as something that was, by its very nature, visible. It was a long time before it was realized that there is a continuum of energy with maximally visible light in the yellows, minimally visible light in the extremes of red and violet, and invisible light beyond in the infra-red and ultra-violet regions.

In 1815 Fraunhofer, studying the problem of the construction of achromatic lenses, realized that he must know the relative intensities of the spectral colors, since the chromatic aberrations of the stronger colors have the greater need of correction. Hence he arranged an apparatus with a divided field, in which any chosen part of the spectrum could be seen on one side and a white light of variable intensity on the other. By varying the intensity of the artificial white light, he undertook to make it match the various parts of the solar spectrum in brightness. In this way he found the maximum brightness to lie in the yellows, "about a third or a quarter of the way from D toward E" in terms of the letters for the spectral lines which still bear his name. Actually this point is about 575 mμ, at the point for pure yellow. The ratios of the intensities of the white light equated to the colors in the solar spectrum were, according to Fraunhofer's average of four determinations, as follows:

Spectral line:	B	C	D (Max.)	E	F	G	H
Relative brightness:	.032	.094	.640	1.000	.480	.170	.031 .0056

This curve has the general shape of the present-day luminosity curve. It does not match it exactly, nor should it match perfectly, for the two curves do not express identical properties of vision.

Purkinje's classical observation was reported in 1825 in the second volume of his *Beiträge*. It is a bit of correct phenomenological reporting which takes up a mere three paragraphs and seems not to have been regarded by its author as of exceptional importance. After noting that "objectively the degree of illumination has a great influence on the intensity of color quality," he went on to describe how the colors emerge from darkness at dawn as the day begins to break. At first there is only black and gray, with the red appearing blackest. Then, as the light increases, the blues are the first colors to appear in their own hues. Purkinje thus described

the 'colorless interval' for low illumination, and the relative darkness of the red end of the spectrum and lightness of the blue end. Because the greatest relative change between daylight and twilight vision lies in the extremes of the spectrum, it is the twilight darkness of the reds and brightness of the blues that are most likely to be noticed.

During the next forty years there were various verifications of the Purkinje phenomenon by Seebeck (1837), Dove (1852), and Grailich (1854). Aubert described the phenomenon in 1865. Most of this discussion, however, was incidental, failing to advance the problem.

In 1869 Vierordt measured the brightness of the solar spectrum and obtained a curve closely resembling Fraunhofer's. Fraunhofer had compared a white with a color in respect of brightness; that is to say, he had attempted a direct heterochromatic equation of brightnesses. This judgment is difficult, so difficult that Helmholtz said for himself that he could not make it with accuracy. Vierordt, therefore, tried to get the brightness function indirectly by determining the amount of white light that must be mixed with each spectral color to give a just noticeable diminution in the strength of the color. This method assumes that the effectiveness of a stimulus for color is proportional to its resistance to perceptible change by a dilutant of constant character, an assumption natural enough in the days when Fechner's law was in the air. The importance of Vierordt's determination, and of Dobrowolsky's in 1872, is to show that there was then a felt need for exact quantification.

Inasmuch as there was no method for determining the absolute energy of the spectrum, all the measurements of the luminosity of the spectrum had up to this time been relative. In 1883, however, Langley succeeded in measuring the distribution of energy in the solar spectrum by means of a bolometer. For the first time now it was possible to determine the absolute sensitivity of the eye for different wave-lengths. It was Strenger who made this general point about the eye in 1887. If the intensity of the spectrum is greatly reduced, it shrinks to a small luminous band near green. Hence, Strenger argued, the eye is more sensitive to green than to red. Newton had said that yellow is brighter than red. Fraunhofer had said how much brighter. The problem had not, however, been envisaged before as a problem of relative sensitivities, of the eye's sensitivity diminishing from a mid-point in the spec-

trum to the extremes where the limits of visibility mean merely that the eye is completely insensitive to the invisible 'light' that lies beyond the ranges of the spectrum.

Langley himself in 1888 worked out this function for the sensitivity of the eye. In preference to the direct heterochromatic equation of brightness, he used a method of acuity in which the relative intensities of two wave-lengths are varied until the two lights give equal visual acuity. Since he knew the absolute intensities of each wave-length by way of the bolometric measurements, it was now possible for him to plot the energies that would give, for all the spectral hues, equal acuities and, therefore, presumably equal brightnesses. Such a curve would be U-shaped with a minimum in its center. Langley, however, started the modern practice of taking the reciprocals of these energies as measures of the sensitivity of the eye; as a consequence, his curve, like all the others after it, is bell-shaped.

The most important investigation in this field of visual sensation was certainly König's, for he systematically determined brightness and sensitivity of the eye as a joint function of wave-length and intensity of light, thus bringing the brightness curve and the Purkinje phenomenon under one large generalization. König found the distribution of brightness in the spectrum for the eight levels of intensity that had to one another the ratios 1: 16: 256: 1024, and so on up to 262,144. The lowest intensity was approximately the absolute threshold. At each intensive level he equated in brightness thirteen different wave-lengths to the brightness of 535 m μ . His results take the form of the set of curves in Fig. 23, which are for König's own eye. The curves are all referred to 535 m μ as unity, which was kept at a constant intensity. For comparison colors darker than this standard, the collimator slit had to be opened wider, to make the intensity high enough to get a match of brightnesses. A "brightness curve" plots the reciprocal of these observed slit-openings. König's curves show how the point of maximal brightness shifts from about 615 m μ at higher intensities to about 530 m μ at lower intensities. In the set of curves of Fig. 23 the intermediate intensities seem to show a combination of the two distributions, a form of transition which has not, however, generally been found.

That these facts should now come to appear as functional differences between the rods and cones was quite natural. Schultze,

who had distinguished between the rods and cones in 1866, had also suggested that the rods function for achromatic vision in dim illumination. Others had supported this view, and there had

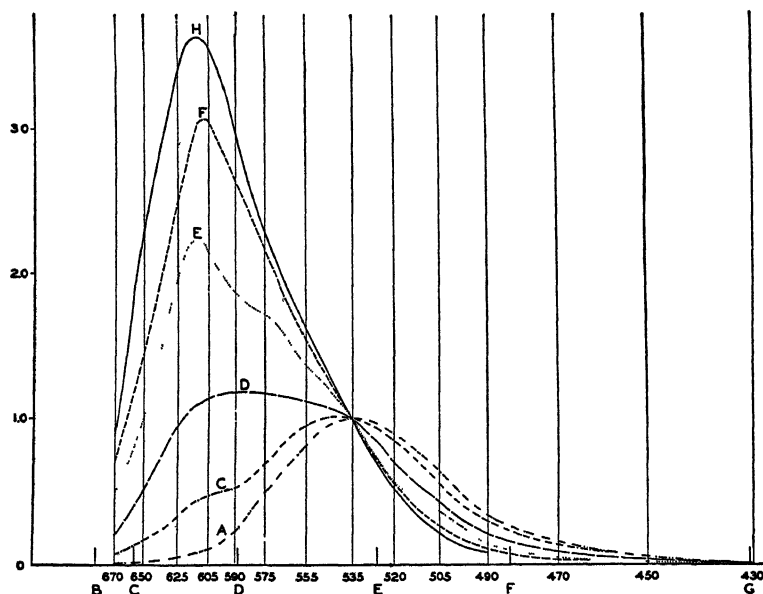


FIG. 23. KÖNIG'S CURVES OF BRIGHTNESS FOR HIS OWN EYE (1891), SHOWING DISTRIBUTION OF SENSITIVITY FOR THE SPECTRUM AT DIFFERENT LEVELS OF INTENSITY, AND THUS THE PURKINJE PHENOMENON

The curves are for six levels of intensity, in which the lowest, A, is about liminal, and the highest, H, is about 262,144 times the lowest. The curves really measure sensitivity, with 535 mμ taken as the point of reference, because they are the reciprocals of the amount of energy (width of collimator slit) necessary to make any hue appear just as bright, as the standard; but, since these ratios indirectly show the relative brightnesses which wave-lengths of equal energy would have, König called them "curves of brightness."

been a great deal of discussion about the function of the visual purple. In 1894 von Kries put the facts together into his "duplicity theory," which we shall have to consider in the next chapter. It is enough for the moment to see that König's data and von Kries's theory belong together, and that König's curves came eventually

to be interpreted as two kinds of sensitivity curves, one for rods and one for cones.

In a large literature there appear to have been after König three main problems: (1) the relation of the Purkinje phenomenon to the fovea, (2) the dependence of the Purkinje phenomenon upon dark adaptation, and (3) the precise establishment of the luminosity curve as a quantitative standard.

If the Purkinje phenomenon depends upon the differences in sensitivity of the rods and cones, then it ought not to occur in the fovea where there are no rods. On this problem there have been many experiments, with conflicting results. Koster (1895) and Sherman (1898) reported finding the phenomenon in the fovea; other studies, including a very careful experiment by von Kries and Nagel (1900), contradict these findings. In this case the burden of proof does not lie with the negative finding. If the fovea is defined too generously so that the region of stimulus-projection is not strictly within the rod-free area of the retina, or if fixation is not rigidly maintained so that the stimulus-projection wanders outside the rod-free area, then the Purkinje phenomenon could appear as an artifact. Von Kries and Nagel failed to find it with careful fixation within a two-degree field, and this result seems likely to remain correct.

Hering (1895) argued that the Purkinje phenomenon is the consequence, not of the low illumination of the stimulus, but of the state of dark-adaptation of the eye. Thus he was opposed to the Helmholtzian tradition which found its culmination in König's work. That dark-adaptation favors the Purkinje phenomenon had long been known. Purkinje himself described the phenomenon for the twilight of dawn (when the eye is, of course, dark-adapted) instead of for the twilight of evening. Parinaud in various articles (1881 *et seq.*) had argued that the visual purple is essential to dark-adaptation. Now (1895) he held that the visual purple is a condition of the Purkinje phenomenon. Although there were in the 1890's some researches indicating the similarity between the light-absorption curve of visual purple and the sensitivity curve for faint light, the confirmation of this view has been recent (Hecht and Williams, 1922). Since the absorption values of the visual purple would tend to create the Purkinje shift and since the visual purple is associated with the rods, the theory is convincing.

That does not mean, however, that Hering was right and König wrong, but simply that König and the others of the Helmholtzian school measured the sensitivity of the functioning rods, whereas dark-adaptation is a condition for their functioning. It is generally supposed now that cone and rod vision overlap throughout a very wide range of illuminations, and that any person indoors is somewhat dark-adapted.

So well accepted nowadays is the dual functioning of the eye that Parsons (1915) introduced the term *photopia* for daylight (presumably cone) vision and *scotopia* for twilight (presumably rod) or dark-adapted vision. The 'brightness curve,' 'sensitivity curve' or 'luminosity curve' has come fairly generally to be known now as the *visibility curve*. In 1920 the Illuminating Engineering Society of America adopted a standard visibility curve for photopic vision, a table of coefficients that can be applied to an energetic spectral distribution in order to get its total photometric (brightness) value. Hecht and Williams (1922) have determined both photopic and scotopic visibility curves, the values for which are now often used in the computations for other research. In sharp contrast with the untechnical phenomenology of Purkinje and the simple computations of Fraunhofer are the assurance and complication of modern technique.

The eye (like the ear; see p. 339) is a very sensitive organ. Langley's experiment showed that the retina at 550 m μ can be excited by three-thousandths of a millionth of an erg (3×10^{-9} ergs) of light. Others have obtained similar values. The latest for 510 m μ , got by Hecht, Schlaer and Pirenne in 1941, lies between 2×10^{-10} and 5×10^{-10} ergs at the cornea and even less at the retina. This minimal value represents only from five to seven quanta of light.

Color Blindness

No other topic in the field of color vision has created so much interest as color blindness. Because it is a common defect that leads to amusing mistakes which may nevertheless exist unsuspected in its possessor, the topic has interested the layman. For the same reasons, it has interested the general scientist, who was thus sometimes drawn into investigating it or inventing a test for it. Psychologists it interested because it was obviously related to the normal color blindness of the peripheral retina and might be re-

lated to the normal color blindness of scotopic vision, and because it seemed to bear even more directly upon the question of retinal color theory, upon the decision between the Helmholtzian three-color theory and the type of theory that was held by Hering. Yet not only did these theories stimulate interest in color blindness; they also biased the findings. On the Helmholtzian view there would be three color substances, one for red, one for green, one for violet. Since presumably any one of them might alone be missing, there could be red-blindness, green-blindness, and perhaps also occasionally violet-blindness. Hering's theory required red-blindness and green-blindness to go together; that is to say, there should be blindness to both red and green when these colors are affected at all. It was not so much that the proponents of one view or the other found what they looked for; more often they presupposed one theory or the other and analyzed their results in respect of the theoretical hypothesis. At any rate, interest was keen. König, in his bibliography of vision in 1896, gave about twelve hundred citations of the literature simply on the topic of color blindness—about one-seventh of all his references. He found eighty-eight papers to cite for the year 1881, a year which represents the peak of prolificity.

Although color blindness is probably nothing new in the human race, it is hard to know what account of deficient visual capacity should stand as the first recorded instance of this defect. For example, there is the letter printed by the Royal Society from "the great and experienced Oculist, Dr. Turberville of Salisbury," who reported in 1684 the case of a girl who saw "no colour beside Black and White." That phrase, however, if correct, indicates total color blindness, whereas it is partial color blindness that, being common, has created the most interest. The first indubitable case of partial color blindness was reported in 1777 by Joseph Huddart to the chemist Joseph Priestley (the author of the book on vision, light and color in 1772). Huddart described the case of a shoemaker who had observed that "other persons saw something in objects that he could not see." In seeking the owner of a child's stocking, which he had found, he noticed that "people called it a *red* stocking, though he himself did not understand why they gave it that denomination." As a child he had been able to distinguish cherries on a tree only at close hand by their shape, whereas other children could see them at a distance. After this case of Huddart's

there appeared during a few years descriptions of several other cases of color blindness. Scientific interest, however, was not fully aroused until Dalton described his own case.

In 1794 John Dalton, a young man of twenty-eight who was shortly to become the author of the atomic theory in chemistry and a scientist of great influence, presented to the Manchester Literary and Philosophical Society an account of his own color deficiency. It is easy to see, when we read his report, why so great an incapacity existing unsuspected in a person of ability should excite interest. For instance, in discussing the reds, Dalton wrote: "All crimsons appear to me to consist chiefly of dark blue: but many of them seem to have a tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud*, which were very nearly alike. Crimson has a grave appearance, being the reverse of every shewy and splendid colour. Woollen yarn dyed crimson or dark blue is the same to me. . . . The colour of a florid complexion appears to me that of a dull, opaque, blackish blue, upon a white ground. . . . Blood appears to me red; . . . to me it is not unlike that colour called *bottle-green*." Dalton gave his defect scientific status by studying it and describing it. He concluded that "in the solar spectrum three colours appear, yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind." Thus later it was possible to conclude that Dalton had "dichromic vision"; that is to say, blind to the reds and greens, he saw only the yellows and blues. Dalton, supposing that his defect might be due to the absorption of certain colors by the ocular media, provided for a post-mortem examination of his eyes. The examination was made and his theory disproved.

Still other cases of color blindness were brought to the attention of the scientific world in the early part of the nineteenth century. Goethe discussed two instances in *Zur Farbenlehre* (1810). The term *Daltonism* was first applied to the defect in 1827.

As we have already seen in the last section, Seebeck in 1837 confirmed Purkinje's observations on twilight vision. This study of Seebeck's was in addition really a thoroughgoing investigation of color deficiency. In it he argued that color blindness is of two kinds: one, where the defect is associated with the visibility of the entire spectrum; the other, where it involves the shortening of the spectrum at the red end. Since red-blindness would give a shortened spectrum, such a view may be taken as an anticipation of the

distinction between green-blindness and red-blindness respectively. Actually we know now that Seebeck's dichotomy was the anticipation of the distinction between deuteranopic and protanopic red-green blindness, as von Kries established the facts much later.

Though it is easy to find out what portions of the spectrum are visible to the color-blind, as Seebeck did, it is hard to know just what it is that the color-blind person sees in the visible portions. Nevertheless, in 1845 Sir John Herschel formed the inference that the color blind have what he called "dichromic vision," that is to say, they see in the spectrum only two colors, yellow and blue. Dichromic vision is, in short, red-green blindness, and Herschel's view is consistent both with Dalton's confusions (bottle-green blood) and with Dalton's conclusions (that he saw yellow, blue, and a purple which is a weak blue). The history of the knowledge of color blindness might have been straight going had it not now become complicated by the Young-Helmholtz 'trichromic' theory of vision.

Helmholtz advertised Thomas Young's views and formulated the three-color theory in 1852. Clerk Maxwell supported Helmholtz. In 1855 George Wilson published a book, *Researches on Colour-Blindness*, in which he supported the three-color theory and argued against Herschel's dichromic theory. Maxwell had written him a letter supporting his view and describing tests of color blindness with rotating colored disks ("Maxwell disks"), and Wilson published this letter in his book. Wilson's argument was that Dalton was green-blind, but not red-blind—this in spite of Dalton's confusions of red with green and in spite too of the incident of his wearing about the streets his scarlet doctoral gown conferred upon him by Oxford, wholly unaware of its striking incongruity with his habitual Quaker garb. This belief of Wilson's about Dalton was a case of theory affecting conclusions. Most people were coming around to a belief that there are only three kinds of nerve fibers originating in the retina and, in consequence, that the eye can 'see' only three colors, red, green and violet. Hence it would follow that color blindness must be due to the lack of one or more of these colors, like red or green, but hardly to a lack of both, for then all vision would be violet. Thus it would not be possible to perceive yellow, a mixture of red and green, if one were blind to both red and green.

At this point in the history terminology becomes confusing. Helmholtz and Maxwell supported a theory of normal vision that others sometimes called "trichromic." Herschel for his part held that inasmuch as only two colors are seen, color-blindness is "dichromic." The implication of Herschel's view is that normal vision is tetrachromic, but that in dichromic vision two colors have been lost. On the Helmholtz-Maxwell view likewise it would be logical to say that a color-blind person has 'dicromic' vision, since he would be supposed to see one less color than the three which are normal. In actual usage, however, the word *dichromic* was employed as meaning that only yellow and blue are seen.

The year after the publication of Wilson's book, Pole described his own color-blindness, reaching the conclusion that his vision was dichromic; in other words, though blind to red and green, he could see yellow and blue. His case continued in the literature. Maxwell thought him red-blind, whereas Holmgren subsequently classified him as green-blind. Much later (1893) Pole attempted a summary of the whole argument about himself and persisted in his former notion that he was red-green-blind, whereto Holmgren now agreed.

In 1880 the dichromic theory received support from von Hippel's description of a case of monocular color blindness. Here at last was the opportunity for getting a direct comparison between what is seen by the color-blind eye and by the normal eye. There had been another such case reported earlier and there were presently to be still others, but von Hippel's account was especially clear and convincing. His subject could see all colors with his left eye. He could see yellows and blues with his right eye, but not reds and greens. Von Hippel put him through eight tests. Holmgren at first thought him red-blind, only later agreeing that he was blind to green also.

The practical concern with color blindness as well as the theoretical interest in it created a need for tests. The earliest was the confusion test, which simulated the conditions originally leading to the discovery of color blindness. Wilson had shown that objects can be named correctly as to color on the basis of other than their chromatic attributes: grass is green. Thus good tests require objects which are different in color and yet alike in such other respects as shape, texture, touch and smell. Wilson had for this purpose used colored yarns, a procedure which Holmgren developed

in 1874 to create the test that bears his name. The contrast colors of shadows had also been used to test for color blindness, for shadows meet these specifications perfectly. In 1880 Stilling established what he called "pseudo-isochromatic" tests, in which patterns of spots, varying in hue and brightness, present a figure that stands out from its background in terms of hue alone. These charts can be constructed so that the figure will be seen only in normal vision, or else only in color blindness of one type or the other. The Ishihara tests are the modern form of Stilling's invention, and the Nagel cards were a variant of it. Lord Rayleigh in 1881 set up the conditions for what was called later the "Rayleigh equation," which specifies crucial conditions for matching colors that involve both red and green in order to measure the relative strength of these two factors in color mixing. Though the test presupposed the three-color theory, according to which red and green vary independently, actually his results showed, not only that the two may vary independently, but also that color blindness is not an all-or-none affair.

While these somewhat uncertain tests might have to serve for determinations of color blindness in railway engineers and for other practical uses, it was obvious that, before color blindness could be understood scientifically, careful quantitative measurements were needed. In this work König, who belonged to the Helmholtz school, was the pioneer. The first important research he published with Dieterici in 1886. They undertook to measure for normal and color-blind persons the intensive distribution of the *Grundempfindungen* in the spectrum. Whether one held to Helmholtz's or Hering's views about the physiological action of the retina, Newton's great fact remained true, that one can get all the colors from a mixture of three properly chosen wave-lengths. König and Dieterici undertook to find what three basic wave-lengths would be most satisfactory to use for a system of mixtures, and how the response of the retina to intermediate wave-lengths is related quantitatively to its response to these three. In other words, they undertook to work out, by way of spectral color matches, sensitivity curves for three hypothetical basic frequencies which correspond to the three modes of excitation posited by the Young-Helmholtz theory. Inasmuch as they confirmed Newton's assertion that any color can be got from a proper combination of a proper three colors, they therefore proceeded to char-

acterize normal vision as *trichromatic*. Thus persons with normal vision came to be called *trichromats*. They also found—and this is the important matter for our present discussion—that partially color-blind persons can match the entire spectrum with the proper combinations of a proper two colors; hence partial color blindness they called *dichromatic* vision and the persons *dichromats*. (So it happens that the “dichromic vision” of Herschel and the “dichromatic vision” of König have almost opposite meanings, for Herschel meant that one sees *two* out of a total of *four* colors, and König meant that one gets all the colors he sees from *two* out of a total of three stimuli.) Even though Hering remained critical, the importance of König’s work was recognized, not only by Helmholtz, but also by von Kries and Nagel. As a consequence the weight of opinion was thrown to the color-matching technique, which in turn seemed, though incorrectly, to imply the truth of Young-Helmholtz theory.

The effect of von Kries was in many ways that of mediator between Helmholtz and Hering. Here in the matter of color blindness he could accept the finding about dichromatic vision, and still be concerned about what the color-blind actually see. It was he who directed attention again to the distinction that had been made by Seebeck between the color blindness of the shortened spectrum and that of the full-length spectrum. As we have seen in the last section, in 1894 von Kries propounded his duplicity theory concerning the rods and the cones and the different brightness distributions, as found by König. In 1896 von Kries and Nagel went on to measure the brightness distributions of some dichromats. (Nagel himself was a dichromat.) Thus it became clear that Seebeck’s two kinds of color blindness were accounted for by the fact that some dichromats have the normal (photopic) distribution of brightnesses for the spectrum, whereas others have the Purkinje (scotopic) distribution. The latter kind of color blindness von Kries called *protanopia*, and the persons *protanopes*, because he thought of the scotopic type of sensitivity as more primitive. The other kind of color blindness he called *deutanopia* and the persons *deutanopes*, because he regarded this type of sensitivity as derived. A protanope, then, has his red so dark that red appears to be missing; his spectrum is shortened and he might be called red-blind. A deutanope sees the whole extent of the spectrum; the reds appear as poor yellows since the basic red is

not in the spectrum, and the greens (darker than in the protanope) so gray that the subject might be called green-blind. It was von Kries's point that protanopes and deutanopes alike are blind to both red and green.

It was just after this time that color weakness was established as a common phenomenon. Although it was now clear that both deuteranopia and protanopia are forms of red-green deficiency, it remained for Nagel to show that there are many intermediate anomalous cases, where vision is trichromatic, but where the subject has diminished sensitivity to green *or* to red. Von Kries called these cases "deuteranomalous trichromacy" and "protanomalous trichromacy." The anomalous trichromats turn out to be fully as numerous as the dichromats. Thus Hering was justified in his contention that dichromatic vision is blindness to both red and green, but Helmholtz was also justified in claiming that sensitivity to red and to green may nevertheless vary independently. It was Lord Rayleigh (1881), as we have seen, who first made this discovery. Since then S. P. Hayes (1911) has shown that dichromatic vision can be regarded as the limit of the variation of trichromatic vision, a view to which S. W. Terman (1929) has added support.

In reviewing the development of knowledge about the usual forms of partial color blindness, we have had to ignore many isolated facts of minor significance in the trend of thought. (1) There is a literature of what is called by the Hering school blue-yellow blindness (supposing that the blue-yellow substance is lacking from the retina) and by the Helmholtz school violet or blue blindness (assuming that the violet substance is lacking from the retina). It is a confusing literature, wherein cases are not clearly established and theory has affected conclusions on many occasions. (2) For all that there is no doubt about the existence of total color blindness or *monochromatic* vision, not more than eighty cases had been reported when a survey was made some years ago, and there are complicating factors that have prevented any positive conclusions on this subject. Since foveal vision is diminished, there is the implication that the monochromats may have to rely more on their rods than do dichromats. (3) There are reports of the sudden acquisition of partial color blindness as a result of accident or eye-strain, but these cases lack the detailed investigation that might make them fit the total picture. (4) It is established that dichromacy is much more common in men than in

women, and that it follows approximately a sex-linked form of inheritance. Later all these facts may enter the history of the psychology of vision in some dynamic way, influencing thought, occasioning a theory or explaining some other phenomenon. At present, however, they are about as isolated as was Seebeck's classification of the two kinds of color blindness in 1837 before von Kries found its significance sixty years later.

Notes

Joseph Priestley's historical text is *The History and Present State of Discoveries Relating to Vision, Light and Colours*, 1772, 812 pp. The book examines the history of vision from the objective or physical point of view. The history of the discovery of subjective visual phenomena is brought together by J. Plateau, *Bibliographie analytique des principaux phénomènes subjectifs de la vision*, depuis les temps anciens jusqu'à la fin du XVIII^e siècle, suivie d'une bibliographie simple pour la partie écoulée du siècle actuel, *Mém. Acad. roy. Belg.*, 2 ser., 42, 1878, the first five memoirs, separately paged, 266 pp. altogether. The *bibliographie analytique* gives full abstracts of more than a hundred papers before 1800, and the reader must go to this source for the references and for the full account of the discoveries and opinions. The *bibliographie simple* is just a list of more than 700 papers after 1800 with simple annotations for some items. Later there is, of course, A. König's classified bibliography of 7833 titles in H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, 2 ed., 1896.

After-Images

For the history of discovery, see Plateau's *Bibliographie*, *op. cit.*, the first three memoirs; also H. L. F. v. Helmholtz, *Physiological Optics*, trans. 1924, II, 261 f. For the references to Ptolemy, Aguilonius, de

Peiresc, Boyle, de Godart, etc., see Plateau.

Athanasius Kircher's book is *Ars magna lucis et umbræ*, 1646, [n.v.]. For his wager about the after-images, see p. 162. His theory that the eye might give out light as well as absorb it was not strange for that time. Even as late as 1834 a man claimed in court to have recognized an assailant who struck him in the eye in the dark because the blow on the eye created a flash of light sufficient to illumine the features of the assailant; see J. Müller in his *Jahresbericht* for 1833, *Arch. Anat. Physiol. Leipzig*, 1834, 140 f.

For Newton's experiments on the spectra of the sun's image, see D. Brewster's publication of Newton's letter to Locke, written in 1691: I. Newton, *Experiments on ocular spectra produced by the action of the sun's light on the retina*, *Edinb. J. Sci.*, n.s. 4, 1831, 75-77.

The later papers mentioned in the text are: J. Jurin, *An essay upon distinct and indistinct vision*, which is printed as an appendix to Robert Smith, *A Compleat System of Opticks*, 1738, Bk. 4, 115-171, esp. 168-171; G. L. L. Buffon, *Dissertation sur les couleurs accidentelles*, *Mém. Acad. Sci.*, 1743, 147-158; Benjamin Franklin, letter to Lord Kames, June 2, 1765, printed in the later editions of [New] *Experiments and Observations in Electricity*, 4 ed., 1769, 469, [n.v.], or 5 ed., 1774, 487 f., and reprinted in the A. H. Smith and the

J. Bigelow editions of Franklin's works (it was just a paragraph, but it was an important fact noted by an important man, and it got attention); Charles Scherffer, *Abhandlung von den zufälligen Farben*, 1765, [n.v.], French trans., *Observations sur la physique* (de Rozier), 26, 1785, 175-187, 273-290; R. W. Darwin, New experiments on the ocular spectra of light and colours, *Phil. Trans.*, 76, Pt. 1, 1786, 313-348.

The general orienting articles by Plateau and Fechner on this topic are: J. Plateau, *Essai d'une théorie générale comprenant ensemble des apparences visuelles qui succèdent à la contemplation des objets colorés, et de celles qui accompagnent cette contemplation*, *Mém. Acad. roy. Belg.*, 2 ser., 8, 1834, 68 pp. (separately paged); G. T. Fechner, Ueber die subjektiven Complementärfarben, *Ann. Phys. Chem.*, 120, 1838, 221-245, 513-535; Ueber die subjektiven Nachbilder und Nebenbilder, *ibid.*, 126, 1840, 193-221, 427-470.

Adaptation

There are three German words. (1) *Akkommodation* was fixed by Helmholtz as the adjustment of the lens and ciliary process to changing focal length, but Johannes Müller and others before Helmholtz used the word in the sense of (2). (2) *Adaptation* was fixed by Aubert for adjustment to the intensity of stimulation, and after Hering it came to mean adjustment to the intensity of white, black or color; nevertheless, before Aubert it was sometimes used in the sense of (1). (3) *Anpassung* means biological adaptation of the race or individual to his environment, but Hering used it in the sense of (2). Adaptation (2) is also sometimes called *Umstimmung*.

In general on the facts and history of the experimental psychology of adaptation, see A. Tschermak, *Die Hell-Dunkeladaptation des Auges*

und die Funktion der Stäbchen und Zapfen, *Ergebnisse der Physiologie*, I, Pt. 2, 1902, 694-800 (bibliography of 274 titles).

Hermann Aubert's pioneer book is *Physiologie der Netzhaut*, 1865, but his *Grundzüge der physiologischen Optik*, 1876, was also very important. Aubert established the concept of adaptation, invented the episcotister (adjustable rotating sectors for diminishing intensity of a visual stimulus behind them), and invented the Aubert diaphragm (square diaphragm adjustable to any area because it consists of two overlapping pieces that slip together or apart along a diagonal of the square as an axis).

Ewald Hering wrote *Zur Lehre vom Lichtsinne* in 1872-1874 (2nd unaltered printing, 1878). His later views appear in his posthumously published *Grundzüge der Lehre vom Lichtsinn*, 1920. In part because he represented the phenomenological tradition and opposed Helmholtz, his influence was much greater than the extent of his writings would lead one to expect.

The later classical papers from Nagel's laboratory on dark-adaptation and light-adaptation are: H. Piper, Ueber Dunkeladaptation, *Z. Psychol.*, 31, 1903, 161-214; W. Lohmann, Ueber Helladaptation, *Z. Sinnesphysiol.*, 41, 1906, 290-311.

The initial paper on chromatic adaptation is S. Exner, Ueber einige neue subjektive Gesichterscheinungen, *Arch. ges. Physiol.*, 1, 1868, 375-394. The next early paper on this subject is John Aitken, Colour and colour sensation, *Trans. roy. Scot. Soc. Arts*, 8, 1873, 375-418, esp. 396-405, where he showed how adaptation to one color changes the appearance of other colors. The paper by G. J. Burch is On artificial temporary colour-blindness with an examination of the colour sensations of 109 persons, *Phil. Trans.*, 191 B, 1899, 1-34. Burch gives colored plates which show how the spectrum looks to persons with

complete adaptation for particular spectral colors. The recent phenomenological description of the process of chromatic adaptation is H. Sheppard, Foveal adaptation to color, *Amer. J. Psychol.*, 31, 1920, 34-58. L. T. Troland's contradiction of the finding that all colors adapt completely into gray if given time enough is: The colors produced by equilibrium photopic adaptation, *J. exp. Psychol.*, 4, 1921, 344-390.

Exner's paper, *op. cit.*, was the first to indicate that, except for certain primary colors, all colors shift in hue under adaptation. This conclusion was seemingly confirmed by C. Hess, Ueber die Tonänderungen der Spectralfarben durch Ermüdung der Netzhaut mit homogenem Lichte, *Arch. Ophthal. Berlin*, 36 (1), 1890, 1-32, and by H. Voeste, Messende Versuche über die Qualitätsänderungen der Spectralfarben in Folge von Ermüdung der Netzhaut, *Z. Psychol.*, 18, 1898, 257-273. Troland and C. H. Langford explain this result away as an artifact of the Bezold-Brücke phenomenon; cf. Troland, *op. cit.*, 180.

• Contrast

For the early history of contrast, see Plateau, *op. cit.*, the fifth memoir, and for the history of the theory of colored shadows, see *ibid.*, the sixth memoir. Here lie the references to Aristotle, Alhazen, Leonardo da Vinci, Halley, Buffon and many others. The quotation from R. W. Darwin is from his *op. cit.*, 345.

The eighteenth century book on colored shadows is H. F. T., *Observations sur les ombres colorées*, 1782, [n.v.]. The paper of the famous scientist, Benjamin Thompson, Count Rumford, deserves more attention than it has had, for it established contrast as a subjective phenomenon, discovered synthetizing conditions for it, and related the colored shadows to contrast: An account of some experi-

ments upon coloured shadows, *Phil. Trans.*, 84, 1794, 107-118.

The nature of M. E. Chevreul's work is shown by its full title: *De la loi du contraste simultané des couleurs, et de l'assortiment des objets colorés, considéré d'après cette loi dans ses rapports avec la peinture, les tapisseries des Gobelins, les tapisseries de Beauvais pour meubles, les tapis, la mosaïque, les vitraux colorés, l'impression des étoffes, l'imprimerie, l'enluminure, la décoration des édifices, l'habillement et l'horticulture*, 1839.

Sir Thomas Young's theory of sympathetic adaptation is in his *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 1807, I, 455. For the reference to Fechner on *Nebenbilder*, *vide supra*.

The crucial experiment of H. Meyer is Ueber Contrast- oder Complementarfarben, *Ann. Phys. Chem.*, 171, 1855, 170 f.

For Helmholtz on contrast, see his *Physiological Optics*, II, [1860] 1924, sect. 24, 264-301 in the Eng. trans. This section has a fair historical account of the problem up to 1860. Helmholtz's doubt about the sensory actuality of contrast runs all the way through the section: he admits contrast, yet tries to argue it away. The explanation as illusion of judgment comes in here, as does also his puzzle about Meyer's experiment. For Helmholtz on unconscious inference in general, see *ibid.*, III, [1866] 1925, sect. 26; E. G. Boring, *A History of Experimental Psychology*, 1929, 300-304.

For the reference to Hering, see his *Zur Lehre vom Lichtsinne*, 1872-1874, esp. Mittheilungen 2 and 3, 1873. For Hering on contrast induced with colored shadows and also some of his criticism of Helmholtz, see his Ueber die Theorie des simultanen Contrastes von Helmholtz, *Arch. ges. Physiol.*, 40, 1886, 172-191.

The classical experimental studies of brightness contrast are: A. Leh-

mann, Ueber die Anwendung der Methode der mittleren Abstufungen auf den Lichtsinn, *Phil. Stud.*, 3, 1886, 497-533, esp. 516-528; C. Hess and H. Pretori, Messende Untersuchungen über die Gesetzmässigkeit des simultanen Helligkeitscontrastes, *Arch. Ophthal. Berlin*, 40 (4), 1894, 1-24. A. Kirschmann's experimental study of color contrast is Ueber die quantitativen Verhältnisse der simultanen Helligkeits- und Farbencontrastes, *Phil. Stud.*, 6, 1890, 417-491. Another classical study of color contrast is H. Pretori and M. Sachs, Messende Untersuchungen der Simultancontrastes, *Arch. ges. Physiol.*, 60, 1895, 71-90; but this study, being based upon Hering's conception of valences, lacks the permanent value of Kirschmann's.

On the elimination of contours as a condition of contrast, see W. Wundt, *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, 415 f.; E. B. Titchener, *Experimental Psychology*, I, 1901, Pt. i, 20 f., Pt. ii, 34 f. On the other hand, one finds no serious consideration of this principle in the handbooks of Helmholtz (1860), Aubert (1865), William James (1890), Külpe (1892), Nagel (1905), and Ladd and Woodworth (1911), although all these texts discuss the laws of contrast. On the way in which Gestalt psychology finds contrast involved with the principles of perceptual integration, see K. Koffka, *Principles of Gestalt Psychology*, 1935, 133-138. Of course Koffka's book carries all the way through the implication that these perceptual integrations depend mostly on the laws of the brain and not the laws of the retina.

The argument for contrast's being a central neural phenomenon originates in the special case of Schumann's vision. He was blind to green but perceived, for green light, red after-images and red contrast sensations. See F. Schumann, Ein ungewöhnlicher Fall von Farbenblindheit,

Ber. I Kongr. exper. Psychol., 1904, 10-13, and cf. 20 f. Similar instances are mentioned by C. E. Ferree and G. Rand, Some areas of color blindness of an unusual type in the peripheral retina, *J. exp. Psychol.*, 2, 1917, 295-303. On the general role of the brain in the perception of color, see also L. T. Troland, *Principles of Psychophysiology*, II, 1930, 192.

Indirect Vision

Excellent historical and critical accounts of the experimental research in the chromatic sensitivity of the peripheral retina are given by J. W. Baird, *The Color Sensitivity of the Peripheral Retina*, 1905, 7-41, and G. Rand, The factors that influence the sensitivity of the retina to color, *Psychol. Monogr.*, 15, 1913, no. 62, 6-78. The former is more historical, the latter more constructive and critical.

For Thomas Young on the range of vision, see his On the mechanism of the eye, *Phil. Trans.*, 91, 1801, 23-88, esp. 44-46; reprinted in *Miscellaneous Works of Thomas Young*, 1855, I, 12-63, esp. 30 f. D. Troxler's study is Ueber das Verschwinden gegebener Gegenstände innerhalb unseres Gesichtskreises, in Himly and Schmidt's *Ophthalmologische Bibliothek*, 1804, II, Pt. 2, 1-53, [n.v.].

J. E. Purkinje's account of indirect vision is in his second volume, *Neue Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht*, 1825, 1-31.

V. Szokalsky is cited at second-hand from Baird, presumably his *Ueber die Empfindung der Farben in physiologischer und pathologischer Hinsicht*, 1842, [n.v.], since his *Essai sur les sensations des couleurs*, 1841, which reprints the nine papers of *Annales d'oculistique*, 2, 1839, and 3, 1840, does not have this discussion of the color zones.

H. Aubert's basic paper for this topic is Ueber die Grenzen der Farbenempfindung auf den seitlichen

Theilen der Retina, *Arch. Ophthalm. Berlin*, 3 (2), 1857, 38–67. See also his Untersuchungen über die Sinnes-thätigkeit der Netzhaut, *Ann. Phys. Chem.*, 191, 1862, 87–116, esp. 99–113; also Aubert's own summary of the facts in his *Physiologie der Netzhaut*, 1865, 116–124. Baird, *op. cit.*, 11, cites five other relevant references, none later than 1876.

E. Landolt's work is scattered, but his most inclusive article on indirect vision is Il perimetro e la sua applicazione, *Ann. Ottalm.*, 1, 1872, 465–484.

For the early papers supporting the Helmholtz theory from the facts on indirect color vision, see R. Schelske, Zur Farbenempfindung, *Arch. Ophthalm. Berlin*, 9 (3), 1863, 39–62, esp. 39–49; M. Woinow, Zur Farbenempfindung, *ibid.*, 16 (1), 1870, 212–224.

One of the first papers to provide support for the Hering theory (before the theory had been fully developed) was R. Schirmer, Ueber erworbene und angeborene Anomalien des Farbensinnes, *Arch. Ophthalm. Berlin*, 19 (2), 1873, 194–235. Schirmer determined the order of the color zones as G, R, Y and B from the center to the periphery, and he concluded that in cases of atrophy of the optic nerve the zones shrink and disappear, maintaining the same order. Ole Bull's most informative paper on the zones is Studien über Lichtsinn und Farbensinn, *Arch. Ophthalm. Berlin*, 27 (1), 1881, 54–154, esp. 135–154. See also his Sur la périmétrie au moyen de pigments colorés, *Ann. oculist. Paris*, 110, 1893, 169–181; *Périmétrie*, 1895, 1–24. The classical paper for C. Hess on this topic is Ueber den Farbensinn bei indirectem Sehen, *Arch. Ophthalm. Berlin*, 35 (4), 1889, 1–62. The figures in the text are taken from Baird, who translated Hess' cm. into visual angles. For E. Hegg, see his La périmétrie des couleurs, *Ann. oculist. Paris*, 109, 1893, 321–348. Here also

the figures in the text are translated into visual angles from Hegg's dimensions of the visual field. For Baird's confirmation of Hering, see *op. cit.*, 42–74.

For the example of the modern point of view on chromatic indirect vision, see C. E. Ferree and G. Rand, Chromatic thresholds of sensation from center to periphery of the retina and their bearing on color theory, *Psychol. Rev.*, 26, 1919, 16–41. The figure of the text is adapted by change of scale from their Chart III.

Luminosity Curve and Purkinje Phenomenon

Two historical accounts are L. L. Sloan, The effect of intensity of light, state of adaptation of the eye, and size of photometric field on the visibility curve (a study of the Purkinje phenomenon), *Psychol. Monogr.*, 38, 1928, No. 1, 7–40, and the shorter E. M. Chamberlain, *A Study of the Purkinje Phenomenon with Spectral Lights*, 1911, 2–10. One also finds a good deal of history in W. A. Nagel's account of the duplicity theory in relation to twilight vision, in Helmholtz's *Physiological Optics*, 3 ed., II, [1911], Eng. trans. 1924, 343–394 [290–332 in the German], and with respect to the recent history in the computation and correction of the visibility curve, in W. W. Coblentz and W. B. Emerson, Relative sensibility of the average eye to light of different colors and some practical applications to radiation problems, *Bull. U.S. Bur. Stand.*, 14, 1917, 167–236 (also called *Scientific Papers of the Bureau of Standards*, No. 303).

For Newton on the differences of brightness among the colors, see his *Opticks*, 1704, Bk. I, Pt. 1, prop. vii (p. 97 of the 1931 reprint of the 4 ed.). The first brightness curve was plotted by J. Fraunhofer, Bestimmung des Brechungs- und Farbenzerstreungs-Vermögens verschiedener Glasarten, in Bezug auf die Ver-

vollkommen achromatischer Fernröhre, *Denkschr. Acad. Wiss. München*, math.-naturwiss. Cl., 5, 1815, 193-226, esp. 210-216 and Plate II.

The original reference to the Purkinje phenomenon is J. E. Purkinje, *Neue Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht*, 1825, 108-110 (to be found also in Purkinje's *Omnia Opera*, 1918). The men who first confirmed this observation were A. Seebeck, Ueber den bei manchen Personen vorkommenden Mangel an Farbensinn, *Ann. Phys. Chem.*, 118, 1837, 177-233, esp. 222 f. (incidental reference); H. W. Dove, Ueber den Einfluss der Helligkeit einer weissen Beleuchtung auf die relative Intensität verschiedener Farben, *ibid.*, 161, 1852, 397-408 (more than incidental, but not quantitative); J. Grailich, Beitrag zur Theorie der gemischten Farben, *Sitzungsber. Akad. Wiss. Wien*, math.-naturwiss. Cl., 13, 1854, 201-284 (incidental); H. Aubert, *Physiologie der Netzhaut*, 1865, 126-129 (rather incidental).

The second determination of the form of the brightness curve was by K. Vierordt, Beschreibung einer photometrischen Methode zur Messung und Vergleichung der Stärke der farbigen Lichtes, *Ann. Phys. Chem.*, 213, 1869, 200-222; and the third was by W. Dobrowolsky, Ueber die Empfindlichkeit des Auges gegen die Lichtintensität verschiedener Spectralfarben, *Arch. Ophthalm. Berlin*, 18 (1), 1872, 74-92.

S. P. Langley's measurement of the distribution of energy in the solar spectrum was reported by him as The selective absorption of solar energy, *Amer. J. Sci.*, 3 ser., 25, 1883, 169-196. His determination of the luminosity curve is in his Energy and vision, *ibid.*, 3 ser., 36, 1888, 359-379. F. Strenger's paper is Zur Lichtemission glühender fester Körper, *Ann. Phys. Chem.*, 268, 1887, 271-275. There was at this time also a study by H. Ebert, showing that the chromatic

differential sensitivity of the eye would affect the perception of spectroscopic lines in the spectrograms of stars: Ueber den Einfluss der Schwellenwerthe der Lichtempfindung auf den Charakter der Spectra, *ibid.*, 269, 1888, 136-158.

A. Kömig's classical paper is Ueber den Helligkeitswerth der Spectralfarben bei verschiedener absoluter Intensität, *Beiträge zur Psychologie und Physiologie der Sinnesorgane* (Helmholtz Festschrift), 1891, 309-338, reprinted in his *Gesammelte Abhandlungen zur physiologischen Optik*, 1903, 144-213.

For a discussion of the duplicity theories of M. Schultze, H. Parinaud and J. von Kries, see Nagel, *loc. cit.*, and J. H. Parsons, *An Introduction to the Study of Colour Vision*, 2 ed., 1924, 206 f. Von Kries's original paper on his theory is Ueber den Einfluss der Adaptation auf Licht- und Farbenempfindung und über die Funktion der Stäbchen, *Ber. naturf. Ges. Freiburg i. B.*, 9, 1894, 61-70, but a more accessible account is Ueber die Funktion der Netzhautstäbchen, *Z. Psychol.*, 9, 1896, 81-123.

For representative papers in the controversy as to whether the Purkinje phenomenon occurs in the fovea, see W. Koster, Untersuchungen zur Lehre vom Farbensinn, *Arch. Ophthalm. Berlin*, 41 (4), 1895, 1-20, and F. D. Sherman, Ueber das Purkinje'sche Phänomen im Centrum der Netzhaut, *Phil. Stud.*, 13, 1898, 434-479, who find the phenomenon in the fovea; von Kries and Nagel, Weitere Mittheilungen über die functionelle Sonderstellung des Netzhautcentrums, *Z. Psychol.*, 23, 1900, 161-186, esp. 173-177, who are on the other side and who say why; and L. T. Troland, The absence of the Purkinje phenomenon in the fovea, *J. Franklin Inst.*, 182, 1916, 111 f., who is generally supposed to have settled the issue in favor of von Kries and Nagel.

On E. Hering's position that dark-

adaptation is the condition of the Purkinje phenomenon, see his Ueber das sogenannte Purkinje'sche Phänomen, *Arch. ges. Physiol.*, 60, 1895, 519-545. On H. Parinaud's similar view, see Parsons, *loc. cit.*; also Parinaud, La sensibilité de l'oeil aux couleurs spectrales, *Rev. sci. Paris*, 4 ser., 3, 1895, 709-714. For the early work on the nature of the absorption of light by the visual purple, the studies by W. Kühne, W. Trendelenberg and others, see Nagel in Helmholtz's *Physiological Optics*, Eng. trans., II, 51-56. The recent authoritative work is S. Hecht and R. T. Williams, The visibility of monochromatic radiation and the absorption spectrum of visual purple, *J. gen. Physiol.*, 5, 1922, 1-33, q.v. for the history and bibliography of this topic, for the photopic and scotopic visibility curves (p. 16), and for the close relation of the scotopic visibility curve to the absorption curve for visual purple (pp. 24-30).

For the use of *photopia* and *scotopia*, see Parsons, *op. cit.*, 1 ed., 1915, 17. For the I.E.S. officially adopted mean normal photopic visibility coefficients, see F. G. Nutting, 1919 report of the Standard Committee on Visual Sensitometry, *J. opt. Soc. Amer.*, 4, 1920, 55-79, esp. 55-60. For the general picture of modern standardization in this field, see Coblentz and Emerson, *op. cit.* For instance, they show, superposed, the visibility curves of 125 persons, indicating the agreement in this function (Fig. 13, facing p. 206).

On the minimal sensitivity of the retina, see Langley, *loc. cit.*; S. Hecht, S. Schlaer and M. H. Pirenne, Energy at the threshold of vision, *Science*, 93, 1941, 585-587, who cite other references.

Color Blindness

There is a very good account of the history of the psychology of color blindness in M. Collins, *Colour-*

Blindness, 1925, 3-30. There is considerable historical information in Parsons, *Introduction to the Study of Colour Vision*, 2 ed., 1924, 170-202. V. P. Scheidt, *Revision and Further Application of the Nela Test for Color Blindness*, 1936, 3-20, summarizes all the other summaries. I. A. Haupt, Tests for color-blindness: a survey of the literature with bibliography to 1928, *J. gen. Psychol.*, 3, 1930, 222-265, has some historical value.

For bibliography, besides König's 1205 references in Helmholtz, *Handbuch der physiologischen Optik*, 2 ed., 1896, there are the 335 titles in Haupt, *loc. cit.* For general information, see Parsons and Collins, *op. cit.*; also W. deW. Abney, *Colour Vision*, 1895, about half of which treats of color blindness. All these books make it clear how closely the problems of color blindness have been related to retinal theory.

For the two early instances cited in the text, see D. Turberville, Several remarkable cases in physick, relating chiefly to the eyes, *Philos. Trans.*, 14, 1684, 736; J. Huddart, An account of persons who could not distinguish colours, *ibid.*, 67 (1), 1777, 260-265.

For the classical paper, see J. Dalton, Extraordinary facts relating to the vision of colours, *Mem. Manchr. lit. phil. Soc.*, 5, Pt. 1, 1798 [paper read Oct. 31, 1794], 28-45. Dalton (1766-1844) had just been elected to the Manchester Literary and Philosophical Society, of which he was later secretary and then (1817-1844) president, and to which he eventually contributed, all told, 116 memoirs. The story about his gown is that when Oxford conferred on him the Doctorate of Civil Laws and its scarlet gown, he, a Quaker, wore the robe for several days because he liked it, not realizing that it was gay and conspicuous. George Wilson (*op. cit. infra*, p. 6) says that Pierre Frévest first used the term *Daltonism* in 1827.

Seebeck's paper has been cited in the notes to the last section. It is

formally divided between the two types of color blindness; one half of the paper describes the first, the other half the second.

Sir John F. W. Herschel, son of Sir William Herschel (both Herschels were astronomers), made his remarks on color blindness in his article on Light, *Encyclopaedia Metropolitana*, 4, 1845, 434 f.

George Wilson's book is *Researches on Colour-Blindness*, 1855, a very informative text, exhibiting knowledge and belief in the period when the Helmholtz theory had just been publicized. J. C. Maxwell's letter to Wilson is entitled On the theory of colours in relation to colour-blindness, and was printed by Wilson, pp. 153-159. It was reprinted in *Trans. roy. Scot. Soc. Arts*, 4, 1856, 394-400; and reprinted again in *Scientific Papers of James Clerk Maxwell*, 1890, I, 119-125.

W. Pole's original description of his own defect is On colour-blindness, *Proc. roy. Soc.*, 8, 1856, 172-177; reprinted in *Phil. Mag.*, 4 ser., 13, 1857, 282-286. His final summary and argument is On the present state of knowledge and opinion in regard to colour-blindness, *Trans. roy. Soc. Edinb.*, 37, 1893, 441-480.

On A. von Hippel's monocular case, see Ein Fall von einseitiger, congenitaler Roth-Grünblindheit bei normalen Farbensinn des anderen Auges, *Arch. Ophthal. Berlin*, 26 (2), 1880, 176-186.

F. Holmgren developed his thought about testing gradually, beginning in 1871. The representative article establishing his test seems to be Om den medfödda färgblindhetens diagnostik och teori, *Nord. med. Ark.*, 6, 1874, 24-28, [n.v.], which establishes a date, but a fuller discussion occurs in his Om några nyare praktiska metoder att upptäcka färgblind-

het, *Uppsala Läkforen. Förh.*, 13, 1878, 193-226. For an Eng. trans. of his instructions and a full discussion of the test, see B. J. Jeffries, *Color-Blindness*, 1879, 201-219. For the first pseudo-isochromatic test, see J. Stilling, *Ueber das Sehen der Farbenblinden mit Tafeln*, 1880, 2 ed., 1883. Lord Rayleigh's equation is to be found in his Experiments on Colour, *Nature*, 25, 1881, 64-66. On other matters concerned with the tests, see Haupt, *op. cit.*

It is impossible to cite adequately in these notes the contributions to this problem of König, von Kries and Nagel. The extent of König's work appears in his *Gesammelte Abhandlungen zur physiologischen Optik*, 1903. There are really two important papers on dichromatic vision by A. König and C. Dieterici: Die Grundempfindungen und ihre Intensitätsvertheilung im Spectrum, *Sitzungsber. preuss. Akad. Wiss.*, 1886, 805-829 (reprinted in *Ges. Abhandl.*, 60-87); Die Grundempfindungen in normalen und anomalen Farbensystemen und ihre Intensitätsvertheilung im Spektrum, *Z. Psychol.*, 4, 1892, 241-347 (reprinted in *Ges. Abhandl.*, 214-321). Then see J. v. Kries and W. Nagel, Ueber den Einfluss von Lichtstärke und Adaptation auf das Sehen des Dichromaten (Grünblinden), *ibid.*, 12, 1896, 1-38; von Kries, Ueber Farbensysteme, *ibid.*, 13, 1897, 241-324.

Two recent articles implying that color blindness is not an all-or-none affair but that dichromatic vision is the limit of normal variation of trichromatic vision, are: S. P. Hayes, The color sensations of the partially color-blind, a criticism of current teaching, *Amer. J. Psychol.*, 22, 1911, 369-407; S. W. Terman, A new classification of the red-green color-blind, *ibid.*, 41, 1929, 237-251.

Chapter 6

COLOR THEORY

WE have already seen in Chapter 3 how the problem of color theory arose. It is hard to say what the original theory of color must have been. The view that, in perception, the mind correctly represents the characteristics of an external object to itself—such a view includes a theory of color. Epicurus' belief that fine images of objects, exact replicas of the perceived bodies, are discharged from the surfaces of the bodies to reach the soul by way of the organs of sense—that is an even better theory of color because it tells why the perception is correct, and the nature of the dependence of the perception upon the stimulus. What has been called 'theory' in connection with color is always an assertion, generally somewhat speculative, about the nature of the processes that are intermediate between the stimulus, on the one hand, and the sensation or the brain, on the other. John Locke's doctrine of the secondary qualities was an assertion that the properties of the perception do not always resemble the properties of the stimulus; so it was a sensory theory. The doctrine of the specific energies of nerves was a similar sensory theory, because it asserted that all qualities are secondary, that one perceives, not the objects themselves (or images emanating from them) but the states of the nerves that have been affected by them.

The first correct theory of color, as we have seen, was Newton's. He was concerned with the nature of light, the process between the object and the eye. White and purple, he showed, are always complex lights; the spectral hues may be either complex or simple; and all colors are indeterminate as to constitution, for every color may be compounded in a variety of ways. Newton's conception of the process beyond the eye was naive. He supposed that the vibration rates of the seven primary colors are transmitted by the optic nerve to the sensorium. (See pp. 110 f.)

After Newton there was a question as to the place of the 'seat'

of vision in the eye, as to whether it was the "crystalline" (the lens), the "choroid" (coat of the eye), or the retina. By the time of Thomas Young, opinion had pretty well settled upon the retina as the percipient organ, that is to say, the crucial point at which the perceptual process ceases to be light and becomes nervous.

We have also seen that Thomas Young (1801) realized the necessity for a retinal theory of color, and in making his three-fiber hypothesis anticipated the theory of specific nerve energies. Knowing that there is an infinitude of simple colors in the spectrum, he did not see how the frequencies of all could be taken up at a single point in the retina and transmitted along a single fiber. Thus he took advantage of Newton's principles of color mixture to suppose that there might be only three kinds of fibers, each selectively activated by different wave-lengths of light. This view of his was an attempt to explain color perception by retinal events. Until Helmholtz publicized it in 1852, however, it attracted scant attention; thereafter it was to seem more important, inasmuch as, coming after Johannes Müller's doctrine of specific nerve energies, it appeared as an application of that dogma. (See pp. 70, 110-112.)

As we have also seen, Goethe, Schopenhauer, and Szokalsky had theories of color in the first half of the nineteenth century, but there was no real progress until after 1850.

The Young-Helmholtz Theory

Of Thomas Young's theory of color Maxwell said: "It seems almost a truism to say that colour is a sensation; and yet YOUNG, by honestly recognizing this elementary Truth established the first consistent theory of colour. So far as I know, THOMAS YOUNG was the first who, starting from the well-known fact that there are three primary colours, sought for the explanation of this fact, not in the nature of light, but in the constitution of man." Newton's critics, noting that the sensation white is achromatic, had not been able to see how white light can be a mixture of colored lights. For the same reason it was easy to suppose that, if all the colors can be made by mixing three, there must be only three kinds of physical light. Even Sir David Brewster as late as 1848 continued to make this mistake. In fact, Thomas Young's theory had to be rediscovered by Helmholtz, so little was it known. In advocating the un-

dulatory theory of light Young had been opposing the Newtonians and their corpuscular theory. This happy heresy of Young's met with so great opposition, however, that his color theory has been said to have been obscured by the general suspicion aroused by his theory of the nature of light.

The revival of Young's theory, as we have seen, occurred in connection with the experiments on color mixture by Helmholtz and Maxwell. (See pp. 140-144.) Although Helmholtz's experi-

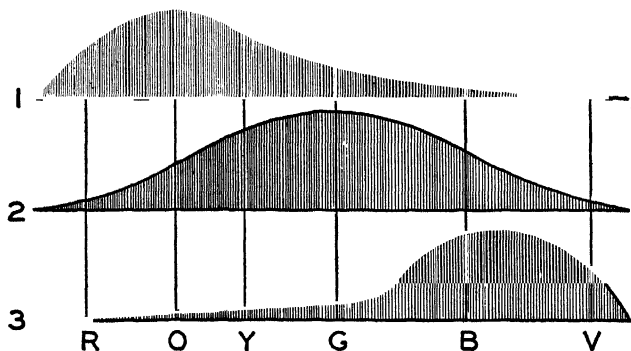


FIG. 24. EXCITATION CURVES OF THE YOUNG-HELMHOLTZ THEORY (1860)

Approximate construction by Helmholtz. 1 = distribution of excitation for the "red-sensitive fibers." 2 = the same for the "green-sensitive fibers." 3 = the same for the "violet-sensitive fibers."

mental study of complementaries in 1852 is the first date for his espousal of the theory, the event was marred by Helmholtz's failure to find that he could get white from more than one binary combination. Grassmann then pointed out the faulty logic of Helmholtz's first conclusion, and Helmholtz, with an improved experiment, rectified his position in 1855. In this year, too, Maxwell supported Young's theory in the report of his experiments upon color mixing. Ultimately, Helmholtz propounded the theory formally and effectively in the section of his *Physiologische Optik* that came out in 1860.

Newton's laws required that any color can be matched by a proper mixture of three properly chosen primaries. Young seemed to think it an obvious consequence of the laws that all color sensations could be got from three physiological processes in the retina. Actually, however, there is an important step that has to be taken

between Newton's laws and Young's theory. If there were merely three kinds of nerve fibers in the retina, stimulated respectively by R , G and V light, then there would be no reason why orange should ever be seen. One has to assume that all light affects all the processes, but in unequal degree. It follows then that the physical primaries cannot have simple physiological effects: R light excites the R , G and V processes, and G light too excites all three processes, and so does V light. Thus one has to assume excitation curves like those of Fig. 24, which show Helmholtz's tentative suggestion in 1860 as to the form of these functions. The important point is that, if all the colors can be matched by mixtures of three simple physical primaries, then these primaries will require not more than three physiological components in order that all pure lights may be perceived as different and that the laws of color mixture may hold. Young merely implied this step. Maxwell took it explicitly by his calculation in 1855; he computed from his color triangle a set of three excitation curves and later (1860) charted the two components for a "dichromic" color-blind person. The mathematics of this relationship was given explicitly much later (1886) by König and Dieterici, whose exposition Helmholtz repeated in the second edition of his *Optik* (1896). (Cf. pp. 145-147.)

To Maxwell and Helmholtz it was apparent that the pure physiological sensations are stronger than can be got by a mixture of physically pure lights. There are no three spectral colors which by mixture can match all the other spectral colors in hue and saturation, for a mixture is usually of less saturation than the spectral color of the same hue. Thus, in constructing the mixture diagrams, it is always necessary to form a hypothetical triangle large enough to include the spectrum, and here the spectrum is represented. This diagram is extended to the triangle

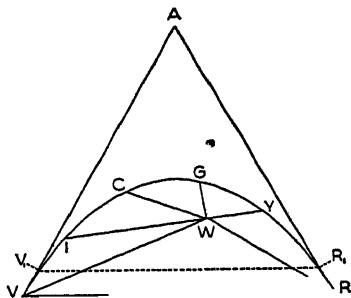


FIG. 25. CONSTRUCTION OF THE
COLOR TRIANGLE: HELMHOLTZ
(1860)

The three basic physiological sensations (*Grundempfindungen*) are represented at R, A and V. The line of the spectrum is R', Y, G, C, I, V'. Cf. Fig. 15, p. 147.

enough to include the spectrum, as in Helmholtz's chart of Fig. 25. Here the spectrum is represented by the line R_1YGCIV_1 , but the diagram is extended to the triangle RAV . The three corners, R , A

and V, represent the colors which each of the physiological processes acting alone would give. Although the construction is hypothetical, it receives support, as Helmholtz noted, from the fact that spectral colors can become supernormal in saturation if the eye is first fatigued to the complementary hue.

In all this argument there was never any certainty as to which colors should be selected as primaries. Young had first chosen a

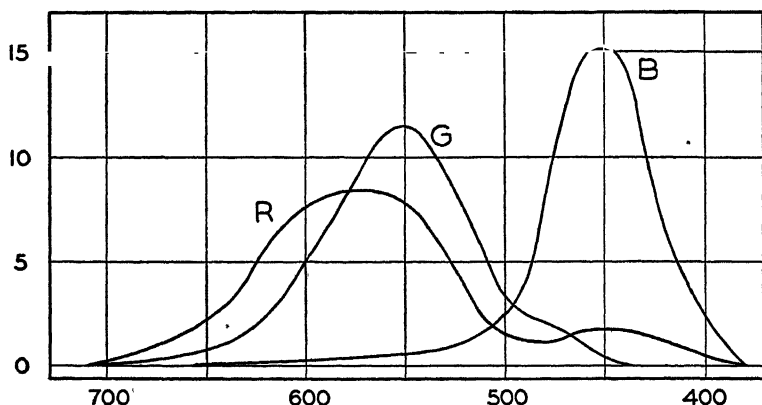


FIG. 26. EXCITATION CURVES: KÖNIG AND DIETERICI (1892)

The curves for the three *Grundempfindungen* as computed for König's eye. These data are classical. Modern curves differ from them.

red, a yellow and a blue, and then later a red, a green and a violet. In general, it seems best to choose three colors that form a triangle (cf. Fig. 25) which extends as little as possible beyond the spectrum. Young and others after him preferred to choose two colors at the extremes of the spectrum; otherwise a purple would have been required, and the excitation curve (cf. Fig. 24) for it would have had two modes, one at each end of the spectrum.

The Young-Helmholtz theory, as we saw in the last chapter, was developed with great elaboration in connection with dichromatic vision. König and others sought to work out the excitation curves for trichromats (Fig. 26), protanopes and deuteranopes. (See pp. 187-189.) There was a chance to check the choices of primaries here, and König and Dieterici found that color mixtures for the protanopes resemble closely, but not exactly, the mixtures for trichromats with the R-component lacking, and that the mix-

tures for deuteranopes resemble closely, but not exactly, the mixture for trichromats with the G-component lacking. Thus the theory received at this stage a general, but not a precise, corroboration. Meanwhile the Hering theory was gaining attention for certain facts which lay outside the scope of the Young-Helmholtz theory but which had eventually to be brought within the general theory of color. In 1881, for example, Donders published a significant paper which derived its importance, partly from the fact that it verified the color matches for dichromats and trichromats in the way that the Young-Helmholtz theory required, and partly from the fact that it proposed, as we shall see presently, a more general theory which tended to resolve the incompatibilities between Helmholtz's and Hering's views.

The Duplicity Theory

The *Duplizitätstheorie* is a term applied by von Kries in 1894 to the theory that twilight vision is a function of the retinal rods and daylight vision a function of the retinal cones. The theory, however, took shape gradually before von Kries, and it may properly be regarded as the second advance in the theory of retinal action. The Young-Helmholtz theory was the first.

When, in 1838, Johannes Müller discussed the functions of the retina, he could appeal only to Treviranus's elaborate researches on the anatomy of the eye (1828). Consequently he localized the percipient function in the "rod-shaped terminations of the nervous fibers upon the internal surface of the retina," "a layer of cylinders arranged closely side by side." He did not know about rods and cones.

The anatomy of the retina and the relation of the rods and cones to each other were described by H. Müller in 1851, and his findings incorporated by Kölliker in his *Mikroskopische Anatomie* in the following year. Müller and Kölliker made it clear that there are two different recipient organs in the retina. Müller also discovered the visual purple that is associated with the rods. Helmholtz, however, considering theories of vision in 1860, was chiefly concerned with Young's three-component theory and, for this reason, did not realize the true significance of the dual structure of the retina.

The first person to attach a proper meaning to the 'duplicity' of

the retina was Max Schultze, who in 1866, in the second volume of his new journal, the *Archiv für mikroskopische Anatomie*, published a lengthy study of the anatomy and physiology of the retina. The date is significant, for the theory which that paper included would not have been possible except for Aubert's publication of his *Physiologie der Netzhaut* the year before. Aubert had pointed out that both color discrimination and visual acuity diminish toward the periphery of the retina (cf. p. 172). Schultze now showed that, since the number of cones diminishes in indirect vision, the rods predominate greatly at the retina's periphery. Hence, by putting two and two together, he concluded that the cones are the mediators of color vision and the basis of accurate spatial discrimination. His studies in the comparative anatomy of the retina led him to observe that the cones are missing or few in the retinas of animals with nocturnal habits (e.g., bats) and that the rods are few in the retinas of animals with diurnal habits (e.g., lizards). From that point, since night vision in man is achromatic, it was a simple step to assign the rods to night vision and the cones to daylight vision. Schultze exaggerated the degree in which nocturnal animals lack cones, but he was right in general as to the tendency for predominance of rods in nocturnal eyes and of cones in diurnal eyes. Moreover, because cone vision is more highly differentiated in respect of chromatic and spatial discrimination, he concluded that rod vision—he was writing in the days when the theory of evolution had just become important—is the more primitive, and that cone vision is a later development, mediating both light and color sensations. Thus Schultze, with less evidence than von Kries had later, espoused the duplicity theory almost thirty years before von Kries named it.

In 1877 Charpentier, who did not know Schultze's work, set up the argument that the variation of vision in different parts of the retina must indicate the existence of two independent visual functions. He thus made out a case for a light sense and a color sense, citing the data of Purkinje and Aubert. The light sense he called "perception lumineuse brute." Its action, he said, is diffuse, and he called its organs the "photoesthetic elements," without definitely identifying them with the rods. The other sense he called "vision nette." It mediated, he held, the perception of both light and color and of accurate spatial discrimination, and he called its organs the "visual elements."

Just at this time the visual purple came under investigation. In 1876 Boll found that the visual purple is 'bleached' to yellow in daylight, a discovery which suggested the possibility that the formation of visual purple is the mechanism of dark-adaptation and that the visual purple accounts for the shift of brightness in the Purkinje phenomenon (cf. pp. 176-181). A series of researches on the action of the visual purple was published by Kühne in 1877-1878, and thereafter the study of rod vision was always associated with the facts of dark-adaptation.

Besides Charpentier there was another anticipator of von Kries, H. Parinaud, who formulated "*la théorie des deux rétines*" in 1881. He also was unaware of the work of Schultze. Studying night blindness, and finding that it did not diminish foveal sensitivity, he attributed this deficiency to a defect of the rods and visual purple. When, on the basis of later experiments (1884), he concluded that the fovea is not subject to dark-adaptation, he accounted for this fact by the absence of rods and visual purple in the fovea. His theory of "the two retinas" was quite explicit: the cones are the organs of spatial vision and color vision, functioning in daylight; the rods are nocturnal organs which mediate solely the light sensation and function only when the visual purple is formed under dark-adaptation.

It was König who supplied the last important fact needed to establish the function of the visual purple in night vision. In 1894 he published determinations of the absorption coefficients of the visual purple and the visual yellow, and showed that the brightness shift of the Purkinje phenomenon agrees very closely with what should be expected from this absorption.

Von Kries, bringing all these arguments together in 1894, formulated the duplicity theory that has since received general acceptance. He stressed particularly the implications of the facts of adaptation. Ever since Aubert's pioneer work on adaptation (pp. 160 f.) it had been customary to measure the degree of dark-adaptation by observing the decrease in the size of the threshold for illumination. These results show that, whereas in light-adaptation the periphery of the retina is a little less sensitive than the fovea, in dark-adaptation it is fully a thousand times more sensitive. Von Kries held that the rods are primitive receptors, giving rise only to the light sensation and functioning with great sensitivity in low illumination under the influence of the visual purple.

The cones, present alone in the fovea and scarce in the periphery, are, von Kries believed, the organs of vision in good illumination, mediating both the chromatic and achromatic sensations. Because it assumes that the sensation white is aroused by two very different processes, one from the rods and one from the cones, von Kries's theory has sometimes been called a theory of the "double white." Since von Kries also accepted the Young-Helmholtz theory as applying to the cones, it seemed strange at first that white should appear in his theory both as the sole function of the rods and also as the emergent quality of the simultaneous excitation of the three color-processes in the cones. Later theories, like Ladd-Franklin's, which regard the perception of color as a differentiation of the more primitive perception of white, and the cone as evolutionarily later than the rod, answered this objection. At any rate, the objection was not fatal, for the duplicity theory is the only part of all the theorizing about retinal function that can today be regarded as a fact established beyond the speculative stage.

The Hering Theory

In general the phenomenologists have always agreed that there are four fundamental colors: red, yellow, green and blue (cf. pp. 126-131), although Brentano questioned the simplicity of green (cf. p. 131), and sometimes it has seemed that orange and purple, which have special names, must have a different status from yellow-green and green-blue. Leonardo da Vinci (ca. 1519) held for the four principal colors. Goethe (1810) kept to the same basic principle. Mach affirmed the same fact later. The four-hue theory of color is pretty obvious to phenomenological observation, but the phenomenological mixtures do not agree with the physical mixtures. For all that a 'pure' red stimulus mixed with a 'pure' green stimulus gives a yellow, no one ever thought that yellow looks as if it were reddish green. The Young-Helmholtz theory, based upon the facts of color mixture, thus missed these important phenomenological facts.

Ernst Mach pointed out the difficulty in 1865. He laid down the general principle of psychophysical parallelism. "*Jedem Psychischen entspricht ein Physisches, und umgekehrt. Gleichen psychischen Processen entsprechen gleiche physische, ungleichen ungleiche.*" Some people, he said—and he may have been thinking

of Helmholtz's explaining of color contrast as an illusion of judgment—dismiss psychic facts as illusions; nevertheless, even an illusion is lawful and should have its principle determined. Since it is clear that red, yellow, green and blue are four basic colors, there must be something fundamental in the physiological processes to account for their peculiar status. There must also be something physiologically unique for black and white, since they are unique as experiences. As no one questioned psychophysical parallelism in those days, this logic of Mach's was convincing; it influenced even Hering in the formulation of his theory.

A similar objection to the Young-Helmholtz theory was expressed in the same year (1865) by Aubert, who brought together numerous cases where the matches of color mixture differ from phenomenological expectation. The most obvious instance is that 'pure' red and 'pure' green, when mixed, give yellow, an unsaturated yellow, but nevertheless a 'pure' yellow, if the red and green are in the right proportions. The Young-Helmholtz theory depends on the principle that $R + G = Y$, and that $R + G + V = W$, if R , G , and V correspond to the elementary physiological processes. Aubert also brought out other cases, like $Y + Bk = G$, an instance of what was later called the Bezold-Brücke phenomenon (cf. 132). All these cases go to show that the psychological and physical components of colors do not agree, that the stimulus-components may be other than what one could expect from the direct observation of the color, and that we must, therefore, as Mach also was saying, admit the importance of phenomenological fact. Aubert ended by adopting the term *principal color* (*Hauptfarbe*) for black, white, red, yellow, green and blue.

Somewhat later it came to be said that the Young-Helmholtz theory depends upon an "additive principle." If $R + G = Y$, and Y is not merely a reddish green, then Y is an emergent from the addition of R and G . Similarly, in holding that $R + G + V = W$, the theory assumes that white emerges from a combination of all three components. It was precisely this difficulty of accepting the fact of emergence that had led in the 1670's to the opposition to Newton's analysis of white light into colors. In a way Hering was attempting a Hegelian synthesis of the controversy between Newton and his critics, for he was showing that, although Newton was right, his critics were also right, were insisting, indeed, on a fact that required explanation.

Hering promulgated his theory in 1874. Showing that all the colors can be regarded as intermediates between red, yellow, green, blue, black and white, he described the "natural system of color sensations." In this system, he pointed out, yellow and blue are opposites (*Gegenfarben*): there can be no yellowish blue. Similarly in opposition are red and green. Such a four-color theory descends from Leonardo da Vinci and Goethe. At first Hering held that every simple color has a simple opposite, every mixed color a mixed opposite; but he had later to abandon this view in the face of the fact that pure red and pure green are not complementary, and that the red and green which do not change hue in indirect vision are both bluish (cf. p. 143). Thus, he concluded, the fundamental colors are yellow and blue, because one half of the spectrum is yellowish and the other half is bluish. The critical point that divides the two halves is pure green, and pure red does not occur in the spectrum. Red and green, he thought, are therefore a later development, as the facts of color blindness show (pp. 185-189).

To account for this natural system of colors, Hering supposed the existence of three visual substances in the retina, each capable of undergoing metabolic or chemical change in either of two antagonistic directions. These opposing changes he called *assimilation* and *dissimilation*. The substances were named for the components to which their processes give rise: the 'black-white' substance, the 'blue-yellow' substance, the 'green-red' substance. How the pure colors were regarded as dependent upon these six different processes is shown in the following table.

	White-black substance	Yellow-blue substance	Red-green substance
Dissimilation	White	Yellow	Red
Assimilation	Black	Blue	Green

All colors other than these six are mixed colors, dependent upon the excitation of more than one process. No one color process, as a matter of fact, is ever excited alone, for all light affects the white-black substance. This theory was greatly strengthened by the final decision that all partial color-blindness, whether of the deuteranopic or the protanopic type is red-green blindness. Hering argued further that the facts of indirect vision lend the theory similar support—that the middle retinal zone is red-green blind, lacking the red-green substance, and that the peripheral zone is totally

color blind, lacking both the red-green and the yellow-blue substances. This contention was not, however, univocally established.

One great difficulty in the Hering theory is its requirement of different assumptions for the action of the white-black substance and for the action of the other two substances. An intermediate hue depends upon the ratio of the two processes combining in it. Violet, for instance, depends for its exact hue upon the ratio of the dissimilative process in the red-green substance to the assimilative process in the yellow-blue substance. Since assimilation and dissimilation are mutually exclusive in the red-green substance and the yellow-blue substance, in any one substance there is simply less or more of either process. Hering had to imagine, however, that in the white-black substance both processes may go on together, as is true of many antagonistic processes in reversible chemical reactions. The brightness of a gray, he asserted, must depend upon the ratio of assimilation in the white-black substance to the total of assimilation and dissimilation taken together; i.e., the brightness equals $A/(A + D)$ or $W/(W + Bk)$. Thus 'middle gray' is experienced when assimilation and dissimilation equal each other in the white-black substance. This point is very important, for, if assimilation and dissimilation were mutually exclusive in the white-black substance, as they were said to be in the other two substances, then, instead of middle gray at the point of equilibrium for all three substances, one should see nothing at all—as he sees with the back of his head.

This necessary difficulty in the theory led Hering to develop a somewhat elaborate theory of color "valences." A light orange would have a red valence, a yellow valence, a white valence (since it is light), and also a black valence (since all light arouses both processes in the black-white substances). Later Hering had to explain the facts of color mixtures by assuming, in addition, the existence of a "specific brightness" for the colors, such that a white valence is added to mixtures for red and yellow and a black valence for green and blue. These valences represented for him the experiential potentialities of the excitation. Their historical importance, however, is negative: other theories came into existence in order to avoid hypotheses of such dubious value.

Modifications of the Fundamental Theories

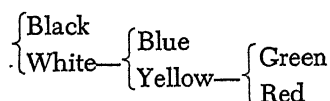
The end of the last century was a period when psychologists found it easy to manufacture speculative physiological theories. An important psychologist was expected to espouse some theory of important phenomena, and generally—aided by this social pressure and by the closeness at that time of the new experimental psychology to the speculative freedom of philosophy—each man modified existing theories to form his own. Thus, after Helmholtz and Hering, there were theories of color vision associated with each of the following important names: Wundt (1880), Donders (1881), Edridge-Green (1891), Ladd-Franklin (1892), Ebbinghaus (1893), König (1894), G. E. Müller (1896), McDougall (1901), Schenck (1907). The details of these theories, many of them, were mere guesses or suggestions as to how certain special facts could be accounted for *if* the retina were thus and so. Because they were not steps toward any view finally validated or rendered plausible, they have little historical importance. On the other hand, these theories did yield two important advances: they brought out a conception of the evolution of the color sense and they demonstrated the need for including the brain along with the retina in a theory of color.

Evolution of Color.—Schultze (1866) had supposed the rods to be more primitive organs than the cones, achromatic twilight vision to be more primitive than chromatic daylight vision. Von Kries (1894) established this view. Most of the intervening discussion of rod vision and cone vision had implied an evolutionary distinction between them.

Hering's supposition that the red-green visual substance is later in development than the yellow-blue was based upon the fact that red and green may disappear alone in color blindness or indirect vision, whereas yellow and blue are seldom lacking unless red and green are also gone. The implication is that the white-black substance is the oldest and most stable, the yellow-blue substance next, and the red-green substance least. The fact that the spectrum is divided between yellow and blue—the warm half is yellowish, the cold half is bluish—seemed to Hering to mean that yellow and blue are more primitive than red and green.

Christine Ladd-Franklin's theory in 1892 made this evolutionary relationship focal. She supposed that the primitive sense is the

black-white of achromatic vision, that white vision becomes differentiated into yellow-blue vision, and that yellow vision is still later differentiated into red-green vision. For some unknown reason blue vision has not become further differentiated. The relationship of these colors takes account of the six principal colors with whose uniqueness Hering was concerned, as well as the facts of color mixture which were basic to Helmholtz's theory. Since pure red and pure green mix to give yellow, they should represent a differentiation of yellow; and since pure yellow and pure blue mix to give white, they should represent a differentiation of white. The diagram of differentiation is this:



Mrs. Ladd-Franklin gave this relationship concrete objectivity by supposing it to represent differentiation in the decomposition of a retinal color molecule. Although Donders had anticipated her (1881) in the general assumption that color vision may be explained by reference to the decomposition of a color molecule, whose product of decomposition selectively stimulates the nerves, it was Mrs. Ladd-Franklin who added the evolutionary principle to Donders' idea. The original undifferentiated molecule, she held, alone excites the simple sensation black, but white light, by some principle of resonance, tears off from the inner core of the molecule a white-excitant group of atoms. The residual core becomes inactive and white is seen. This function, which is primitive, is all that can happen in the rods. In the cones, however, there has been developed a further selectivity, so that the yellow half of the spectrum tears off one half of the white-excitant atoms to arouse the sensation yellow, and the blue end of the spectrum tears off the other half to arouse blue. At this stage of evolution there can be yellow or blue or the yellow-blue which is white. The next stage of development would have to be the differentiation of yellow into red and green, in the way that white became capable of differentiation into yellow and blue. Thus the theory makes red, green and blue the three ultimates, with a chemical decomposition-product for each. They are, moreover, what Mrs. Ladd-Franklin called simple colors. The theory requires that $R + G = Y$ be a simple color, and that $(R + G) + B = Y + B = W$ be a simple color.

All other colors, like $R + B = \text{Purple}$, are complex. Certainly the hypostasizing of these relationships as concrete relations between parts of molecules gives the evolutionary principle clarity, although there was never enough knowledge of the chemistry of the retina to justify such assumptions.

Both McDougall in 1901 and Schenck in 1907 sponsored theories of color vision that included this same evolutionary relationship among the principal colors. McDougall's theory was focused upon some crucial experiments in visual induction and inhibition which he argued implied certain relationships in the optic nervous system. Both he and Schenck, moreover, accepted the Young-Helmholtz three-component theory as holding between red, green and blue in the initial stage of the process of color vision. In fact, one important function of the evolutionary theory is that it provides a synthesis between the theory of three components and the theory of four (or six) simple hues.

It may be said, in general, that this evolutionary view has been accepted. Red and green are paired, as against yellow and blue, in many phenomena, and they appear to be less stable in their laws. Moreover, red and green, representing as they do a finer discrimination of wave-length than do yellow and blue, might be supposed to have been developed later. For a similar reason it is natural to assume that primitive sight would include only the black-gray-whites of monochromatic vision.

Central Factors.—When Mach argued for a one-to-one correspondence between the psychical and the physical and therefore for six physical terms to account for the six principal colors—red, yellow, green, blue, white and black—it did not seem to occur to him that the six physical terms might be localized somewhere other than in the retina. In the history of psychology the knowledge of perceptual mechanisms has proceeded from without in, from stimulus to sense-organ to nerve to brain, and speculation has been most specific at the frontier between knowledge and ignorance. The theory of specific nerve energies was merely a device to get the qualities separated among different nerve fibers on the assumption that the 'sensorium' could then discriminate the particular fibers excited. It was not long, however, before the difficulties of color theory forced the consideration of processes subsequent to the initial retinal events.

Thus Donders in 1881 suggested that the Young-Helmholtz

theory might hold at the retina and that the other simple colors might emerge in the brain. Though he accepted the view of the six principal colors, he thought nevertheless that there might be only three specific energies in the fibers of the optic nerve: *R*, *G* and *B*. Then *Y*, for instance, would appear in the brain because *R* and *G*, together would excite a third central substance for *Y*. Similarly *W* would correspond to a central substance excited by the simultaneous cooperation of all three energies, and *Bk* would arise centrally in the absence of any of the energies. Donders was following out Mach's principle of psychophysical parallelism: for any simple color there must be a simple substance or process in the brain. Purple can immediately be seen to be a mixture of red and blue because it depends directly upon these two processes or 'energies.' Yellow does not appear to be a mixture of red and green, because another simple process intervenes.

In 1896 G. E. Müller formulated his modification of Hering's theory. His most important addition was the concept of cortical gray. Hering, as we have seen, had to complicate his theory in order to account for the fact that something (gray) is seen when the *R-G*, *Y-B*, and *W-Bk* processes are all three in equilibrium. Though logically one should see nothing, actually one does see gray. Müller supposed that the brain added to all vision a constant gray upon which all specific excitation is superposed. In addition to the needs of theory he had evidence from the grayness of scotomata (blind spots) in the retina. He posited further six central color processes or "valences," which respond to the retinal processes in such ways as to account for color mixture and also the specific brightnesses of the colors.

It was in these papers that Müller laid down his psychophysical axioms, setting forth the commonly accepted fundamental relations between psychic events and their correlates in the brain. (We have already discussed this matter in an earlier chapter: pp. 83-90.) The modern reaction against the speculative theorizing of the late nineteenth century is due largely to the fact that these axioms, so fundamental to the thought of that period, today seem dubious, especially as applied to the specifications of processes in the sense-organ. In 1865 (to Mach) and in 1896 (to Müller), however, it seemed convincing to say: If yellow does not appear to be a mixture of red and green, then it must depend upon some simple process in the brain and not immediately upon a mixture of the

red and green processes. Today, although we still meet the axioms of Mach, Donders, Hering and Müller in the isomorphism of Gestalt psychology, visual theory, resting for the most part in the hands of the physiological psychologists, seems to be more in need of observational facts—about the retina and the brain—than of axioms.

On the other hand, modern psychology has been forced into the acceptance of important central factors in color vision. Chromatic phenomena often depend upon binocular relations, as, for example, in binocular color mixture; and the excitations from the two eyes cannot interact at any level lower than the common central region of projection. (On color contrast as a central phenomenon, see pp. 170 f.)

Modern Visual Theory

The first third of the present century differs markedly from the last third of the nineteenth century in its attitude toward the psychophysiology of sensation. Not only has the emphasis upon the phenomenological aspects of vision become less and the concern with its physical basis more, but there is less physiological speculation and more carefully reasoned argument. Psychologists do not attempt complete theories of vision or of hearing, nor is it any longer an argument against a theory that it leaves many collateral phenomena unexplained. Instead the theorists are concerned to bring out by experiments various properties of visual phenomena until an implication as to corresponding properties of retinal processes becomes apparent.

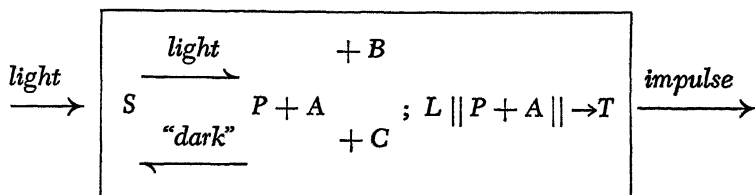
The discovery of the all-or-none principle of nervous conduction (pp. 59–68) radically altered visual theory. Whereas, in the nineteenth century, it had been taken for granted that nerve fiber can be excited in varying degrees, there existed now a definite problem about sensory intensity. In a single fiber excitation cannot vary in amount. To meet this difficulty Forbes and Gregg invented the frequency theory in 1915, a theory which Adrian and his associates amply verified in 1926 and immediately thereafter. The fact that the apparent magnitude of stars increases with intensity showed, moreover, that brightness may depend upon the number of fibers excited; yet the situation offered no easy solution for the theorists, who already needed three independent sets of nerves for

the three color processes. Troland, nevertheless, in 1920 made it clear that the all-or-none law is not necessarily incompatible with the facts of visual perception, suggesting a plausible synthesis, which has not, however, been substantiated.

The only thorough endeavor to establish a theory of retinal excitation consistent with modern knowledge of neural action is that of Hecht, which depends upon a series of researches beginning in 1919 and summarized once by Hecht in 1929 and then again later. It is not possible to state Hecht's analysis of the photochemistry of the retina briefly, but the nature of his argument can be exhibited.

Hecht began with the Bunsen-Roscoe law of photochemical reaction, $It = C$, where I = intensity, t = time, and C is a constant. The law states that a photochemical reaction depends upon the total energy and not upon intensity or time alone, for the reaction is constant if I and t vary inversely. That this law holds for the retina appears at first to be contradicted by the fact of the threshold, since a low intensity may never be sensed, no matter how long it persists. Hecht drew from this fact the inference that the product of the initial reaction must in some way be dissipated, so that, below the threshold, dissipation prevents the accumulation of enough of the product to excite the nerve fibers. The retina does not, however, remain permanently exhausted after stimulation; it recovers in time, and the rate of recovery should be proportional to the amount of the product of the original decomposition. Hecht suggested formulae for each of these two opposing reactions—the decomposition under the action of the stimulus, and the recomposition under the accumulation of the products of decomposition. It is the sum of the two processes, and that $It = C$ (The Bunsen-Roscoe law), when the time is very short, is just what experiment shows.

Hecht summarized his theory in the following diagram:



The diagram is read as follows. Light, as stimulus, acts upon the retinal photosensitive substance, S , which is thus decomposed into

two decomposition products, P and A , and possibly a third residue, B , which is ultimately dissipated. The cessation of the light, that is to say, the 'dark,' acts to recompose S out of P and A , and perhaps some other agent, C , supplied by the retina to induce the recombination. $P + A$ now acts as a catalyzer by which another retinal substance, L , is transformed into T , which acts upon the nerve fiber, exciting the neural impulse. This theory is speculative, but it must be noted that Hecht has checked some of its assumptions against certain measurements of dark-adaptation, light-adaptation, acuity and sensitivity.

Hecht has also extended his theory to include color vision at the level at which the Young-Helmholtz theory accounts for color mixture. He has explained the increase of visual acuity with intensity of illumination by reference to a population theory of the retina—the theory that the rods and cones constitute a population of varied thresholds, and that at greater intensities of the stimulus more receptors are put into function, with the result that a mosaic of more numerous points is excited in the retina and the excitatory pattern becomes capable of finer spatial differentiation. One of Hecht's calculations computes, from the data for the dependence of acuity on illumination, the result that a given retinal area should contain 542 cones, whereas anatomical data indicate that the number should be 540. Such remarkably close agreement has helped to gain acceptance for the theory, at least for the time being.

Notes

In general on theories of vision before 1850 see Chapter 3. An excellent discussion of nine of the theories (Young-Helmholtz, Hering, Donders, Ladd-Franklin, McDougall, Schenck, Wundt, Müller, Edridge-Green) is to be found in J. H. Parsons, *An Introduction to the Study of Colour Vision*, 2 ed., 1924, 203–314. This exposition includes a historical review, pp. 206–214, but the whole account is historically oriented. See also the discussion in H. v. Helmholtz, *Physiological Optics*, Eng. trans., 1924, II, 140–155 (Helmholtz's own account of his theory as of 1860), 426–454 (von Kries's account of other theories as of

1911), 455–468 (Ladd-Franklin's account of her theory in relation to others). Helmholtz's own discussion of theories is fuller in *idem*, 2 ed., 1896, 340–350, 376–384. For a clear, concise, elementary exposition of six of the theories mentioned above, see M. Collins, *Colour-Blindness*, 1925, 31–58.

Young-Helmholtz Theory

For Young, see Chapter 3; for the relation of the theory to color mixture, see Chapter 4; for the relation of the theory to color blindness, see Chapter 5.

For H. v. Helmholtz's chief contributions to the theory, see his Ueber die Theorie der zusammengesetzten Farben, *Ann. Phys. Chem.*, 163, 1852, 45-66 (reprinted in his *Wissenschaftliche Abhandlungen*, 1883, II, 1-23); Ueber die Zusammensetzung von Spectralfarben, *ibid.*, 170, 1855, 1-28 (reprinted in *Wiss. Abhandl.*, II, 45-70); *Handbuch der physiologischen Optik*, sect. 20, in the 1 ed., 1860, reprinted in the 3 ed., 1911, and the Eng. trans., 1924; see also the extended discussion in the 2 ed., 1896.

J. C. Maxwell's most important papers, where he develops the excitation curves from the color triangle of mixtures, are: Experiments on colour, as perceived by the eye, with remarks on colour-blindness, *Trans. roy. Soc. Edinb.*, 21 (2), 1855, 275-298 (reprinted in *The Scientific Papers of James Clerk Maxwell*, 1890, I, 126-254); On the theory of compound colours, and the relation of the colours of the spectrum, *Phil. Trans.*, 150, 1860, 57-84 (reprinted in *Sci. Papers of J. C. M.*, I, 410-450).

F. A. C. Donders' paper, supporting the Young-Helmholtz theory but also adjusting it to make it compatible with certain other facts (*vide infra*) is Ueber Farbensysteme, *Arch. Opthal. Berlin*, 27 (1), 1881, 155-223.

The classical paper of A. König and C. Dieterici is Die Grundempfindungen und ihre Intensitätsvertheilung im Spectrum, *Sitzungsber. preuss. Akad. Wiss.*, 1886, 805-829 (reprinted in A. König's *Gesammelte Abhandlungen*, 1903, 60-87).

Of course the Young-Helmholtz theory still survives in sophisticated form in modern views, like Hecht's; *vide infra*.

Duplicity Theory

The best discussion of the history and nature of the duplicity theory is

by A. Tschermak, Die Hell-Dunkel-adaptation des Auges und die Funktion der Stäbchen und Zapfen, *Ergebnisse der Physiologie*, I (2), 1902, 694-800, esp. 780-800. It has a classified bibliography of 274 titles. It is significant that the duplicity theory should have come to be discussed under the general head of *Adaptation*, when the origin of the theory had so little to do with adaptation.

The crucial paper on the rods and cones and the visual purple is H. Müller, Zur Histologie der Netzhaut, *Z. wiss. Zool.*, 3, 1851, 234-237. Müller's contribution was evaluated and placed in relation to the rest of the knowledge of the structure of the retina by the famous histologist, A. Kölliker, in particular in his Zur Anatomie und Physiologie der Retina, *Verh. phys.-med. Ges. Würzburg*, 3, 1852, 316-336, and in general in his *Mikroskopische Anatomie*, II, 1852, Pt. 2, 648-703, esp. 649-662, 690-703. Kölliker says that the rods were described by Leewenhoek (ca. 1720) but had to be rediscovered by Heuschke (1835) and Treviranus (1835). Certainly the distinction between rods and cones was not clear to Johannes Müller in 1838 and was not firmly established before H. Müller in 1851.

The paper of Max Schultze's that fixes the date of his 'duplicity' theory is Zur Anatomie und Physiologie der Retina, *Arch. mik. Anat.*, 2, 1866, 175-286, esp. 247-261, but Schultze made his ideas somewhat more explicit in Ueber Stäbchen und Zapfen der Retina, *ibid.*, 3, 1867, 215-247, and Bemerkungen über Bau und Entwicklung der Retina, *ibid.*, 3, 1867, 371-382.

A. Charpentier first expressed himself on this matter in De la vision avec les diverses parties de la rétine, *Arch. Physiol. Paris*, 2 ser., 4, 1877, 894-945. He held insistently to his view in many later short papers up to 1891; see Tschermak, *op. cit.*, for them.

For F. Boll's work on the retina and the visual purple, see his *Zur Anatomie und Physiologie der Retina*, *Monatsber. preuss. Akad. Wiss.*, 1876, 783-787, *Zur Physiologie des Sehens und Farbenempfindung*, *ibid.*, 1877, 2-7. W. Kuhne's various papers on the visual purple are in *Untersuch. phys. Inst. Heidelberg*, 1, 1877-1878, and 2, 1878. See esp. his study with E. Ewald, *Untersuchungen über den Sehpurpur*, *ibid.*, 1, 1877-1878, 139-218, 248-290, 370-455.

H. Parinaud first proposed his theory of the two retinas on the basis of his study of night blindness in L'héméralopie et les fonctions du pourpre visuel, *C. R. Acad. Sci. Paris*, 93, 1881, 286 f. For his discussion of peripheral vs. foveal adaptation, see his *De l'intensité lumineuse des couleurs spectrales; influence de l'adaptation rétinienne*, *ibid.*, 99, 1884, 937-939. He summarizes his view in *Sur l'existence de deux espèces de sensibilité à la lumière*, *ibid.*, 101, 1885, 821-823. For many other references to Parinaud, see Tschermak, *op. cit.* Parinaud carried on a controversy with Charpentier about this matter; he also pointed out that von Kries did not have priority in his duplicity theory.

A. König's important contribution to this topic is his *Ueber den menschlichen Sehpurpur und seine Bedeutung für das Sehen*, *Sitzungsber. preuss. Akad. Wiss.*, 1894, 577-598.

J. v. Kries established the duplicity theory briefly in *Ueber den Einfluss der Adaptation auf Licht- und Farbenempfindung und über die Funktion der Stäbchen*, *Ber. naturf. Ges. Freiburg i. B.*, 9, 1894, 61-70, and at greater length in *Ueber die Funktion der Netzhautstäbchen*, *Z. Psychol.*, 9, 1895, 81-123.

Hering Theory

The Hering theory starts from the criticism that the Young-Helmholtz theory does not take account of the

uniqueness of the six principal colors: white, black, red, yellow, green, blue. In general on the problem of simple colors, see Chapter 4, pp. 127-131. For the two criticisms that immediately antedated Hering's theory, see E. Mach, *Ueber die Wirkung der räumlichen Vertheilung des Lichtreizes auf die Netzhaut*, *Sitzungsber. Akad. Wiss. Wien, math.-naturwiss. Cl.*, 52 (2), 1865, 303-322, esp. 319-321; H. Aubert, *Physiologie der Netzhaut*, 1865, 177-186, esp. 186.

For E. Hering's account of his own theory, see his *Zur Lehre vom Lichtsinne*, 1878, 70-141, esp. 73-80 (light sensation), 107-121 (color sensation). These papers were originally published in 1874. After this Hering published many papers on such topics as color blindness and indirect vision, topics which brought facts to bear upon the question of the validity of his theory. His later statement of the theory, though more in respect of the light sense than of the color sense, occurs in his *Grundzüge der Lehre vom Lichtsinn*, 1920, 1-62, 100-115, 273-275, 290-294. This book was published posthumously; its parts were written in 1905-1916. The first two citations just given appeared in 1905 and 1907.

Later Theories

Nine of the most important theories of color vision are listed below. These theories represent modifications and elaborations of the fundamental principles which were, for the most part, brought out by Helmholtz and Hering.

W. Wundt, *Grundzüge der physiologischen Psychologie*, 2 ed., 1880, I, 450-460, esp. 455 f.; extended and further particularized in *idem*, 4 ed., 1893 (there were many new theories of color in that decade), 529-541, esp. 535-538; and continued to 6 ed., 1910. Wundt supposed that white and gray depend upon the amplitude of the light waves, and

that hue depends primarily upon the wave-length but secondarily upon the nature of the photochemical processes aroused in the retina—a theory of “polyform photochemical processes.”

F. C. Donders, Ueber Farbensysteme, *Arch. Ophthalm. Berlin*, 27 (1), 1881, 155–223. Donders anticipated Ladd-Franklin more than she ever admitted.

F. W. Edridge-Green, *Colour-Blindness and Colour-Perception*, 1891, esp. 30–54. The 1909 “edition” of this book is really a reprint. The theory became more explicit in his *Hunterian Lectures on Colour-Vision and Colour-Blindness*, 1911, 11–23, and *Physiology of Vision with Special Reference to Colour Blindness*, 1920, 263–273. Edridge-Green made the visual purple the sole retinal perceptual agent. He had little influence because he was not known in Germany when the theorizing was rife.

Christine Ladd-Franklin, Eine neue Theorie der Lichtempfindungen, *Z. Psychol.*, 4, 1892, 211–221; On theories of light-sensation, *Mind*, 2 ser., 2, 1893, 473–489; and, in general, her book where all her papers on color are reprinted, *Colour and Colour Theories*, 1929. The student who wishes to comprehend the full significance of Ladd-Franklin’s evolutionary theory should perform the following *Gedankenexperiment*. *Y* has become differentiated into *R* and *G*. Imagine that *B* has similarly, in the course of evolution, become differentiated into two new simple colors, *Q* and *S*. The color series about the color circle is then *R-Y-G-Q-B-S-R*. The following relations, given the right proportions of mixture, are necessary because they reverse the evolutionary process: $Y + B = W$, $R + G = Y$, and $Q + S = B$. It would be possible for *Q* to turn out to be the complementary of *R*, and *S* the complementary of *G*, so that $R + Q = W$ and $G + S = W$, but these relationships would be fortui-

tous and not the necessary consequences of the line of evolutionary differentiation.

H. Ebbinghaus, Theorie des Farbensehens, *Z. Psychol.*, 5, 1893, 145–238. Ebbinghaus had presented the theory briefly at the London International Congress of Psychology in 1892. He assumed two stages of decomposition of the visual purple to account for the perception of *Y* and *B*. Then he hypostasized two other retinal substances to account for *R* and *G* and for *W* and *Bk*.

A. König, Ueber den menschlichen Sehpurpur und seine Bedeutung für das Sehen, *Sitzungsber. preuss. Akad. Wiss.*, 1894, 577–598, esp. 589–598; reprinted in his *Gesammelte Abhandlungen zur physiologischen Optik*, 1903, 338–363, esp. 353–363. König’s theory assumes that the function of the visual purple is the perception of gray, of the visual yellow the perception of blue, while red, green and yellow depend on the pigment epithelium. The cones, he thought, are not receptors, but condensers.

G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Z. Psychol.*, 10, 1896, 1–82, 321–413; 14, 1897, 1–76, 161–196. For the psychophysical axioms, see 10, 1–4, 15–20. For cortical gray, see 10, 30–32, 411 f., and 14, 40–46 (esp.), 60–64. For the rest of the theory, see 10, 338–413.

W. McDougall, Some new observations in support of Thomas Young’s theory of light- and colour-vision, *Mind*, 2 ser., 10, 1901, 52–97, 210–245, 347–382. This account includes reports of *experimenta crucis* on visual induction and inhibition.

F. Schenck, Theorie der Farbenempfindung und Farbenblindheit, *Arch. ges. Physiol.*, 118, 1907, 129–180, esp. 176–180.

The theorists were not quite so free with imaginary physiology as these simple *ipse dixit*s imply. There was a great mass of argument from experiment and from less systematic

observation. There is also running through the history of the theories an important thread of theorizing concerning what axioms of psychophysical parallelism could be accepted *a priori*, a subject that became explicit in Fechner (1860), Mach (1865), Donders (1881) and G. E. Müller (1896).

Modern Theory

On the all-or-none theory of nervous conduction, see Chap. 2. L. T. Troland's early recognition of the relation of this theory to visual theory

is his The 'all or none' principle in visual response, *J. opt. Soc. Amer.*, 4, 1920, 160-185, esp. 160-164, 181-185.

For S. Hecht's theory, see his The nature of the photoreceptor process, in *Handbook of General Experimental Psychology*, 1934, 704-828, and references there cited, including 31 to himself. His work is part of the larger movement of general physiology to apply physical and chemical principles to the reactions of the intact organism, research which has occupied many pages in the *J. gen. Physiol.*, since its beginning in 1918.

Chapter 7

VISUAL PERCEPTION OF BIDIMENSIONAL SPACE

THE history of the psychology of the visual perception of space introduces philosophical considerations in sensory physiology to an extent not true for the psychology of color. The earliest problem of visual space perception was a simple one of physiological optics: how do the spatial relations of the external world get represented in the image upon the retina so that the 'sensorium' can perceive them? Only when this question had been answered, could the second problem be faced: how, if the retinal image is bidimensional, do we come to perceive the third dimension? Do the eyes provide cues other than the form of the retinal image for the perception of depth?

It was at this point that philosophy came into psychology by way of Kant's doctrine of space as an *a priori* intuition. Is visual space actually native in perception, as Kant had it, or is it generated in experience, as British empiricists believed? Out of this problem arose the long and barren controversy about nativism and empiricism. Descartes, Kant and Hering—to span the ages—were nativists. Locke, Lotze and Wundt were empiricists. Wundt, in fact, was so much of an empiricist that he never even admitted that extensity is a sensory attribute like quality and intensity—for the attributes were supposed to be innate, not learned. His system emphasized the distinction between the simple, elementary, native *Empfindung* and the complex, meaningful, learned *Vorstellung*, and it described *Empfindungen* as organized in respect of space (cf. Kant!) into *Vorstellungen*.

Wundt's distinction has continued, somewhat obscured, into the present century. Analytic physiological psychology finds the perception of space more complicated than the perception of color. In general, it has been content to deal with color as dependent solely upon retinal processes, but for space it has had to add to the shape of the retinal image such indicators of depth as accommodation of the lens and, in binocular vision, the convergence of

the eyes. Gestalt psychology, on the other hand, represents a return to the nativism of Hering. It notes that extension, shape and even depth are just as immediate in perception as hue and brightness and it looks more to the brain (though in that it passes beyond Hering) than to the retina for its explanatory hypotheses.

The Retinal Image

It is easy enough nowadays to scoff at the Epicurean theory that objects emit images which are projected into the eye to be perceived by the sensorium; yet a more sophisticated theory of vision requires a greater knowledge of optics than the ancients had. Though they realized, of course, that the eye is the percipient organ and that light travels in straight lines, they knew nothing about the formation of images. Alhazen (d. ca. 1039 A.D.) had designated the crystalline humor as the sensitive element in the eye without recognizing that it is a lens, and his view persisted for a long time. The *camera obscura*—the arrangement by which light, entering a dark room through a very small hole, casts upon a screen or wall an inverted image of the illuminated field outside—was known in the sixteenth century. Leonardo da Vinci (ca. 1519), for example, knew it and compared the eye to it. G. B. Porta (1591) later developed this analogy, assuming the crystalline humor to be the screen upon which the image is cast. Yet Porta, though he knew about the image-forming power of lenses, did not recognize the “crystalline” as a lens. F. Maurolycus, however, had already (1575) identified it as such and suggested its function to be the formation of images.

It was Johann Kepler, the astronomer, who, in a work on optics published in 1604, gave the correct view with sufficient argument to establish it with considerable certainty. Noting once more that the eye is like a *camera obscura*, he demonstrated in detail how that principle works. The surprising thing about a *camera obscura* is that the image of the sun, projected through a square hole, is nevertheless round, but Kepler was able to show in detail why the image of a square object projected through a triangular hole is square and the image of a triangular object through a square hole triangular. Because the eye has a lens, however, it is not, he argued, a simple *camera obscura*. He showed exactly how the lens must

form an image at the back of the eye, concluding that the retina must therefore be a sensitive screen upon which the image falls. Noting further that the image would be blurred except for objects at one particular distance, he concluded that the eye must have the capacity of adjusting itself for vision at different distances. Such an adjustment, it seemed to him, would have to take place by a variation of the distance of the lens from the retina. He noted too that the retinal image is inverted, right for left and up for down, and went on to pose without solution that problem which bothered so many after him, as to why we see rightside up when the image is upside down. This is, to be sure, a pseudoproblem, for 'up' and 'down' and 'right' and 'left' have no meaning in the image except as it is brought into relation with something else, with the object imagined or with the real retina, since the mind does not see the object. It sees its image; nor does it see the retina when it sees the image.

That the image on the retina would actually be visible were there another eye to see it was Kepler's belief. It is said that Scheiner carried out just such an experiment in 1625. Descartes, at any rate, made the experiment and described it fully in his *Dioptrique* in 1637. By placing the eye of a bull in a hole in a shutter after removing the thick coats from the back of the eye, he received the image on a piece of thin paper placed in the position of the retina. So modern are Descartes' diagrams of the optics of the formation of the retinal image that it is hard to realize how recent that knowledge was.

After this for a very long time the retinal image was practically identified with perception. Euclid in his geometrical analysis of visual perception had, as a matter of fact, equated the apparent size of an object to the visual angle which it subtends, and now W. Molyneux (1692) insisted on the same relationship. He was, in short, saying that extension in perception corresponds exactly to extension in the retinal image. Berkeley (1709), on the other hand, was somewhat more sophisticated. Naming the possible criteria for the perception of distance, he showed that the perception of magnitude *in the object* would have to depend on a knowledge of the distance of the object; but his whole argument was based on the assumption that the size of the retinal image is immediately apparent to the observer. If the observer knows the size of the image

and the distance of the object, Berkeley argued, he can become aware of the size of the object.

This view was not greatly altered in Helmholtz's time, much more than a century after. Its exact correctness was, however, challenged a little later when, toward the end of the nineteenth century, the optical illusions became one of the important centers of interest. Yet the illusions for the most part turn out to be exceptions that prove the rule, for, if illusion were the rule, it would not be called *illusion*. So it happened at the beginning of the present century that psychologists still thought of apparent magnitude as measured by visual angle, that is to say, by the size of the retinal image. Looking upon illusions as interesting exceptions, they succeeded in bringing many directly under the rule by hypotheses that explained them as alterations in the actual size of the retinal image. Though later Wertheimer and the Gestalt psychologists attacked this "constancy hypothesis" of the equivalence between the perceptual and retinal patterns, the fact remains that retinal extension is still regarded as the fundamental factor among those that determine extension in the visual perception. We must return to these exceptions later.

The problem of the inversion of the retinal image shows a conflict between two points of view, the old and the new, regarding perception. Astonishing to modern psychologists is the length of time it took for the new view to prevail. Kepler (1604), as we have seen, raised the problem and did not solve it. Molyneux (1691), however, for all that he was not very sure of himself, already had the right answer. Berkeley (1709), with the same right answer, spoke out with assurance, yet the length of his argument is a measure of how well entrenched he conceded the contrary view to be. Johannes Müller (1826, 1838) repeated the solution even more positively. Only by the time of Helmholtz (1866) could the new view be taken for granted and the discussion of the pseudoproblem dismissed. What were the two views?

The older theory of perception regarded the mind (the soul, the sensorium) as a personal entity within the head, shut off from the external world but seeking information about it and perceiving the representatives (images, copies) of objects that the nerves bring to it. This was the common-sense theory: a cranial homunculus able to perceive directly anything that comes within range of its apprehension. The mind, according to such a view, would see the

image *on the retina* and would see it inverted because it is inverted. Thus, since the mind does see objects without inversion, there was a problem.

The newer theory held that the mind knows only what comes to it in sensation. The mind sees the image, but not on the retina. The image in itself is perceived neither as erect nor inverted. If by *erect* we mean that the tree's branches are near the sky and its roots at the ground, then the retinal image of a tree is erect when its imaged branches are near the imaged sky and its imaged roots at the imaged ground. To say that the image is inverted would be to know the relation of the retinal image to the actual external object of which it is an image, or to the real sky and the real ground, and this relation of the image to the world which it images has been known only to natural philosophers who lived after Kepler's time.

Molyneux said: "*Erect and Inverted* are only Terms of *Relation* to *Up and Down*, or *Farther from* and *Nigher* to the Centre of the Earth. . . . But the Eye or Visive Faculty takes no Notice of the Internal Posture of its own Parts, but uses them as an Instrument only." Berkeley made this principle of *relativity* even more explicit. The retinal image, he argued, is not in itself inverted, for all high objects lie in one part of it and all low objects in the opposite part of it. It can be considered inverted only when the world of touch is brought into relation to the world of sight, as when the eye turns upwards to view objects that have been imaged on the bottom of the retina. Johannes Müller, who cited A. W. Volkmann as agreeing with him, said: "Even if we do see objects reversed, the only proof we can possibly have of it is that afforded by the laws of optics; and, if every thing is seen reversed, the relative position of the objects of course remains unchanged." He ended by observing that what is erect at noon is inverted by midnight because of the rotation of the earth, and yet we notice no change! So the final solution of the problem lay in the recognition of the relativity of space, but it took a long time for the solution to prevail over the popular theory of the absolute perceptions of the homunculus within the head. (Cf. pp. 237 f.)

Singleness of Vision and the Horopter

The solution of the problem of the inverted retinal image was relativistic: *up* in the image is simply that part of the image where the things that mean *up* lie (the sky, people's heads, the branches of a tree), and the mind does not see the retina in relation to the earth and the sky. There still remained, nevertheless, the question as to whether the different parts of the retina do not have some fixed and constant characteristic that identifies each, a "local sign" as it came later to be called. It was the implicit belief in some such absolute characteristic of retinal position that made the relativistic solution of the problem of the inverted image so hard to accept. If the bottom of the retina is labeled as different from the top, does it not come labeled as the bottom of the retina? or does it just have a characteristic label which is later learned to mean the top of the seen world? On this question turned a long debate about nativistic and empiricistic theories of space perception, a debate which we must consider later. Here we are concerned with the precursors of the local sign.

In this connection we have first to consider the problem of the singleness of binocular vision. It has always been obvious that, although there are two eyes, they seem to give but a single view of the external world. If the mind sees retinal images, why does it not see both of them? It takes only a little careful observation, however, to show that not all of the field of vision appears single when it is viewed with both eyes, and it was only nine years after Kepler had established the fact of the retinal image that Aguilonius defined the horopter.

Aguilonius (1613) coined the word *horopter* to indicate the locus of all points seen as single in the binocular field of vision. Incorrectly, he thought of the horopter as a plane containing the point of fixation. The fact is that the point of fixation, seen single, moves toward the eyes as they converge upon a nearer object. Thus it seemed to him that it would be objects behind and in front of the plane of fixation that appear double, whereas objects in such a plane would be seen as single. This conception really implies the notion of corresponding points on the two retinas. If all objects in the plane of fixation are seen as single, it must be that the eyes have been converged so that not only do the images of the fixated point fall upon corresponding points in the two eyes, but also the

paired images from every point in the plane fall upon corresponding points. The images of farther and nearer objects would then fall upon other pairs of points, and such images would appear double. In this manner the concept of the horopter makes the problem of the singleness of binocular vision into a problem of corresponding points.

Because this relationship was not at all clear at first, there were three different theories of the singleness of binocular vision.

(1) In the first place, there was the projection theory which Kepler (1611) had proposed. The images, he argued, are projected into external space; corresponding images, being projected to the same place, cannot appear double since they are in the same place. Even Aguilonius looked with favor upon this view, and Porterfield sponsored it in his *Treatise on the Eye* in 1759. The theory fails because it does not show why some images are correctly projected and seen as single, whereas others, incorrectly projected, are seen as double.

(2) There was also the theory of alternate vision, the suggestion that we see, now with one eye and now with the other, but never with both at once. This theory gained support from cases of cross-eyedness and other defects in binocular coordination. It was held by Porta (1593) and certain other writers of the seventeenth and eighteenth centuries. Like the preceding theory, it is inadequate because it does not explain why there are any double images, nor why it is always the fixated object that is seen as single.

(3) The third and correct theory of the singleness of binocular vision is anatomical, taking into account the partial decussation of the fibers of the optic nerves in the optic chiasma. This view actually goes back to Galen (ca. 175 A.D.), who thought that the fibers from the two eyes were connected in the chiasma. Newton (1717) was quite specific. He suggested that the fibers from the right half of the left eye cross at the chiasma and join the fibers from the right half of the right eye, connecting either there or subsequently at the Sensorium, so that all the fibers from the right halves of the two retinas go to the right half of the brain; and conversely for the left halves. David Hartley (1749), the physician-philosopher-associationist, implied the same relationship among the optic fibers, except that he entertained the possibility of the impression in one eye being actually propagated to the other. He spoke definitely of corresponding points in the two retinas. W. H.

Wollaston, the contemporary of Thomas Young, told in 1824 how, after excessive exertion, he had twice lost sensitivity of one-half of the field of vision, a loss which he ascribed to the distribution of the optic fibers in the way that Newton had suggested. He argued then, as we argue now, that blindness of half of the field of vision must be due to the "semi-decussation" of the optic fibers. Thus,

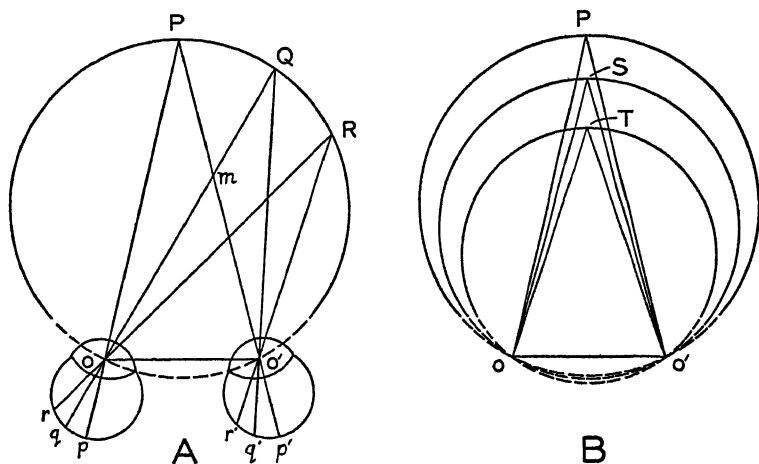


FIG. 27. HOROPTER-CIRCLE: JOHANNES MÜLLER (1826)

A. Only if the points P , Q and R lie in a circle will their images fall on corresponding points in the two retinas. Corresponding points are p and p' , r and r' , q and q' because r and r' are separated respectively from p and p' by equal angles, and so also for q and q' . O and O' are the optical centers of the two eyes. The horopter circle is dotted near and through the eyes where no points can actually be seen. For geometrical proof, see text.

B. Three different horopter-circles for three different distances of fixation at P , S and T . For the true horopter-line, see Fig. 28.

when Johannes Müller reviewed the theories in 1838, this fact was already pretty well established, and the general principle has never been questioned since.

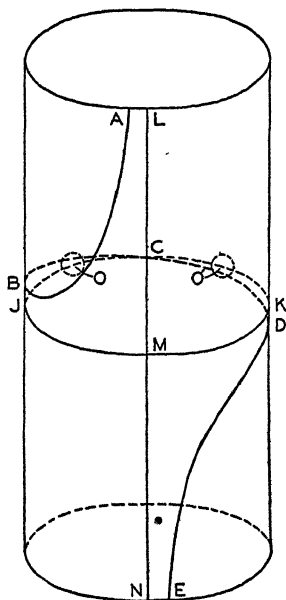
Meanwhile the conceptions of the horopter and of corresponding points had been developing. Aguilonius (1613) had thought, as we have seen, that the horopter must be the vertical plane of fixation, a view that would make the horopter in the horizontal plane of the eyes a transverse straight line at the distance of the fixation point from the eyes. That is not correct. The horopter in the horizontal plane is a circle that passes through the point of

fixation and the optical centers of the two eyes. Although this relationship was first worked out by Vieth in 1818, general knowledge of it is due to the exposition of Johannes Müller in 1826, who discovered it independently and who advertised the fact so well that this horopter came to be called "Müller's circle."

Fig. 27 shows the argument of Vieth and Müller, which was geometrical, not physiological. O and O' are the optical centers of

FIG. 28. GENERALIZED HOROPTER-LINE:
HELMHOLTZ AND HERING (1863-1864)

The general horopter is a curve, symmetrical above and below, that can be plotted in the surface of a cylinder. The curve is asymptotic to the line LMN in the front of the cylinder. The eyes are represented by circles at the back of the cylinder with their optical centers at O and O' . The horopter-line is $ABCDE$. The invisible portion of the line that passes through the eyes is dotted, BCD . In a special case this horopter becomes the horizontal circle, $CKMJ$, and the vertical line, LMN . The circle, $CKMJ$, is Müller's horopter-circle; see Fig. 27.



the two eyes, which fixate P , forming images at p and p' . Thus p and p' are corresponding points. It is argued then that other corresponding points, q and q' , r and r' , would be separated from p and p' by equal angular distances; that is to say, the angle pOq equals the angle $p'O'q'$. Hence the opposite angles are equal: $POQ = PO'Q$. But the opposite angles at m are also equal, so that the triangles POQ and $PO'Q$ are similar, and the angles OPO' and OQO' are equal. Hence P and Q must lie on a circle whose chord is OO' , since all triangles, erected upon a chord of a circle to a point on the periphery of the circle, have equal angles at the periphery. The same argument places R on the circle, if r and r' are corresponding points. Thus theoretically the horopter is a

circle, although actually the images of objects near the eyes and within the eyes cannot fall on the retina and the circle is interrupted (cf. dotted portion). If the fixation point changes from *P* to *S* or *T* (Fig. 27B), the horopter changes to a circle of different size.

That Müller's circle is the horopter only in the horizontal plane and for fixation in that plane was soon clear. When the circle is the correct form, the horopter also includes the straight vertical line that runs through the points of fixation; but this is a special and limiting case. Helmholtz and Hering, working independently and criticizing each other in successive papers, concluded in 1862–1866 that the general case of the horopter is a line, a curve of the fourth degree representing the intersection of a hyperboloid with a cone. The curve has the form of the line *ABCDE* in Fig. 28. Asymptotic to the vertical line *LMN*, it is symmetrical above and below about its midpoint. In one special case this curve becomes the circle *CKMJ*, which is Müller's circle (Fig. 27) and the straight line *LMN*.

Eye-Movements

Johannes Müller's monograph of 1826 on eye-movements in relation to human vision was more or less the beginning of a series of monographs and articles on the anatomy of the eye with the exact measurement of its dimensions, the dioptrics of the eye with the determination of the constants that affect the formation of the retinal image, and movements of the two eyes as they are related in the convergence of common fixation. In the books of Helmholtz and Hering in the 1860's this interest came to a culmination. It had resulted in the accumulation of a very considerable body of detailed descriptive fact. We have here to consider, however, only the way in which the facts of eye-movement came into relation to the problem of visual space perception.

The two most discussed laws of eye-movement are Donders' law and Listing's law. The facts of the former were established by Donders in 1846, although it was Helmholtz in 1866 who called the principle a law and named it for its originator. Listing's law was formulated and thus named by Reute in 1853. Apparently Listing never published the law himself. To understand the importance of these laws is to understand one of the problems of visual space perception in the middle of the nineteenth century.

Donders' law states the principle that for any direction of regard

the eye always assumes the same position. It is conceivable that the eye might rotate about its own axis as it variously changes fixation, and so come to a given direction of regard, sometimes with one amount of torsional rotation and sometimes with another. Donders' law denies such variation. If there is torsion, it is always the same for any direction of regard. The effect of the law is to establish the fact that the retinal image of any object fixed in the field of vision will, for any given position of the head and fixation of the eye, always fall upon the same retinal points. Thus Donders' law becomes the basis for a constancy theory of localization: if the orientation of the head and eye are known, then the position of a perceived object in relation to the vertical and horizontal of the visual field is determined by the retinal points stimulated.

Listing's law states that, when the eye moves to any position from the primary position (the natural position in looking horizontally straight ahead), it rotates about an axis that is perpendicular to the initial and final lines of regard at the point of their intersection. This is the simplest sort of rotation possible. It is rotation without torsion: or at any rate, there is no torsion of the eye about the line of regard as an axis. So simple is this principle that it is often supposed to be self-evident. Why should the eye undergo torsion about its own axis in moving from one fixation to another? The importance of the law becomes apparent, however, when one understands that the geometry of rotation requires the eye under ordinary circumstances to undergo some torsion.

Consider the block shown in Fig. 29. This block is so fixed that it can rotate about the point *O*. If it rotates from position 1 to position 2 without torsion and about *O*, it will be oriented as shown. If next it rotates without torsion from position 2 to position 3, then it will again lie as in the figure. If finally it is rotated without torsion from position 3 back to position 1 again, then we are faced with the contradiction that altogether it has undergone torsion through 90° , for the shaded side will now be up instead of in front. The three motions described are the sort of motions prescribed by Listing's law. Yet these three motions, each separately without torsion, have the effect of producing very considerable torsion. Thus, if the eye *always* moved in accordance with Listing's law, then it would disobey Donders' law, because it would come back to the same position with different amounts of torsion about its own axis. Hence it follows that, since Donders' law is true, the eye is subject

to torsional rotation in most of its movements. Listing's law states only a special case, viz., that movement *from the primary position* is without torsion.

There is still the question as to why psychologists became interested in the special case of movement from the primary position.

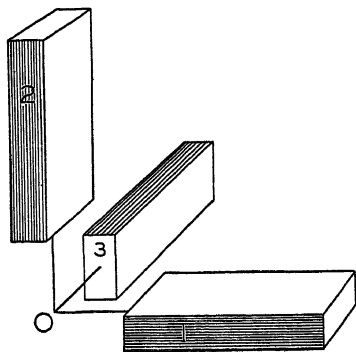


FIG. 29. KINEMATICS OF OCULAR TORSION

If the block were rotated from position 1 to position 2 about an axis through *O* perpendicular to the plane of rotation, it would be rotating according to Listing's law. Similar rotation would bring it to position 3. If it were now rotated in the same way back to position 1 again, it would be found to have undergone 90° of torsional rotation: the cross-hatched side would now be up instead of forward. Thus the block would not obey Donders' law. Since the eye obeys Donders' law, it follows that it cannot move in accordance with the simple principle of Listing's law (without torsional rotation) *except* when moving toward or from the primary position.

the image of an object moved from the given point to the center of the retina, that is to say, if the eye moved so as to fixate the peripherally perceived object. Since the eye tends automatically to fixate peripheral objects, these associations might easily be set up—so Lotze thought in 1852. His theory depended, however, on the truth

in the special case of movement from the primary position. This question is answered as follows. Donders' law stated that for any direction of regard the eye always has the same torsional position. But just what torsional position does it have in any given position? It has the position that would be given it by moving from the primary position to the given position in accordance with Listing's law. Thus Donders' law shows that the actual position of the eye is the same for any direction of regard, and Listing's law specifies what that position is.

It must have been all this contemporaneous discussion of eye-movements that led Lotze to his theory of visual local signs in 1852. Though he did not mention Donders or Listing in that discussion, Lotze did assume simplicity and constancy of eye-movements. Any given retinal point, he argued, becomes associated in experience with the series of impressions that would be produced if

of the principle that the eye moves from any one position to any other in accordance with immutable laws and not fortuitously.

Nativism and Empiricism

We have already seen (pp. 27-32) how, in the second half of the nineteenth century, there was a great to-do about the origin of the individual's idea of space. British empiricism made it appear that the knowledge of space, like the knowledge of everything else, comes out of experience by way of the senses. Kant, holding that space is an *a priori* intuition, added the weight of his prestige to the view that the conception of space is innate in the individual and is not learned. Johannes Müller, acknowledging Kant's authority in this matter, held that spatiality is intrinsic to the mind, which arranges its impressions so as to build up its idea of space. Hering was the first explicit nativist, however, writing after Lotze, who was, except for the associationists, the first explicit empiricist. The later exponent of nativism was Stumpf.

Although Lotze can be placed at the head of the modern empiricist line, he was, in this matter as in other philosophical affairs, a compromiser and synthetizer: he admitted the Kantian contention that spatiality is intrinsic to mind, but held further that space, as it is known, is something more than spatiality, that it is an organization or system generated in experience. To the support of Lotze came Helmholtz and Wundt, who thus bore the weight of the empiricist tradition over against Hering and Stumpf. Külpe and Titchener belong with Hering in that they believed extension to be a primitive attribute of visual and tactual sensations, but they also belong with Wundt in their assertion that localization is learned. Gestalt psychology stems from Hering: for it, form as well as extension is native.

No simple exposition of this great and largely fruitless controversy can, however, be adequate to its complexities. For one thing, almost every protagonist turns out, whatever he was called, to have been both nativist and empiricist. Everyone believed that the organism brought something congenitally to the solution of the problem of space; everyone believed that the organization of space may be altered or developed in experience. The differences lie in the way the men viewed the different parts of the total problem,

for within the total problem we find part-problems, which have, for the most part, not been clearly distinguished and which therefore confuse the issue. Let us take up these part-problems separately.

(1) *Spatiality*. Although the nativists looked to Kant for authority in their quarrel with the empiricists, the fact is that both groups admitted one fundamental Kantian principle—namely, that the capacity of the mind to conceive such relationships as ultimately come to make up the idea of space is given in its primary nature and not developed in particular experience. To accept this view was natural enough for Johannes Müller, writing in 1826, so soon after Kant. It was natural too for Lotze in 1852 to suppose that the local signs would never have got themselves related into a concept of space had not spatiality been intrinsic to the mind. Helmholtz, who was at great pains to show that the axioms of Euclidean geometry are founded upon experience and are not innate, nevertheless admitted that space itself may be 'transcendental.' So too Wundt was really allowing the validity of this Kantian category when he held that the sensory elements can be organized in space to make up *die räumliche Vorstellungen*. Wundt admitted only quality and intensity as attributes of sensation; space and time were in his system frames of reference for the patterning of sensory complexes. Külpe, on the other hand, in bringing in space and time as attributes of sensation showed that he thought extension and duration to be just as primitive in experience as quality and intensity. Titchener followed Külpe. Although he said in 1910 that this primitive experience of extensity is necessary for the idea of space, he did not believe that native extension explains localization any more than did the other empiricists. There was, therefore, really no psychologist in the nineteenth century to deny the Kantian doctrine of the *a priori* nature of space at its fundamental level. The difference of view appeared at the higher level of the organization of spatial perception.

(2) *Local sign*. The outstanding fact about visual and tactual space perception is that the spatial relations of the pattern of stimulation get themselves more or less correctly represented in perception. Perception, within limits and with known exceptions, mirrors stimulation. Thus in general the stimulation of three points, *abc*, in a row, with *b* lying between *a* and *c*, gives rise to a perception *ABC*, with *B* between *A* and *C*, and a correspondence of *A*

with *a*, *B* with *b*, and *C* with *c*. This fact means not only that the three stimulations, *a*, *b* and *c*, cannot be identical but that, because of their differences in position, they possess local characteristics which are reflected in the differentiation of the perception. In this sense every point must have a 'local sign,' be that sign a conscious quality immediately indicative of the position of the point or a physiological potentiality for giving rise to different perceptual effects. Such an effect in perception may be a label of locality, like the visual image of a tactual stimulation, or it may be a perceived relation, like seeing the order *ABC* for the stimulus order *abc*. In this broad sense all the physiological psychologists of the nineteenth century believed in local signs.

As we have already seen (pp. 29 f.), it was Lotze who, in 1852, originated the theory of local signs. That every retinal and tactual point is distinguished from every other one was his chief tenet. For him these local labels were not simple qualities that varied from place to place, but patterns of intensities—tactual patterns for touch, intensities of eye-movements for vision. His general theory was never worked out in detail, was open to many objections; nevertheless it started a trend in thought that occupied attention for a long time.

Wundt, rejecting Lotze's notion of intensive patterns, believed in distinct local qualities which characterize every retinal or cutaneous point of stimulation. He founded this belief upon the introspective fact that difference in localization is as immediately obvious in perception as any other qualitative or intensive difference. To an empiricist, like Wundt, the local signs had to be purely qualitative, or at least not consciously spatial; while they serve to differentiate the sensations, they do not at first signify the spatial arrangement of the sensations. The spatial relationships must be learned.

The nativists, on the other hand, though also believers in local signs, thought of the signs themselves as intrinsically spatial: given the signs for *a*, *b* and *c*, one could know without experience that *b* lies between *a* and *c*. As we have already seen, Hering and Stumpf were the chief exponents of this view. Hering went so far as to posit three local signs for every retinal point: one for height, one for breadth and one for depth.

There were still other psychologists who believed that the local *differentia* is unconscious. For them two sensations could be iden-

tical in all conscious respects and yet have different potentialities for their effects. Many of the associationists held such a view. They considered sensations to be differentiated spatially, not by their own conscious characteristics, but by the different associations that they are able to acquire. Külpe made this position explicit. Sensations in different places may be identical in quality, intensity, extent and duration, he held, and yet come to be distinguished associatively. For instance, identical pressure sensations on the left and the right index fingers are immediately discriminated by the associative context of visual imagery that localizes each differently. In other words, sensations alike in all of their conscious attributes are still capable, because of a physiological difference in their excitations, of forming different associations. Titchener's theory was similar to Külpe's.

Thus there were four views about the local sign. Lotze thought of it as (a) an intensive sensory pattern. Wundt held that it is (b) a simple quality. Külpe found evidence only for (c) an unconscious physiological predisposition. These men all count as empiricists because they thought that non-spatial characteristics, which vary with position although not intrinsically spatial, come by learning to signify position. Hering, on the other hand, held that the local sign is (d) originally intrinsically spatial, indicating position at once without learning.

(3) *Localization*. Obvious from what has been said is the fact that the empiricists believed the discrete qualitative local signs become related in experience in such a way that each point acquires a position in relation to the others and all the points together form a spatial continuum. These relationships were established, they believed, by the continuous movement of stimulation across the retina or the skin. That was Lotze's view and Wundt's view. One learns that *b* lies between *a* and *c* because the order of stimulation is *a-b-c*. Wundt eventually developed an elaborate schema of space perception in which all visual and tactual localization was established by reference to the kinesthetic sensations that are aroused in the movements of the eyes and body. The continuity of space, he held, comes from the continuity of experience in movement.

Strange to say, the nativists did not succeed in avoiding the problem of the genesis of space. Hering, for one, was forced to explain how visual space is built up in relation to a "nuclear

plane," as well as to meet many other difficult problems of the organization of visual space. His nativism did not spare him much pains.

Status praesens. We have already considered in Chapter 1 (pp. 32 f.) the fates of nativism and empiricism. The nativism of Hering is now lost in the larger nativistic phenomenology of Gestalt psychology. The empiricism of Helmholtz has given place to the behavioral theories of the positivists. These systematic faiths are, however, no longer so important as the experimental findings. The modern emphasis is on fact, though the underlying systematic bias may determine emphasis of the interpretation. Let us examine, with this matter in mind, two experiments neither of which is yet out of date.

(1) The first experiment is empiricistic and associationistic in that it shows how a localizing context can be changed by learning.

Stratton in 1896—the insight of this experiment is modern even if the date is not—tried the effect of reversing the field of vision, up for down and right for left, by a system of lenses which he wore during the waking hours of eight consecutive days. Though confused at first, he presently became quite satisfactorily adjusted to the inverted field. What did this discovery mean? In the older terms it would have meant that the local signs can acquire in experience either new spatial qualities or new relations to one another. Stratton, however, envisaged the problem as one of external relativity—"the interorganization of motor, tactual, and visual experience." If a man, seeing the ceiling and reaching for it, finds the floor, he is not well oriented in space, and that is approximately what happened when the reversing lenses were first put on. Later, after a period of conflict and of deliberately being 'right' by moving the 'wrong' way, the seen world 'turned right-side up' in the sense that the correct movement followed automatically upon the visual perception. Thus Stratton may be said to have settled both Kepler's problem of erect vision with an inverted image and Lotze's problem of the role of experience in space perception, by showing that the 'absolute' localization of retinal positions—up-down and right-left—are learned and consist of bodily orientation as context to the place of visual excitation.

(Ewert repeated Stratton's experiment in 1930, achieving similar results with almost complete readjustment in twelve days of

wearing the lenses. He worked quantitatively, plotting the learning curves which describe the course of readjustment.)

(2) The other experiment, in spite of the fact that it involves the learning of spatial relationships or form, is fundamentally nativistic, because it offers no explanation as to how a straight vertical line in visual perception differs from a bowed vertical, except to assert that the one or the other is given directly in experience. Gestalt psychology, of course, tends to interpret these results as meaning that the straight line is more 'natural' than the bowed, but that too is a kind of nativism, a faith that certain dynamic principles are given in the nature of the organism, not acquired in experience.

Wooster in 1923 and Gibson in 1933 found that perceived vertical lines, when bowed in perception by having the subject wear prismatic wedges in a spectacle frame, become straight if the wedges continue to be worn. If for both eyes the wedges taper toward the left, then the verticals appear bowed to the *left*. After the lines have become straight under 'adaptation' and the glasses are removed, verticals will for a while appear bowed to the *right*, a case of 'successive contrast.' This whole process can take place in an hour and the amount of 'learning' be measured by seeing how much a flexible rod must be bent in order to appear straight. In other words, we see straight verticals as they are, not so much because they are such, as because the organism is the kind of organism that tends to see verticals and near-verticals straight.

Geometrical Optical Illusions

Strictly speaking, the concept of illusion has no place in psychology because no experience actually copies 'reality.' Certainly John Locke's secondary qualities are 'illusory' in the sense that they do not correspond to the object, and Lotze remarked much later that color is an illusion because the color is "in us" and not in the outer world where there exists only light of various wavelengths. Lotze in his chapter on sensory illusions (1852) discussed color, sensation in general, the two-dimensional projection of the three-dimensional world on the retina, after-images, the perception and after-images of rotation, and many other phenomena where the lack of correspondence between sensation and stimulus is obvious. In the sense that perception is normally dependent

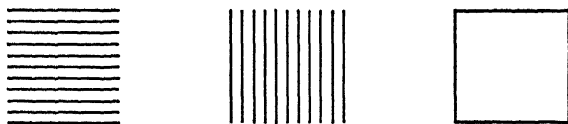
upon subjective factors as well as upon the stimulus, all perception is 'illusory' in so far as it does not precisely mirror the stimulus. In this broad sense the term *illusion* becomes practically meaningless.

The psychologists of the late nineteenth century, nevertheless, found themselves much concerned with illusions, especially with the geometrical diagrams that give rise to such visual perceptions of extent as form exceptions to the general rules. To account for these exceptions theory after theory was devised. The interest was clearly recognized about 1860. Thereafter, for perhaps three decades, a few of the classical illusions were discussed whenever the problems of visual space perception were systematically considered. A knowledge of the principles governing the abnormal perception of extent would certainly help, it was thought, with the understanding of the normal cases. Then in the 1890's there was a sudden culmination of this concern with the subjective conditions of form. Papers and theories multiplied. Not only were new illusions found, but variant forms of the old illusions were fixed up as *experimenta crucis* for the theories. One bibliography lists thirty articles on these illusions during the 1890's, though less than half a dozen in each of the three preceding decades. In the present century the study of illusions has continued as an interesting but not important sector of psychology. The rise of Gestalt psychology, however, has created a new concern with the ways in which visual perception realizes its form and pattern independently of the stimulus, or at least without mirroring the pattern of the stimulus. Thus Gestalt psychology has made the old problem of the illusion broader, given it a new significance, and provided it with a new set of explanatory principles.

The interest of psychologists in geometrical illusions may be said to have begun (ca. 1833) with the reversible perspectives, but we must leave this matter for discussion later (see pp. 268-271). Subsequently there arose, first the problem of interrupted extents, and then the problem of the overestimation of verticals in respect of horizontals.

In a study of geometrical illusions carried out in 1855, Oppel showed that a row of dots appears to occupy a greater extent than the same empty distance between two dots, and that a line interrupted by cross-bars seems longer than the same line without the cross-bars. He made up, in fact, various illustrations of this prin-

ciple. Here was an important fact of space perception, one which Hering tackled in his first important scientific publication (1861). Hering suggested that the perceived distance between dots might be taken to depend on the chord of the arc connecting the dots on the spherical surface of the retina, and that the chord between the two extreme dots, with empty space between them, would be shorter than the sum of the chords between successive points in the row of dots. Kundt (1863) and Aubert (1865) both made measurements of the illusion. In 1866 Helmholtz published



HELMHOLTZ'S SQUARES

FIG. 30. INTERRUPTED AND VERTICAL EXTENTS: HELMHOLTZ (1866)

the two squares of lines shown in Fig. 30 to show that the effect of interruption may more than compensate for the overestimation of verticals. Because the two tendencies work together when the interruptions lie in the vertical dimension, the left-hand square looks much taller than wide. The middle square, however, looks broader than it is high, for here the horizontal interruptions more than overcome the lengthening effect of the vertical.

It was Wundt who, in 1858, called attention to the tendency to overestimate the vertical as compared with the horizontal. Already he was supporting the theory that judgments of extent are based upon eye-movements. Because of the arrangement of the eye-muscles, he pointed out, vertical eye-movements are more difficult than horizontal. The eyes need, moreover, to have greater freedom for horizontal motion in the case of dwellers on the horizontal surface of the earth. This general view Wundt maintained all through his life, although in the twentieth century eye-movements have been shown to be less important in judgments of extent than Wundt had supposed them to be. Helmholtz, as we have just seen, attested the validity of the vertical-horizontal illusion with his lined squares.

There would be no profit in an attempt to list and classify all the geometrical illusions and their variants. We can get the pic-

ture of what was going on if we select a half-dozen of the best-known classical figures and see how they originated.

(1) The Poggendorff figure (Fig. 31) is the first of the illusions of direction. Mentioned to Zöllner by Poggendorff, it was described by Zöllner in 1860. Helmholtz and Hering and Wundt all discussed it without naming it, and it was not given its originator's name until 1896. In explanation of it there have been many theo-

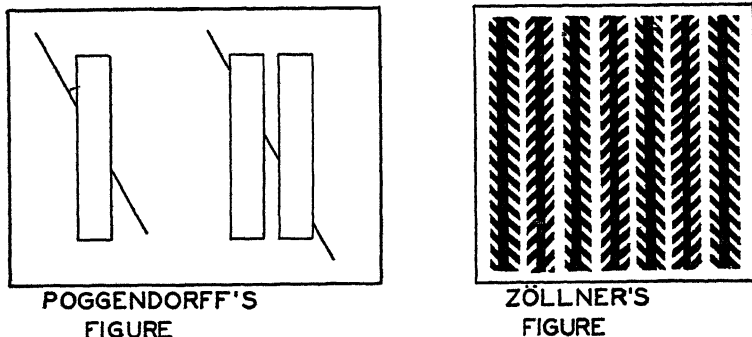


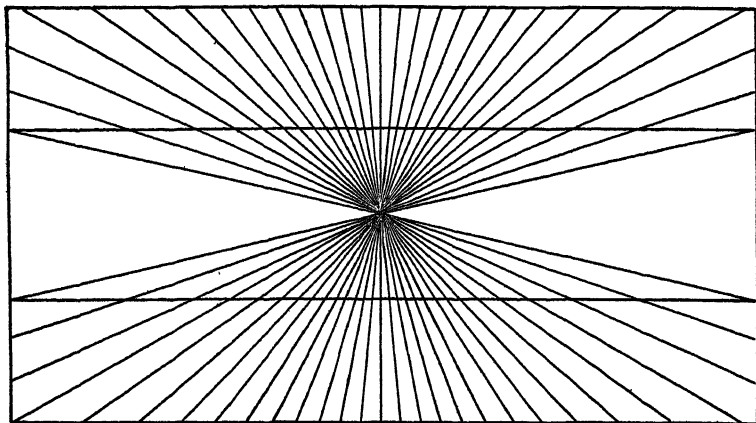
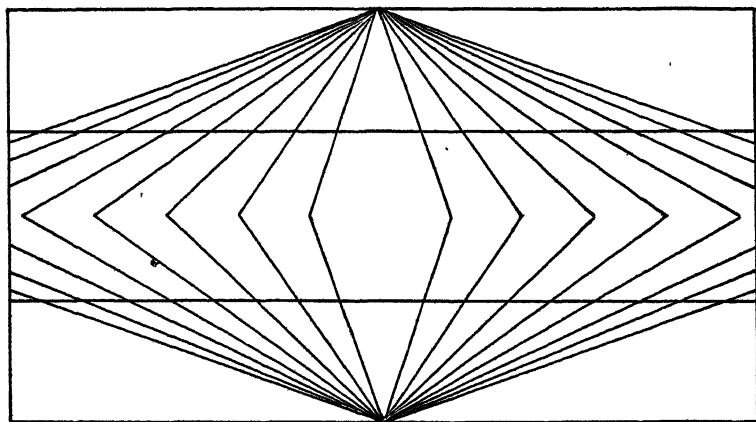
FIG. 31. POGGENDORFF'S AND ZÖLLNER'S FIGURES (1860)

The diagonal lines in the Poggendorff figure appear slightly displaced when interrupted by the vertical rectangles. Similarly the short diagonals in the Zöllner figure seem displaced by the vertical lines, so that there is an apparent shift that makes the marks look like a nonius or vernier scale. In the Zöllner figure there is also a complementary shift of the vertical lines.

ries. The illusion has been referred to eye-movements and also to a tendency to overestimate acute angles.

(2) The Zöllner figure (Fig. 31) in a sense starts off the illusions proper. It is a special abnormal case, and more striking than the Poggendorff illusion. Originally it was described as a "nonius displacement" because the little cross-pieces on either side of the line do not seem continuous, but displaced relative to each other, like the scale marks on a nonius or vernier against the edge of its measuring scale. The Zöllner illusion is a dramatic elaboration of the principle involved in the Poggendorff illusion.

(3) The Hering lines (Fig. 32) are well known because the apparent bowing of the two parallels is so striking. Hering presented this figure in 1861 along with another which shows that the idea arose as a variant of the Zöllner figure,

**HERING'S FIGURE****WUNDT'S FIGURE****FIG. 32. HERING'S (1861) AND WUNDT'S (1896) FIGURES**

The bowed parallels of Hering's figure extend the principle of Zöllner's illusion. Wundt's figure is complementary to Hering's.

(4) The Wundt lines (Fig. 32) are the companion to the Hering lines. Simply reversing the motif, they show the parallels flaring at the ends. They date much later, having been first published in 1896.

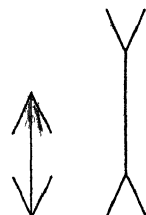
(5) The Helmholtz lined squares (Fig. 30) are fully as important historically as the preceding four figures. Helmholtz published them first in the third part of the *Physiologische Optik* in 1866. The squares introduce the principle of interruption and the principle of vertical-vs.-horizontal, whereas the preceding four illusions are all illusions of direction of the Poggendorff-Zöllner type.

(6) Perhaps the most famous geometrical illusion of all is the Müller-Lyer figure of 1889 (see Fig. 33). This is a straight illusion of extent: the middle sector looks longer with the arrow-feathers than with the arrow-heads. It has been repeated again and again in the psychological texts of the last fifty years, and Müller-Lyer himself described more than a dozen variant forms. The amount of this illusion in several of its forms was measured by Heymans in 1896. The figure has been used as an example for almost all the different theories of the geometrical illusions.

There were many classifications of the kinds of illusions, like Wundt's in 1898, but in general they all boiled down to two groups: illusions of extent and illusions of direction, to which may be added some special complicated cases. Actually it was not always possible to keep the illusions of extent and direction separate: Zöllner's illusion was classified first as a "nonius-like" displacement of the cross-lines, and later as a shift in the direction of the principal lines.

There were even more theories than classes. It is, indeed, possible to list twelve different 'theories' of the Müller-Lyer illusion for the period 1889-1902. Because this phenomenon was then the ground of considerable controversy, we may briefly examine the discussion in order to understand the character of the theorizing.

The Müller-Lyer illusion or "paradox," as it was called (Fig. 33) consists of a principal line to which wings are attached. When the wings extend the figure by making obtuse angles with the principal line, then the line is judged as longer. When the wings make acute angles, returning upon the line, then the line seems to be shorter. This is an illusion of 'assimilation': the line appears altered in length in the direction of the wings. Without



MÜLLER-LYER
ILLUSION

FIG. 33. CONFLUXION PARADOX: MÜLLER-LYER (1889)

the wings there is no illusion. How then do the wings enter in? Is the line actually altered in length by them? or does the subject judge the distance between the wings when he supposes he is judging the lines themselves?

(1) Müller-Lyer himself first offered the principle of "confluxion." A given line seems longer as the side of an obtuse angle than as the side of an acute angle. The arrow illusion is simply, he held, a multiplication of this simple effect. The principal line changes—'flows'—in the direction of the wings. This view implies a sensory shift in the actual length of the perceived line.

Other similar theories suppose the judgment to be based, not on the principal line alone, but on the fact that the wings by their presence enlarge the basis of judgment. (2) Láská, noting how one tends in imagination to close open figures by the shortest possible lines, suggested that the more open of the Müller-Lyer figures would seem the larger because closed by longer lines. (3) Delboeuf thought that the wings would attract the regard away from the ends of the lines so as to create the illusion. (4) Similarly Brunot argued that the judgment would be based upon the mean distance between the wings instead of the distance between the exact ends themselves. (5) Einthoven supposed that there might be enough dispersion of excitation at the retina to make a difference in the average lengths of the retinal images. (6) Auerbach stressed the influence of seeing the total figure in indirect vision, and (7) Schumann sought to explain this and many other illusions on the principle that one judges, not a single element, but the total impression. In a sense all these theories are theories of total impression as the determinant of judgment.

In contrast to such theories of total impression there were several dynamic theories, which supposed that the extensive judgment was mediated by eye-movement, or the tendency to eye-movement, or by a conscious act. (8) The chief exponent of the eye-movement theory was, of course, Wundt. He held that the eyes move beyond the ends of the line for the obtuse-angled wings and that they are checked before the ends by the acute-angled wings. Although experiments later verified this fact, still other experiments have shown that the illusion holds in negative after-image and in tachistoscopic exposure where there cannot be effective eye-movements. (9) Heymans' theory did not suppose actual movement, but was based on the tendencies to eye-movement that

the wings would create. (10) Lipps was the originator of the concept of empathy as explaining many esthetic phenomena. He was an act psychologist, a believer in the theory that the contents of consciousness are all acts. For him the Müller-Lyer illusion was explained by the fact that perception is an act and that the acute-angled wings limit the activity, whereas the obtuse-angled wings free it. In a sense too Schumann's theory of total impression was also dynamic, for he believed it to be due to the act of attention that adjusts itself to the total impression.

There were also two important special theories. (11) For Brentano the illusion was a special case of the general principle that acute angles tend to be overestimated and obtuse angles underestimated. Subjective changes in the angles could compress or stretch the principal line accordingly. (12) Thiéry referred the phenomenon to perspective. He saw the illusion in the third dimension, like a saw-horse. The acute-angled figure would be a saw-horse with the legs extending away from the observer, the back near and hence relatively small, whereas the obtuse-angled figure would be the saw-horse with the legs approaching the observer, with the belly far away and therefore relatively large.

Amongst all these theories there could be no decision. The problem of the geometrical illusion is not a special problem of space perception. When the general laws are known, the illusions will also be understood. This flurry in illusions in the 1890's did something more for psychology, however, than establish certain phenomena as interesting psychological curiosa. It fashioned a problem and bequeathed it to Gestalt psychology. An implicit assumption of the 1890's was the notion that the retinal image is a pretty good copy of the stimulus, and that the perception is a pretty good copy of the retinal image. Gestalt psychology changed that. It showed the correspondence to be less than had been supposed. It showed that, even when the perception comes close to matching the stimulus, it may not match the retinal image: as, for example, when perception puts back the third dimension of the stimulus that was lost in the bidimensional retinal image. It showed that the problem was not a special problem of illusions but the general problem of visual space perception, and it brought new conceptions to bear upon the solution. We shall see something of their nature in the next section.

Dynamical Gestalten

The chief contribution of Gestalt psychology to the psychology of perceived form was its insistence that the perception is formed under certain dynamical laws which give it its specific psychological organization. A perception is not a copy of its stimulus; indeed, if it does turn out to represent its stimulus fairly accurately, that result may be the consequence of the interaction of many of the organism's properties. For instance, a three-dimensional stimulus gives only a two-dimensional image on the retina, yet the organism has the capacity under certain normal circumstances to reconstitute the tridimensionality in the perception. While it remains true that the retinal image is the primary basis for the perception of visual form, nevertheless it can be shown that many of the characteristic features of perception are added by the central nervous system, some of them determined, not by the stimulus, but by the organism, its attitudes and its past experience. This change in the conception of the nature of perceived form came about gradually.

In 1890, when the interest in optical illusions was mounting, von Ehrenfels enunciated the doctrine of *Gestaltqualität* or form-quality, which we have already considered in Chapter I (pp. 16 f.). A form, like a geometrical shape or a tonal melody, is something over and above the elements (lines, tones) which combine to create it. Form is a "founded content," said Meinong, based upon the elementary "founding contents." In general, however, form-quality came to play the role of a superior element, for the psychology of the nineteenth century took elements too seriously to be lightly dispossessed of them.

A surer step in the rejection of elements came with what has sometimes in retrospect been called *experimental phenomenology*. Since elementism had done all it could for psychological discovery, some new conception was needed. To say that form is a kind of higher element did not point the way to research, and the unsuccessful attempt of Külpe's school at Würzburg to account for thought as a new non-sensory element (1901-1907) still further discredited elementism. At Göttingen and at Frankfurt, however, there came into being the method of the phenomenological description of perception without regard to its analysis into elements,

and it is these experimental studies that show the continuity from Wundtians to the Gestalt psychologists.

The first of these events was Schumann's studies of visual space perception which he published in 1900 to 1904. Formerly a pupil of G. E. Müller's at Göttingen, he was then at Berlin with Stumpf. Undertaking to examine the visual perceptions of shape, size and direction, he found their analysis into sensory elements of no help to him. He sought, therefore, to understand the phenomena by describing them, and for this purpose he appealed to the concept of attention. First he noted that attention may bind the parts of a figure into a unitary whole, so that the total impression, instead of some one of its parts, becomes effective. That is the case of the Müller-Lyer illusion (Fig. 33), where the observer judges not the line, but the total impression of the line with the wings. Contrariwise—Schumann observed—attention may abstract some essential part from the whole, to the end

that the perception takes its character from the part. Such is the case with the square of Fig. 34, which, when rotated through 45 degrees, looks larger and like a diamond. We attend, he said, primarily to the vertical and horizontal dimensions; a square with its diagonals vertical and horizontal looks different from the square that stands flat upon a side, and it also looks larger. Because Schumann thus sought the explanation of perception in the description of the immediate experience, he is entitled to rank as one of the early phenomenologists.

It was quite clear that Schumann was using the word *attention* as a dynamical principle under which perception is organized; he was making, in short, a first formulation of what was later to be said so much more clearly by the Gestalt psychologists. Look at Schumann's diagrams in Fig. 35. According as "attention" organizes them, the black squares can be perceived in an endless variety of patterns. One can see columns of squares, or rows, or diagonals, or sets of four squares, or rectangles of four-by-six squares, and so on. There are, in other words, dynamic factors which determine

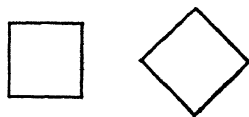


FIG. 34. SCHUMANN
SQUARE AND DIAMOND
(1900)

The square, rotated through 45 degrees, looks larger and like a different figure because, according to Schumann, attention is given to the vertical and horizontal dimensions in each case.

the nature of the perception and which depend upon the perceiving person. On the other hand, neither Schumann nor anyone else has supposed that perception is independent of the stimulus. There has to be something to perceive even though the exact form of the perception may be subjectively determined. Thus Schumann presented the unevenly spaced black circles of Fig. 35B to show that objective conditions may favor one kind of grouping over

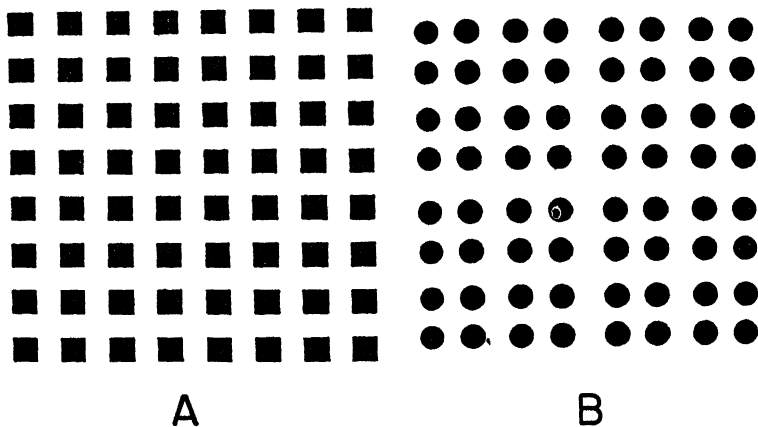


FIG. 35. SCHUMANN CHECKERBOARDS (1900)

A: the squares group themselves under the attention of the subject in various patterns: rows, columns, diagonals, groups of two by two, four by four, four by two, etc. B: the circles behave in a similar manner, but their objective grouping predisposes toward certain patterns and makes other patterns difficult to form.

another. Some of the groupings that are possible with the squares cannot be easily formed with the circles. In the same way the difference between the 'square' and the 'diamond' of Fig. 34 depends initially upon stimulus-position, but secondarily upon the way in which the subject's attention is related to the stimulus. The subject is quite capable of seeing the 'diamond' as a 'twisted square,' or vice versa.

It is possible now to see how the phenomenological method was being introduced into psychology during the first part of the present century. The philosopher Husserl, having taken the word *phenomenology* from Kant and Hegel, applied it to the study of immediate experience or "pure being" (1901). Stumpf at Berlin (1906) took over the term for the study of all experience, without

systematic bias, in and of itself. Schumann's study of perception at Berlin (1901-1904) had been, as we have noted, essentially a phenomenological study. Next we find the method and the word turning up at Göttingen in G. E. Müller's laboratory, with Jaensch, Katz and Rubin.

E. R. Jaensch in 1909 published from Göttingen a monograph dealing with the Aubert-Förster law, the principle that visual acuity is greater for near objects than for far. This is the sort of fact that leads to the consideration of dynamical interaction within total systems, for it shows spatial discrimination to be dependent upon other aspects of the perceptual situation than the mere separation of stimulated retinal points. In 1911 Jaensch put out another monograph in which he dealt with the visual perception of depth, the phenomenology—he used the word—of empty space and the nature of its psychic representation, and certain problems of perceived size where size varies independently of the size of the retinal image. That Jaensch was using the phenomenological method and dealing with the sort of perceptual problem that later was to be the primary concern of the Gestalt psychologists is evident.

It was in 1911 too that Katz brought out his study of the modes of appearance of colors. Although not primarily a study in space perception, its effect was to show that the problems of quality and space cannot be separated, and the method was a perfect example of phenomenology. That visual experience was necessarily limited by the properties of the retinal excitation had been an assumption of the older psychology. It assumed, for instance, that monocular experience is bidimensional because the retina is bidimensional, and that acuity and color are functions of the nature of the action of the retinal elements. As a phenomenologist Katz, on the contrary, could forget about the retina and describe experience in its own right. He found three kinds of colors: (1) the surface colors (*Oberflächenfarben*), which are bidimensional and localized at a given distance and which are in general the colors of perceived objects; (2) volumic colors (*durchsichtige, räumhafte Farben*), which are the tridimensional colors of transparent media, like colored liquids, colored air or a completely lightless space; and (3) film colors (*Flächenfarben*), which are primary and without localization or precise spatial characteristics, like the color in a spectroscope. Katz invented the "reduction screen," a

screen with a hole in it. Viewed through the hole a colored surface is seen as a film color instead of a surface color, because the screen eliminates perception of everything (position, relation to surroundings, general illumination) that provides the context for the establishment of the spatial characteristics of the color. By elimination of this context a surface color is "reduced" to a film. Although the plea for phenomenology is essentially negative in that it demands freedom from convention in the formulation of descriptions of events, nevertheless it has been justified by its positive results; and Katz's monograph was one of the first to show clearly how the conventional rubrics of psychology had been limiting discovery.

In 1915 Rubin published from Müller's laboratory—he had begun the research in 1912—a study of figure and ground in visual perception. He found that a visual perception normally divides into two fields: a figure, which is usually the object of attention, and a ground which occupies the rest of the perception. This distinction becomes especially clear in such ambiguous fields as are shown in Fig. 36, where a black figure can be seen upon a white ground, or a white figure upon a black ground. Figure and ground differ, according to Rubin, phenomenologically. The figure, which is seen as a whole, looks like a thing or an object (*Dingcharakter*), whereas the ground appears not as a thing but as a substance (*Stoffcharakter*). The figure appears nearer than the ground, which usually seems to continue behind the figure, thus giving the impression that the bounding contour belongs to the limited figure and not to the continuous ground. Moreover, the figure acts as a whole functionally, and, being an object, tends toward constancy. A shadow on the figure darkens the figure less than a shadow on the ground darkens the ground. In an ambiguous pattern (Fig. 36) what is first seen as figure tends to reappear as figure if the pattern is seen again at a later time. If at a later time figure and ground do become reversed, then the pattern may not be recognized. Although Rubin published after Wertheimer, the fact that he did this work at Göttingen without knowledge of Wertheimer's study on seen movement shows that the new experimental phenomenology, dealing in the dynamics of perception, was in the air before Gestalt psychology was established.

Wertheimer's study of apparent movement was published in 1912 from Schumann's laboratory at Frankfurt. That it is regarded

as the beginning of the movement of Gestalt psychology is partly due to the fact that it involved all three of the prime movers of the new school, for Köhler and Koffka were Wertheimer's chief observers in the investigation. It is also true that Wertheimer from the first saw in the new phenomenology a general reorientation of psychology, whereas Schumann, Jaensch, Katz and Rubin were

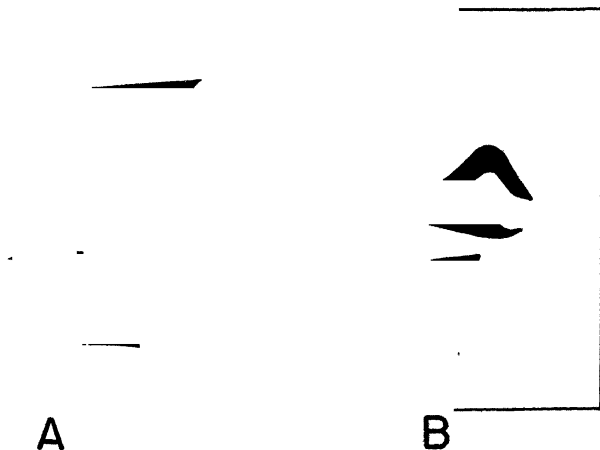


FIG. 36. RUBIN'S AMBIGUOUS FIGURES (1915): VASE-PROFILES AND CLAW-FINGERS

In each pattern figure and ground can be reversed. In A one can see a white vase on a black ground or black profiles on a white ground. In B one can see a black claw on a white ground or white fingers on a black ground.

thinking within the narrower confines of particular subject-matters. Wertheimer's achievement was to describe movement in phenomenological terms, exhibiting it as a dynamic event in a perceptual field. He showed that discrete displacement of stimulation upon the retina is seen, if slow enough, as the successive appearance of two displaced objects, or, if fast enough, as their simultaneous appearance. Between these extremes, however, there is an intermediate speed of displacement which gives rise to the optimal perception of movement, which may be indistinguishable from the real movement in which the stimulation is continuously displaced. Wertheimer thus succeeded in getting away from the notion that perceived movement is merely the change of a sensation in space with time. He realized that movement is itself an

observable phenomenon, which may occur with actual movement of the stimulus and which depends upon such dynamical relationships as the speed of displacement of the stimulation. (For a full discussion of the nature and significance of Wertheimer's paper, see pp. 595-599.)

Gestalt Psychology and the Laws of Form

As an aggressive school Gestalt psychology dates from about 1920, when Köhler published his *Die physischen Gestalten in Ruhe und im stationären Zustand*. The next year he was called to Stumpf's chair at Berlin, and the *Psychologische Forschung*, the journal of the Gestalt school, was begun. *Die physischen Gestalten* is a book that provides a methodological foundation for Gestalt psychology in that it treats of the general dynamics of the formation of form. Köhler dealt there with the development, change and stabilization of differentiation within physical systems. He worked out the principles, showing how the dynamical laws hold only for whole systems and not separately for the parts. Since he also showed that the dynamics of physical systems hold in many instances for perceptual fields, he could thus undertake to apply physics to psychology by way of his principle of isomorphism, a principle which asserts that the perceptual field corresponds to the brain field in such a way that the physical dynamics apparent in the perception might actually be operative in the brain. (Cf. pp. 83-90.)

With such a foundation it was quite natural that Gestalt psychology should start out with perception as its chief field of interest, and that visual form should be the principal kind of perception studied. For more than a decade, in fact, the systematic discussions of Köhler, Koffka and Wertheimer used predominantly visual patterns as *experimenta crucis* to prove their points. Köhler said, in fact, that Gestalt and form are almost synonymous. Exactly what then is it, we may ask, that Gestalt psychology has contributed to the psychology of visual form?

A precise answer to this question would involve the evaluation of this new movement, when unfortunately there is as yet no sufficient precipitation of scientific fact from partisan opinion to establish such a conclusion. There are many 'laws' of Gestalten which in their formulations still reflect to some extent the personal

predilections of their authors. One cannot at present name six, or thirty, sure principles that are ready to be taken over by scientific psychology and accepted as truth with the names of their originators forgotten. Nevertheless one can pick out a fairly complete list of the more important principles of Gestalt, those that apply especially to visual form; and to that undertaking we may now address ourselves.

There is a huge literature. From it in 1933 Helson extracted 114 laws of Gestalten. All but half a dozen of these laws are applicable to visual form; but a great many of them, devoted to showing that a Gestalt differs from a congeries of parts, may be ignored here. We can take it for granted that a visual form is a unitary whole. As many of the other laws are related or overlap, we can, by elimination of the less important and combination of the similar, come out with fourteen that represent the major contribution of Gestalt psychology to the knowledge of this subject matter. Most of the principles are derived from the formulations of Wertheimer, Koffka, Köhler, Rubin and Sander. Most of the principles really specify dimensions in respect of which forms vary.

1. *Naturalness of form.* A field tends to become organized and to take on form. Groups tend to form structures, and disconnected units to become connected.

2. *Figure and ground.* A form tends to be a figure set upon a ground, and the figure-ground dichotomy is fundamental to all perception. The simplest form is a figure of undifferentiated quality set upon a ground.

3. *Articulation.* Forms vary from simple to complex in the degree of articulation or differentiation that they possess.

4. *Good and poor forms.* A good form is well articulated and as such tends to impress itself upon the observer, to persist and to recur. A circle is a good form.

5. *Strong and weak forms.* A strong form coheres and resists disintegration by analysis into parts or by fusion with another form.

6. *Open and closed forms.* An open form tends to change toward a certain good form. When a form has assumed stable equilibrium, it has achieved closure. Thus a nearly circular series of dots may achieve closure by being perceived as a circle.

7. *Dynamic basis of form.* A form is a dynamic system or is

based upon a dynamic system. Since the dynamic principles operate within the organism, a strong form is that which depends more upon the dynamic properties of the organism than upon the properties of the stimulus. The fact that the organism operates to structure the perception means that there need be no correspondence between the form of the stimulus and the form of the perception.

8. *Persistence of form.* A form once perceived tends to persist, and to recur when the stimulus situation recurs. The recurrence of part of a previously perceived form tends to reinstate the whole.

9. *Constancy of form.* A form tends to preserve its proper shape, size and color. This is the well-known "constancy phenomenon."

10. *Symmetry of form.* A form tends toward symmetry, balance and proportion. Many of the geometrical 'illusions' illustrate this principle.

11. *Integration of similars and adjacents.* Units similar in size, shape and color tend to combine to make better articulated forms. Near units also combine more readily than far. Fig. 35B illustrates the effect of adjacency. The principle of the integration of similars would appear in Fig. 35A if certain of the squares were one color and the other squares another color. It would, for example, be easy to create there the form of a cross by coloring the proper squares green and the others red.

12. *Meaningfulness of forms.* A form tends to be meaningful and to have objectivity. The more meaningful the form, the stronger it is, the more easily it is perceived, and the longer it tends to persist.

13. *Fusion of forms.* Two forms can fuse, giving rise to a new form; or, in combination, the stronger one may persist, eliminating the weaker. Simple, poorly-articulated forms fuse more easily than complex, good forms. A more meaningful form tends to predominate over a less meaningful one.

14. *Transposition of form.* A form exists independently of its constituent elements and may thus be transposed without change to other elements. This is von Ehrenfels' original law that a melody is independent of the particular notes that sound in it, and that a shape is independent of the quality and size of the lines that constitute it. The law is of wide application. It holds in sensory discrimination where human subjects and animals learn easily to

choose the larger, or the brighter, of two objects without regard to absolute size or absolute brightness.

Of these principles the most important might be said to be the laws of transposition and of constancy, partly because they are not obvious to common sense, but mostly because they are general principles, indicative of the organism's broad capacities and limitations in dealing with its environment.

The law of *transposition* is inherent in the conception that form itself is independent of the nature of its content. Mach's notions of space-form and time-form mean that something exists over and above the elements formed; it was such a form that von Ehrenfels called a *Gestaltqualität*. Transposition expresses a principle of relativity: it is the relations that persist independently of the contents in the *Gestaltqualität*, as Meinong pointed out. Thus one may expect to find transposability wherever relationships are more important than contents—a situation which has always been known to exist in sensory discriminations of psychophysics. Of two sensations the observer can say *greater*, *equal* or *less* with assurance when quite uncertain of the absolute intensity of either. While Wundt and others of the old school recognized this principle of the relativity of judgment, it was left for Gestalt psychology to identify it with the law of transposition. Köhler in 1917–1918 showed that children, apes and chickens transpose a relation of size or brightness from one pair of stimuli to another, so that the subject, trained to choose the brighter of two grays or the larger of two objects, may reject the stimulus originally preferred when it is paired with another still brighter or larger. In the years since 1918 there have been many extensions and confirmations of the principle that the form or relation is the important datum in a discrimination.

The law of *constancy* is also not new. Many psychologists have noted that persons see objects as they know them to be and not as the retinal image represents them. Helmholtz (1860–1866) explained the constancy of objects in perception by the principle (later rejected) of unconscious inference. Hering (1905) described color constancy under the term *memory color*, and Katz (1911) showed how memory color gives stability to the surface colors of objects. We have already seen how this difference led to the solution of the problem of brightness and intensity in the series

of grays (pp. 135 f.). In the 1920's the rule came gradually to be recognized as a fundamental of Gestalt psychology. Later Thouless (1931) conducted experiments on the constancy of size when the distance between the stimulus and the observer varies, of shape when the angle between the stimulus and the observer varies, and of brightness when the illumination varies. Since the phenomenon is not really one of complete constancy but only of a tendency toward constancy, he called it "regression to the real object," meaning regression *toward* the real object since the regression is seldom complete; and he invented a formula for measuring the amount of regression. There was an extensive quantitative study of "constancy" of size and shape by Brunswik and his associates in 1933. Brunswik made it clear that, since it serves to stabilize the ever-changing perceptual world, object-constancy has biological value for the organism. In the next chapter we return to the matter of constancy of size with varying distance (pp. 288-299).

It should be noted here that the principles of transposition and constancy are so closely related as to be often indistinguishable. The law of transposition holds when relations remain constant with change of content. The degree of the constancy phenomenon can be demonstrated, however, only by a comparison with a standard, that is to say, by the establishment of a relation. Thus both transposition and the constancy phenomenon depend upon the constancy of relations when other data change. For instance, the piece of white paper still looks white in the shadow because its relation to the field is unchanged; but, when a reduction screen eliminates the field, the relation is no longer perceived and the paper is no longer white.

While it is true that the Gestalt laws of form are some of them obvious, and most of them old, and a few of them accepted by all psychologists—a paradox that old principles should still not win general acceptance!—we must nevertheless not lose sight of the fact that the accepted theory of the fundamental nature of perceived form has been greatly altered in the last fifty years. In that time we have passed from the discussion of patterns of elements to the formulation of the dynamic structure of perceptual fields, a radical change in emphasis which has been wrought entirely by Gestalt psychology.

Notes

Retinal Image

G. B. Porta wrote (1540-1615) *Magiae naturalis* in four short books in 1558, but his discussion of the principle of what we call the *camera obscura* (projection through a small hole into a dark chamber) and his pointing out the analogy to the eye is in the second edition, enlarged into twenty short books, 1591, Bk. 17, which is *De catoptrici imaginibus*; or Eng. trans., *Natural Magic*, 1658, Bk. 17, Of strange glasses. In the meantime F. Maurolycus (1494-1575) had published *Photisme de lumine*, 1575 (see esp. Bk. 3 of the 1611 ed., 69-80, on the eye), Eng. trans., 1940, esp. 105-121, in which he recognized the crystalline humor as a lens, but regarded it still as the seat of visual sensation and not as forming an image on the retina. He thought the function of the retina was nutritive.

Johann Kepler's book is *Ad vitellionem paralipomena*, 1604, and the chief reference to this matter is Chap. 5, *De modo visionis*, 158-221. For Kepler's proof that an image in a *camera obscura* would be the shape of the object imaged and not of the hole, see p. 50. The argument about the mechanism of vision is extended in Kepler's second book, *Dioptrice*, 1611. R. Descartes's discussion of the optics and anatomy of vision is in his *La dioptrique*, 1637, Chaps. 3, 5, 6 and 7; see esp. Chap. 5, entitled *Des images qui se forment sur le fond de l'oeil*. See also his *L'homme*, 1662 (*De homine*, 1664), the latter half of Pt. 3. The story that C. Scheiner in 1625 produced visible images with a bull's eye before Descartes performed the experiment is attributed to G. Schott, *Magia universalis*, 1657, Pt. 1, 87, [n.v.], by J. Priestley, *History of Discoveries Relating to Vision, Light and Colours*,

1772, 112; cf. E. Mach, *Principles of Physical Optics*, trans. 1925, 46.

William Molyneux, the friend of John Locke, wrote *A Treatise of Dioptricks*, 1692, in which see Props. 34, 40, 42, etc., for the identification of the "optick angle" with "the apparent magnitude," and pp. 105 f. for the problem of the retinal image. George Berkeley, *An Essay towards a New Theory of Vision*, 1709, considered the problem of magnitude in sects. 52-87 and the problem of the inverted retinal image in sects. 88-121. Johannes Müller discussed the retinal image in *Handbuch der Physiologie des Menschen*, 1838, II, Bk. v, sect. i, Chap. 3, Pt. 1.

Singleness of Vision and the Horopter

On the anatomical explanation of the singleness of binocular vision in terms of the semi-decussation of the optic fibers at the chiasma, see Claudius Galen (A.D. 129-199), *De usu partium libri XVII*, X, Chap. 12; I. Newton, *Opticks*, 1704, query 15; D. Hartley, *Observations on Man*, 1749, I, prop. 59; W. H. Wollaston, On the semi-decussation of the optic nerves, *Phil. Trans.*, 1824, I, 222-231; J. Müller, *Beiträge zur vergleichenden Physiologie des Gesichtssinnes*, 1826, 71-164. On the other theories, see the general discussion by H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, III, 1866, end of sect. 31 (in the Eng. trans., III, pp. 482 f.). Helmholtz cites F. J. Gall as well as Porta as holding the theory of alternating vision. An excellent earlier summary of theories is that of Müller, *Handbuch der Physiologie des Menschen*, 1838, II, Bk. v., sect. i, Chap. 3, Pt. 4.

On the horopter, see F. Aguilonius, *Opticorum libri sex*, 1613, II, 146-150; G. U. A. Vieth, Ueber die Richt-

ung der Augen, *Ann. Phys. Leipzig*, 58, 1818, 233-255, esp. 235-238; Müller, *Gesichtssinn*, 1826, *loc. cit.* and Plate I; also Müller, *Handbuch*, *loc. cit.* For an historical note, see Helmholtz, *loc. cit.* For the general form of the horopter, see Helmholtz, *op. cit.*, III, sect. 31 (in the Eng. trans., pp. 421-482); E. Hering, *Beiträge zur Physiologie*, 1861-1864, most of which is about identical points and the horopter. Helmholtz, Eng. trans., III, 484, has a long note on how Hering and he independently worked out the form of the horopter. Fig. 28 is plotted from Helmholtz's graph of the curve developed. L. T. Troland's similar figure, *Principles of Psychophysics*, 1929, I, 343, is wrong.

In general, see H. A. Carr, *An Introduction to Space Perception*, 1935, 159-194.

Eye-Movement

Problems of eye-movement are the central theme of Müller's monograph, *op. cit.*, 1826.

For the principle of Donders' law, see F. C. Donders, *De bewegingen van het menschelijk oog*, *Ned. Lancet*, 2 ser., 2, 1846, 104-138; *Beitrag zur Lehre von den Bewegungen des menschlichen Auges*, *Holland. Beitr. anat. physiol. Wiss.*, 1, 1846, 105-145.

For J. B. Listing's law, see the reference that his contemporaries always gave for it: C. G. T. Reute, *Lehrbuch der Ophthalmologie*, 2 ed., 1853, 37. The statement sometimes cited in 1 ed., 1847, 14, is not explicit, whereas the 1853 reference calls the principle Listing's law but gives no reference to Listing. Helmholtz says, *Physiological Optics*, Eng. trans., III, 1925, 121, that Listing never published the law; so that must be why Reute was always cited for it. Listing's law should hold only for the primary position, which may be defined as the position of the eyes with

the head normally erect and the lines of regard of the two eyes parallel and in a horizontal plane. More strictly speaking the primary position is defined as the position for which Listing's law holds, i.e., the position from which the eye moves without torsion. If the eye is in the primary position when it fixates a cross, then the projected negative after-image of the cross will not rotate when the eye is moved.

For Helmholtz's discussion of these two laws, see his *Handbuch der physiologischen Optik*, III, 1866, sect. 27 (also 3 ed. and Eng. trans.). For limitations of the laws and deviations from them, see J. von Kries' note to this sect. 27 in the 3 ed. (and Eng. trans.). For Hering's discussion of Listing's law, see his *Beiträge zur Physiologie*, 1864, esp. 248-286. For a general discussion of the laws and the experimental means of their verification, see E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 246-252.

On Lotze and local signs, see the next section.

Nativism and Empiricism

For this 19th-century topic there is a large and unfruitful literature. Excellent summaries of it are: T. Ribot, *German Psychology of To-Day*, trans. 1886, 68-133; O. Külpe, *Outlines of Psychology*, trans. 1895, 343-351, 368-373; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 162-214; W. Wundt, *Grundzüge der physiologischen Psychologie*, 6 ed., II, 1910, 517-529, 702-736; J. v. Kries, in Helmholtz's *Physiological Optics*, 3 ed., III, trans. 1925, 607-625, 635-652. The last reference is the fullest and most sophisticated. These discussions introduce more theorists than does the present text; e.g., J. F. Herbart, T. Lipps and J. Ward.

The history begins in experimental

psychology with Johannes Müller, *Zur vergleichenden Physiologie des Gesichtssinnes*, 1826, 39–67, which is nativistic in the sense that it is Müller's first introduction of the concept of specific energies and that he insists that the mind can perceive only the retinal image and not the object. For Müller's use of the views of the philosophers (including Kant) in a theory of space, see his *Handbuch der Physiologie des Menschen*, 1838, Bk. 6, sect. 1, Chap. 2 (Vol. 2, pp. 1346–1351 of the Eng. trans.).

E. H. Weber influenced Lotze. Weber, in his *Der Tastsinn und das Gemeingefühl*, R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 524–543, discussed cutaneous spatial perception—his famous experiments on the discrimination of two points—under the heading of the "Ortsinn," and sought to explain discrimination by a theory of sensory circles (pp. 527–529), the notion that the nerve-fibers, leading to different spots on the skin, make of the skin a mosaic of sensitive regions, and that spatial discrimination occurs only when different "circles," and therefore different fibers, are stimulated. Cf. pp. 475–477.

R. Lotze's theory of local signs was essentially a theory which endowed each sensory circle with a conscious localizing characteristic and applied the principle to the retina as well as to the eye. See his *Medizinische Psychologie*, 1852, 325–371; and, for a summary, Ribot, *op. cit.*, 68–95.

Although the British empiricists naturally provided a background for the empiricist theories of spatial perception, they do not come directly into this controversy. For an example of the way in which they sought to derive the conception of space from the perception of time and motion, and of the discussion as to whether extension must be given intuitively in the first place if space is presently to be built up, see J. S. Mill, *An Examination of Sir William*

Hamilton's Philosophy, 1865 or later ed., Chap. 13.

Helmholtz discussed these theories in relation to monocular space perception in his *Physiological Optics*, III, [1866], trans. 1925, sect. 28, esp. pp. 154–168, 228–232. For Helmholtz's contention that the geometrical axioms are not intuitive and innate but learned in experience, see the three articles of 1866, 1868 and 1878, reprinted in his *Wissenschaftliche Abhandlungen*, II, 1883, 610–660.

Wundt's final summary of his theory of space perception (1910) has been cited above. The theory developed through all six editions of the *Physiologische Psychologie*. One gets its beginnings in his *Vorlesungen über die Menschen- und Tierseele*, 1863, I, Lect. 15–17; with which cf. 2 ed., 1892, Lects. 9, 10 (or Eng. trans. of 2 ed., 1894).

E. Hering's nativistic slant appears in his *Beiträge zur Physiologie* (the only part ever published has the subtitle: *Zur Lehre vom Ortsinne der Netzhaut*), 1861–64. In this his general discussion of theory occurs on pp. 323–329 (1864).

The other important references for the men cited in the text are as follows. C. Stumpf, *Ueber den psychologischen Ursprung der Raumvorstellung*, 1873, an important book which helped get its author the chair at Würzburg; W. James, *The Principles of Psychology*, 1890, II, 145–176; O. Külpe, *Grundriss der Psychologie*, 1893, sects. 57, 61 (or Eng. trans.); E. B. Titchener, *A Text-Book of Psychology*, 1910, 303–306, 335–338.

G. M. Stratton's experiment on the inversion of the visual field was first reported in 1896 to the III International Congress of Psychology at Munich, but the main paper is *Vision without inversion of the retinal image*, *Psychol. Rev.*, 4, 1897, 341–360, 463–481. The modern repetition of the experiment is P. H. Ewert,

A study of the effect of inverted retinal stimulation upon spatially co-ordinated behavior. *Genet. Psychol. Monogr.*, 7, 1930, 177-363.

On adjustment to a visual field distorted by prismatic wedges, see M. Wooster, Certain factors in the development of new spatial coordination, *Psychol. Monogr.*, 32, 1923, no. 4; J. J. Gibson, Adaptation, after-effect and contrast in the perception of curved lines, *J. exp. Psychol.*, 16, 1933, 1-31. W. Wundt mentioned the straightening (in a few days or hours) of straight lines perceived as crooked because of experimentally induced optical changes or because, in metamorphopsia, of the actual shifting of the retinal elements: *Grundzüge der physiologischen Psychologie*, 5 ed., II, 1902, 512-514. He described his own case of metamorphopsia which arose out of his choroiditis.

There is a related experiment on reversed auditory localization: P. T. Young, Auditory localization with acoustical transposition of the ears, *J. exp. Psychol.*, 11, 1928, 399-429.

Geometrical Optical Illusions

The best systematic secondary sources are E. C. Sanford, *A Course in Experimental Psychology*, 1898, 212-260; E. B. Titchener, *Experimental Psychology*, I, 1901, pt. i, 151-170, pt. ii, 303-328; W. Wundt, Die geometrisch-optischen Täuschungen, *Abh. sächs. Ges. Wiss., math.-phys. Cl.*, 24, 1898, 53-178 (also separate). Wundt, of course, gives his own theory; Sanford and Titchener discuss others.

For R. Lotze's early discussion of the sensory illusions, see his *Medizinische Psychologie*, 1852, 435-452.

J. J. Oppel's pioneer articles are Ueber geometrisch-optische Täuschungen, *Iber. phys. Ver. Frankfurt*, 1854-1855, 37-47, [n.v.]; Nachlese zu den geometrisch-optischen Täuschungen, *ibid.*, 1856-1857, 47-55; 1860-1861, 26-37, [n.v.]. The other early discussions and measurements of the effect of interruption of extent are E. Hering, *Beiträge zur Physiologie*, I, 1861, 66-69; A. Kundt, Untersuchungen über Augenmaass und optische Täuschungen, *Ann. Phys. Chem.*, 196, 1863, 118-158; H. Aubert, *Physiologie der Netzhaut*, 1865, 264-267; H. Helmholtz, *Handbuch der physiologischen Optik*, III, 1866, 159 f. [Eng. trans., III, 192 f.].

W. Wundt's first mention of the overestimation of the vertical with respect to the horizontal is in his *Beiträge zur Theorie der Sinneswahrnehmung*, [1859] 1862, 158-163. Helmholtz discussed this matter in *loc. cit.*

The early references to the six classical illusions listed in the text are as follows:

(1) J. C. Poggendorff called Zollner's attention to the illusion that bears his name, so see Zollner, *op. cit. infra*. E. Burmester, Beitrag zur experimentellen Bestimmung geometrisch-optischer Täuschungen, *Z. Psychol.*, 12, 1896, 355-394, esp. 356, named the illusion for Poggendorff. See also A. H. Pierce, *Studies in Auditory and Visual Space Perception*, 1901, 242-278.

(2) F. Zollner's figure in his Ueber eine neue Art von Pseudoskopie und ihre Beziehungen zu den von Plateau und Oppel beschriebenen Bewegungsphänomenen, *Ann. Phys. Chem.*, 186, 1860, 500-525. The original illusion has the thick lines, as shown in Fig. 31, which has the advantage of being double: both white on black and black on white. It is generally drawn nowadays with thin lines, black on white. See also Pierce, *op. cit.*, 279-314.

(3) E. Hering first published the bowed parallels in his *Beiträge zur Physiologie*, I, 1861, 74.

(4) Wundt's analogous figure of the flaring parallels was based on an older figure of Pisco's and was used

by Wundt in his lectures. It was first published by A. Thiéry, Ueber geometrisch-optische Täuschungen, *Phil. Stud.*, 12, 1896, 74, and then Wundt published it himself in his Die geometrisch-optische Täuschungen, *op. cit.*, 1898, 117.

(5) For Helmholtz's squares, see *loc. cit.*

(6) For F. C. Müller-Lyer's famous arrows, see his Optische Urtheilstäuschungen, *Arch. Physiol.*, Suppl. Bd., 1889, 263-270, which, though brief, figures fifteen variant forms of the illusion and offers the principle of "confluxion" in explanation. Müller-Lyer's fuller account is his Ueber Kontrast und Konfluxion, *Z. Psychol.*, 9, 1896, 1-16; 10, 1896, 421-431. The illusion was analyzed quantitatively by G. Heymans, Quantitative Untersuchungen über das "optische Paradoxon," *ibid.*, 9, 1896, 221-255.

An excellent discussion of these various theories of the Müller-Lyer "paradox" is given in E. B. Titchener, *Experimental Psychology*, I, 1901, Pt. II, 321-328. The original sources in chronological order are: Müller-Lyer, *op. cit.*, 1889, esp. 266 f.; *loc. cit.*, 1896; W. Łaska, Ueber einige optische Urtheilstäuschungen, *Arch. Physiol.*, 1890, 326-328; F. Brentano, Ueber ein optisches Paradoxon, *Z. Psychol.*, 3, 1892, 349-358; 5, 1893, 61-82; T. Lipps, Optische Streitfragen, *ibid.*, 3, 1892, 493-504, esp. 498-504; *Raumästhetik und geometrisch-optische Täuschungen*, 1897, 234-256; J. Delboeuf, Sur une nouvelle illusion d'optique, *Rev. sci. Paris*, 3 ser., 51, 1893, 237-241; C. Brunot, Les illusions d'optique, *ibid.*, 52, 1893, 210-212; F. Auerbach, Erklärung der Brentano'schen optischen Täuschung, *Z. Psychol.*, 7, 1894, 152-160; G. Heymans, *op. cit.*, 1896, esp. 248-252; A. Thiéry, Ueber geometrisch-optische Täuschungen, *Phil. Stud.*, 12, 1896, 67-125, esp. 67-102; W. Wundt, Die geometrisch-optischen Täuschungen, *op. cit.*,

1898, 88-105; W. Einthoven, Eine einfache physiologische Erklärung für verschiedene geometrisch-optische Täuschungen, *Arch. ges. Physiol.*, 71, 1898, 1-43, esp. 2 f.; F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen, *Z. Psychol.*, 30, 1902, 241-291, esp. 286-290.

Dynamical Gestalten

For further orientation on the conflict between elementism and phenomenology, see E. G. Boring, *A History of Experimental Psychology*, 1929; on elementism, 324, 328, 377-379, 423 f., 572, 575-578; on phenomenology, 357-359, 367 f., 401, 572 f., 575 f., 579.

On the school of Gestaltqualität, see Boring, *op. cit.*, 433-440, 448-450. The important papers are cited in the notes of Chap. I (p. 47).

The early work in experimental phenomenology is: F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen, *Z. Psychol.*, 23, 1900, 1-32; 24, 1900, 1-33; 30, 1902, 241-291, 321-339; 36, 1904, 161-185; E. R. Jaensch, Zur Analyse der Gesichtswahrnehmungen, *Z. Psychol.*, Ergbd. 4, 1909; Ueber die Wahrnehmung des Raumes, *ibid.*, Ergbd. 6, 1911; D. Katz, Die Erscheinungsweise der Farben und ihre Beeinflussung durch die individuelle Erfahrung, *ibid.*, Ergbd. 7, 1911; E. Rubin, *Synsoplevede Figurer*, 1915, German trans. as *Visuell wahrgenommene Figuren*, 1921.

Gestalt Psychology

Köhler's difficult prolegomenon is W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920. It has never been translated, but a little of it has been abstracted in English by G. W. Hartmann, *Gestalt Psychology*, 1935, 31-50, and W. D. Ellis, *A Source Book of Gestalt Psychology*, 1938, 17-54. The only

complete systematic treatise that the Gestalt psychologists have published is K. Koffka, *Principles of Gestalt Psychology*, 1935.

The secondary sources are numerous. The best of these are Hartmann, *op. cit.*, and W. D. Ellis, *op. cit.*, where thirty-four of the important German documents have been abstracted in English. A splendid French summary is P. Guillaume, *La psychologie de la forme*, 1937. See also Boring, *A History of Experimental Psychology*, 1929, 571-580, 591-593.

The text has based its summary upon Helson's 114 laws: H. Helson, The fundamental propositions of Gestalt psychology, *Psychol. Rev.*, 40, 1933, 13-32, q.v. for 24 basic references by 18 authors.

On transposition, see W. Köhler, Die Farbe der Sehdinge beim Schimpansen und beim Haushuhn, *Z. Psychol.*, 77, 1917, 248-255; Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. preuss. Akad. Wiss., phys.-math. Kl.*, 1918, 1-101; and the abstract of the latter in W. D. Ellis, *op. cit.*, 217-227.

On the constancy phenomenon, see H. Helmholtz, *Handbuch der physiologischen Optik*, sect. 24 (1860) on contrast, sect. 26 (1866) on nature of object-perception (see esp. Eng. trans., III, 287, which is in sect. 24, for a statement of the

constancy principle); E. Hering, *Grundzüge der Lehre vom Lichtsinn*, [1905] 1920, 1f., 6-12, on objects and memory colors; D. Katz, Die Erscheinungsweisen der Farben, *Z. Psychol., Ergbd.* 7, 1911, 214-223; R. H. Thouless, Phenomenal regression to the real object, *Brit. J. Psychol.*, 21, 1931, 339-359; 22, 1931, 1-30; E. Brunswik (with B. E. Holaday, K. Eissler and S. Klimpfinger), Untersuchungen über Wahrnehmungsgegenstände, *Arch. ges. Psychol.*, 88, 1933, 377-628. The best discussion of the literature of the constancy phenomenon is K. Koffka, *Principles of Gestalt Psychology*, 1935, 211-264. The history of brightness constancy is dealt with briefly by R. B. MacLeod, An experimental investigation of brightness constancy, *Arch. Psychol. N. Y.*, 1932, No. 135, 5-18; M. A. Sheehan, A study of individual consistency in phenomenal constancy, *ibid.*, 1938, No. 222, 14-19. Brunswik's views are described by H. Ansbacher, Perception of number as affected by the monetary value of the objects, *ibid.*, 1937, No. 215, 5-25.

Recently the problem of visual shape, and thus of the optical illusions, has been brought into relation to the field theory of Gestalt psychology: W. D. Orbison, Shape as a function of the vector-field, *Amer. J. Psychol.*, 52, 1939, 31-45, 309.

Chapter 8

VISUAL PERCEPTION OF DEPTH AND DISTANCE

BERKELEY, having said in 1709 that "the estimate we make of the distance of objects *considerably remote* is rather an act of judgment grounded on experience than on sense," went on to argue that the distance of near objects may be perceived by the sensations that arise when the eyes, in looking at a near object, are brought nearer together (convergence), or when an approaching object is kept from being confused by the effort or straining of the eyes. Thus Berkeley represented the fundamental view that prevailed for two centuries after him. It was the notion that the convergence and accommodation of the eyes furnish primary sensory data, basic to the immediate perception of distance, and that the devices which the painter uses, like perspective and shadow and interposition—all of which work for one eye and at great distances—are only secondary judgmental criteria which a person learns by experience to employ in his estimate of distance. Because representative drawing and painting are older than psychophysiology, these 'secondary criteria' gained attention before the 'primary' mechanisms. By the nineteenth century, however, convergence and accommodation had come to be regarded as the two important factors, to which Wheatstone presently added a third when with the stereoscope he brought retinal disparity into prominence as another fundamental means of perceiving depths. In the present century the pendulum has started to swing back; the classical 'secondary criteria' are now being regarded as the primary dynamic mechanisms in visual perception with convergence and accommodation as auxiliary processes. In this chapter we shall consider all these principles and mechanisms in the order in which they became important.

Secondary Criteria of Distance

It was in the nineteenth century that the founding of experimental psychology upon British empiricism led to the acceptance

by psychologists of the view that perception, the knowledge of the external world, is based upon experience and judgment. On the basis of experience the mind makes a judgment as to the nature of the external world—that was the view implicit in most of the thought about perception. Helmholtz's doctrine of unconscious inference (1866) was put forth because these judgments are immediate and the judgmental process is not detectable in consciousness. Thus it came about that the sensory data which form the basis of a perception seemed to function as criteria of judgment. In the perception of distance, therefore, the sensory data of convergence, accommodation and retinal disparity came to be regarded as 'primary criteria.' This term *criterion* persisted long after the immediacy of perception had been admitted and the doctrine of unconscious inference rejected. Like its equivalent *cue*, it is still used somewhat anachronistically today.

It is for the most part the secondary criteria that can be employed to give the perception of distance in drawings and paintings. Thus the history of the knowledge about them turns out to be the history of knowledge about the use of certain techniques in art. It is art if a man can draw what he sees, putting a three-dimensional world into two. It is science if he knows the principles involved in the success of his drawing as a representation of reality.

1. *Superposition*. Near objects may partially obscure far objects; the converse is never true. Hence the mind 'seizes'—if we may use the empiricistic modes of speaking—upon the interruption of one object at the boundaries of another as a criterion of the relative distance of the two objects. The interrupted object is farther away.

The circumstances attending the recognition of this principle are lost in antiquity. It is used in some of the most primitive drawings, and early art which knew no perspective observes this rule, perhaps largely for the need of getting objects close together while avoiding the confusion of drawing parts of different objects in the same place. Although the principle is too obvious ordinarily to receive special mention as an artistic technic, it eventually got into the lists of secondary criteria for the perception of distance, as, for example, Helmholtz's in 1866.

2. *Size and perspective*. The simple rule of perspective is: the remote is smaller than the near. Hence relative size may be a

criterion of distance, and continuous change in size in accordance with certain laws is perspective.

If a drawing or painting is to be realistically representative of the natural world, it must be fashioned in accordance with these laws. Realism is, however, sometimes ignored and sometimes even condemned by art; thus the absence of a competent perspective from ancient pictures may mean that exact representation was not desired. It is said that Agatharcos, who painted scenery for the plays of Aeschylus in the fifth century B.C., wrote a treatise on perspective which greatly impressed Anaxagoras, but the book is lost and there is no evidence as to whether its principles were practiced. Certainly the Romans used perspective; paintings on the walls of Pompeii (destroyed 79 A.D.) are excellent examples of its use. It was the naturalistic movement in Italian art, however, that brought about the formal recognition of the laws as we know them today. Although several famous painters contributed to this development in art, the two most important names seem to be Paolo Uccello (1397-1475), who has been called "the first scientific exponent of perspective," and Piero della Francesca (ca. 1418-1492), who carried the geometrical analysis of the principles much further and invented, it would seem, the vanishing point. Before that time mistakes in perspective were made even by great painters like Fra Angelico, who, according to one measurement, represented Christ as putting water into St. John's mouth with his hand seven feet away. The principles once thoroughly known, there were no longer gross errors, though the artist might choose, of course, to avoid the realistic representation of a third dimension.

The geometry of perspective, in that it approximately duplicates the retinal image, is an adequate basis for the perception of the third dimension. In actual experience, however, perspective is modified by other factors, as we shall see later when we come to discuss the problems of size and distance (pp. 288-299).

3. *Light and shade.* It cannot be a large step from the ability correctly to represent the lights and shadows of nature to the knowledge that the representation of light and shade gives depth to the perception of the picture. Hence the 'discovery' of this criterion of the third visual dimension, like the discovery of interposition, is lost in antiquity, although much primitive art, not seeking to be realistic, contented itself with flat outline drawing.

One can, nevertheless, assess the growing importance of this item of knowledge from the manuscript notes of that great genius—painter, anatomist, architect, engineer, scientist, observer of nature—Leonardo da Vinci (1452–1519), who sought to organize the use of light and shade in a set of principles for painting. What came to be called his *Trattato della Pittura* includes “six books” on the use of light and shadow, a discussion which is classical in the naturalistic movement in art. To speed the progress of naturalism, balked in its effort to get the third dimension of nature on two-dimensional canvas, Leonardo was developing the technics of securing depth by light and shade, as well as by perspective. Later we shall see his admission of naturalism’s defeat in the impossibility of painting a picture that resembles nature as seen in binocular vision (pp. 283 f.).

The scientific interest in the problem of relief came later. Rittenhouse noted in 1786 that a low relief or matrix may reverse its depth and appear like a raised relief or patrix, the concave turning to convex. After the discovery of stereoscopic depth (1833) interest in these reversible reliefs was increased. Schröder (1858) showed that a reversal of a matrix is very easy with a familiar object like a human head or an animal which is always experienced as convex outward, never concave. Oppel (1855), with an ingenious instrument which he called an anaglytoscope, showed that the perception depends, not upon the real direction of the light, but upon its perceived direction. A matrix with the light coming from some known direction is perceived correctly as a matrix because of the disposition of light and shade. In the anaglytoscope the light, perceived as coming, say, from the right, is interrupted, without the observer’s knowledge, by a screen and is reflected upon the matrix from the left by a mirror, thus reversing the disposition of light and shade. What the observer perceives is not a reversal of the direction of the light but a reversal of the relief, a patrix instead of a matrix.

4. *Aerial perspective.* Like the other painters’ criteria, aerial perspective has probably been recognized for a long time by artists who could and would copy nature. The term (“prospettiva aerea”) was proposed by Leonardo, who discussed the principle at great length. “There is another kind of perspective which I call aerial perspective,” he wrote in his notes, “because by the atmosphere we are able to distinguish the variations in distance

of different buildings which appear placed on a single line; as, for instance, when we see several buildings beyond a wall, all of which, as they appear above the top of the wall, look of the same size, while you wish to represent them in a picture as more remote one than another and to give the effect of a somewhat dense atmosphere. . . . Hence you must make the nearest building above the wall of its real color, but make the more distant ones less defined and bluer. . . . If one is to be five times as distant, make it five times bluer."

Berkeley appealed to aerial perspective to explain the moon illusion. Ever since Ptolemy the accepted explanation of this illusion has been that the moon looks farther away at the horizon than in culmination, so that, since it always subtends the same angle, it also looks larger on the horizon. A far object that subtends the same angle as a near object must be larger than the near object. Berkeley, on the other hand, had this novel reason to offer for the changed distance of the moon: the atmosphere is hazy, and the haze is greater toward the horizon because there is more air to look through; hence the horizon moon seems larger. One further argument for this view as against others was that the moon on the horizon varies somewhat in size, and similarly the haze in the atmosphere also varies in amount. One variation might explain the other. Euler, the mathematician, supported Berkeley's view, but there were not many others. To Ptolemy's theory of the moon's size we turn next.

5. *Filled and empty distance.* The principle that a filled distance looks longer than an empty distance is one of the oldest to have received formal recognition as a perceptual law, for it was the principle that Ptolemy (ca. 150 A.D.) employed to explain the moon illusion. The distance to the horizon seems greater than the distances up to the vault of the heavens because it is filled with objects. Hence the moon on the horizon seems farther away and, consequently, larger. It is this explanation which was fixed upon the ancients. Helmholtz named almost a dozen distinguished scientists from Alhazen (d. 1039) to Malebranche (d. 1715) who held the view, which seems likewise current among astronomers today.

6. *Parallax of movement.* Helmholtz noted that the movement of the head may resolve a confused visual field into depth. He wrote: "Suppose, for instance, that a person is standing in a thick woods, where it is impossible for him to distinguish, except

vaguely and roughly, in the mass of foliage and branches all around him what belongs to one tree and what to another, or how far apart the separate trees are, etc. But the moment he begins to move forward, everything disentangles itself, and immediately he gets an apperception of the material contents of the woods and their relations to each other in space, just as if he were looking at a good stereoscopic view of it." Thus he described the way in which the parallax of movement yields a depth which is quite as striking and compulsory as the depth provided by stereoscopy. The phenomenon is familiar in the view from a moving train or vehicle, and more recently in the moving pictures when the camera has been moved continuously while the photographing was in progress.

Because no parallax is possible in a picture, it is not to the painters that one would go for mention of this criterion. Leonardo, of course, may have been aware of this principle, in that it shows one of the ways in which paintings inevitably must fail to represent nature, but he seems to have left no record of such knowledge. Although Helmholtz's contemporaries spoke of the principle as obvious, in general the older optical texts fail to mention it, either because it was not recognized, or perhaps because it was too obvious.

7. *Reversible perspective.* Wheatstone remarked that the geometrical solids which are usually shown in outline in the eleventh book of Euclid are often seen to reverse themselves, the near becoming far and the far near. Thus it is possible that the phenomenon of reversible perspective has been known to geometers ever since Euclid's time. The first person, however, to call attention to the fact in print and as a scientific observation seems to have been the Swiss naturalist, Necker, who in 1832 wrote a letter to Sir David Brewster in England describing this "optical phenomenon which occurs on viewing a crystal or a geometrical solid." Necker illustrated his point with a drawing of a rhomboid resting on one edge (Fig. 37A), but in later years the figure has generally been drawn as a cube (Fig. 37B). Necker noted that, although the reversal of the figure may seem fortuitous, the change can be controlled by fixing the regard on *M* or *N*, for the corner fixated is generally seen as nearer to the observer and thus determines the perspective seen. As a matter of fact, Necker's original rhomboid is reversed more easily than the cube, for the rhomboid stands upon an edge and is prejudiced for neither perspective, whereas

the cube is seen more easily flat upon the ground (*M* near) than in the alternative peculiar uptilted position (*N* near).

Interest in the problems of the visual perception of depth was greatly increased by the invention of the stereoscope. Schröder, in

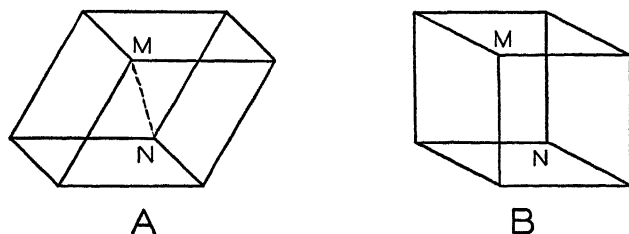


FIG. 37. REVERSIBLE PERSPECTIVE: NECKER'S RHOMBOID (1832) AND CUBE

Fixation of *M* gives one perspective; fixation of *N* gives the reverse. The rhomboid *A* was the original figure. The cube *B* is more easily seen with *M* near.

his study of visual relief (1858) which we have already mentioned, presented the stair figure of Fig. 38. Drawing this figure with shading as if the light were coming from above, he seems to have found that the most usual result is for one figure to be seen as steps, and the other as the under-side of some step-like broken masonry. It is apparent, however, that the light and shadows are not the essential factors, for the two figures can be seen simultane-

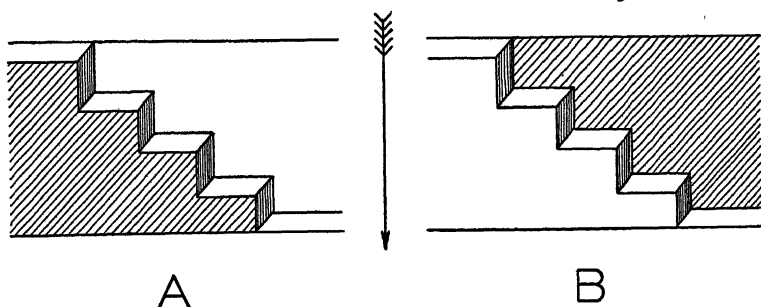


FIG. 38. REVERSIBLE PERSPECTIVE: SCHRÖDER'S STAIRS (1858)

Schröder drew these figures with shading so that the light (indicated by the arrow) could be considered as a determinant of the perspective. The stairs are seen either from above, or from their under-side. Direction of illumination is, however, less important than other factors like fixation (cf. Fig. 37) and this figure is usually reproduced without the shading.

ously in the same perspective, either as stairs or the reverse, and "Schröder's stairs" have usually been reproduced without the shading.

In 1860 Sinsteden noticed that a windmill, seen obliquely and darkly silhouetted against the bright evening sky, kept reversing

its direction of rotation so that he could not tell whether he was seeing the mill from in back or in front. This effect, which has since been reproduced by projecting on a screen the shadow of slowly revolving motor-driven vanes, is even more startling than the reversal of the static drawings. Fig. 39, the silhouette of a perspective drawing of a real mill in Essex, England, shows what Sinsteden must have seen. The arms of the mill rotate in the direction of the arrow. This rotation is clockwise if the arm *m* is seen near the observer, that is to say, if the perspective is that of seeing the arms from behind with the fourth arm obscured by the mill house. When the perspective reverses, the arm *m* is far from the observer, and the arms are seen as from in front with the fourth arm in front of the mill house but in-



FIG. 39. REVERSIBLE PERSPECTIVE:
SINSTEDEN'S WINDMILL (1860)

Sinsteden drew no such explicit figure, but this is what he must have seen, a windmill silhouetted against the sky. When *m* is perceived as near, the observer sees the left side of the plane of rotation and thus the rotation is clockwise. When *m* is perceived as far, then the other side of the plane of rotation is seen and the rotation becomes counterclockwise.

visible in the black silhouette; thus the rotation changes to counterclockwise. The reversal shifts the observer from the left to the right side of the plane of rotation, and the effect is that of changing the direction of rotation, for in one instant the upper arms move away from the observer, and in the next they are found moving toward him. Sinsteden found that he could fix the direction of rotation by imagining himself on the one side of the mill or the other, a discovery that is comparable to Oppel's that relief is determined as a matrix or patrix according as one believes the light to come from one side or the other.

There is not much history to the reversible perspective because the fact was obvious and no one has been successful in offering a plausible explanation of it. Lange (1888) and Marbe (1893), both working in Wundt's laboratory, measured the rate of reversal and sought to relate the phenomenon to the fluctuations of faint stimuli, and thus to the fluctuations of attention, but nothing came of the idea. Recently the facts of perspective have been exhibited as examples of the dynamic organization of a perceptual field, and to this matter we shall return later (pp. 299-303).

Convergence and Accommodation

The 'primary criteria' of the perception of depth and distance came, in the course of the nineteenth century, to be recognized as three: (1) the convergence of the eyes, (2) the accommodation of the eye's focus, and (3) the retinal disparity that is due to binocular parallax. Of these three principles, knowledge about convergence is the most ancient, whereas knowledge of retinal disparity is comparatively recent, being for the most part only of the last one hundred years.

1. *Convergence.* The fact of the convergence of the two eyes upon the object of regard was known to the ancients. Euclid knew it. Indeed the principle could hardly be overlooked by anyone interested in the geometry of binocular vision, since the movement of the eye to 'look at' an object is one of the most obvious facts about vision. The significance of convergence, on the other hand, was not apprehended so early—sometimes for the reason that it was not certain the two eyes can function simultaneously, for one theory of the singleness of binocular vision had been that the vision of one eye is suppressed in favor of vision by the other, with alteration of vision between the two.

The nature of convergence was, however, clear to Aguilonius, who, as we have seen (pp. 226 f.), invented and defined the horopter in 1613. Aguilonius thought of the horopter as a plane containing the point of fixation, so that the distance of the plane from the observer would depend upon the angle between the eyes. Descartes in 1637 described the eyes as feeling out a distance by the convergence of their optic axes, just as a blind man might feel out a distance with two staves, one in each hand. He drew a picture of the blind man. He assumed that a man, knowing the direction of

the regard of each eye, would, by a "natural geometry," be able to appreciate the distance of the object regarded.

Berkeley in 1709, accepting the fact of convergence as well established, noted that there remained still the question as to how these movements of the eyes could convey the idea of distance to the mind. To the solution of the problem he applied empiristic and associationistic theory. "When we look at a near *object* with both eyes, according as it approaches or recedes from us," he wrote, "we alter the disposition of our eyes, by lessening or widening the distance between the *pupils*. This disposition or turn of the eyes is attended with a sensation, which seems to me to be that which in this case brings the *idea* of greater or lesser distance into the mind. . . . Not that there is any natural or necessary connexion between the sensation we perceive by the turn of the eyes, and the greater or lesser distance. But because the mind has by constant *experience* found the different sensations corresponding to the different dispositions of the eyes, to be attended each with a different degree of distance in the *object*: there has grown up an habitual or customary connexion, between those two sorts of *ideas*." And he went on to stress the role of experience in the association of these two ideas and to discredit Descartes' view that one perceives actually the angle of the eyes in order to apply a natural geometry in the judgment of distance.

Through two full centuries of psychology this view of the function of convergence in the perception of distance persisted. It was essentially the view of Helmholtz and Wundt and Titchener. Titchener's context theory of meaning implied it: the meaning of a particular distance is given to the sensory visual core of the perception by the accrual to that core of the kinesthesia of the particular convergence that the eyes assume. Berkeley's theory of vision was published in 1709, and Titchener's context theory of perception in 1909.

2. *Accommodation*. So long as Galen's view that the 'crystalline' is the seat of visual sensation persisted, there could be no problem of the change in the eye's focal length; but as soon as Kepler (1604) had established the 'crystalline' as a lens and the retina as the sensitive element, and had been confirmed by Scheiner (1625) and Descartes (1637), who both showed that an extirpated eye actually focuses an image on the retinal region, then the problem arose. (See pp. 222 f.) Thanks to the knowledge of the tele-

scope, the physics of optical systems was too well known for there to be much doubt in the scientific mind that the eye would have to change its focal length if it was to get on the retina clear images of objects at different distances. It was, in fact, not long after Kepler fixed upon the retina as the organ of sensation that Scheiner (1619) performed the experiment which has since been called by his name. Having prepared a card with two pinholes in it, closer together than the diameter of the pupil, he viewed with a single eye through these holes two pins unequally distant. Either pin, if fixated, he saw as single, because the lens superposed the two images on the retina when the focus was right; but then the other pin, for which the focus was wrong, was seen as double. The fact that the eye changes focal length was thus established.

Kepler had supposed that the focusing might be accomplished by the back-and-forth movement of the lens, but Descartes (1637) hit at once upon the correct idea, that it changes its curvature. About the function of accommodation in the perception of distance, he was quite explicit, noting that accommodation is effective at short distances of three or four feet, whereas convergence is effective at the long distances—15 or 20 feet, he said in one place, 100 or 200 in another.

Berkeley (1709) argued for three kinds of "ideas" as the cues to the perception of distance. Besides the distance between the pupils (convergence), he held that far objects—fitting, as it were, the natural focus of the eye—are seen clearly and that near objects tend to be blurred; as a consequence, the idea of nearness becomes in experience connected with the idea of blurredness. Although in this matter he was in error, since far objects become blurred with near fixation, the point led him to the observation that the "straining" of the eye will make an object in near vision less "confused." This was in accordance with the evidence of introspection that a strain is felt in near-accommodation.

It cannot be said that Kepler or Descartes or Berkeley settled the matter of accommodation as a cue to the perception of depth, for the fact of accommodation remained in some doubt—in spite of the existence of conclusive experiments like Scheiner's—until the mechanism had been established by Helmholtz. A fact gets set in the structure of science, not often by one line of direct evidence, but generally by means of several converging lines along

with circumstantial evidence. If all one knows is that the laws of optics require the eye, having clear vision at different distances, to be able to accommodate, one may still doubt the fact of accommodation; but a person will not doubt it if he knows what is the nature of the mechanism of accommodation—or if he believes that he knows.

There have been during the three centuries since Kepler altogether six theories about accommodation, their histories being to a large extent contemporaneous. Let us consider them, however, successively. The word *accommodation* seems to have been introduced by C. A. Burow in 1841. Johannes Müller before him said *adaptation*. The earlier writers used various descriptive phrases. We speak always of *accommodation* here, adopting a verbal anachronism for the sake of simplicity of expression.

(a) *There is no mechanism of accommodation.* The simplest theory of accommodation is, of course, its denial, the contention that the eye has a universal focus. This view de la Hire presented in 1685. He held that objects at one distance would be clearly focused and the blurring of other images ignored in recognition. While de la Hire, in view of Scheiner's experiment, may seem to us to have been denying a demonstrable fact, we must remember that it was then known that the doubling of binocular images of objects lying outside the horopter is generally ignored. In a very crude way, de la Hire was anticipating the modern constancy phenomenon, for he believed that objects are perceived for what they are in spite of the inadequacies of their immediate sensory excitation.

Much later the famous physiologist Magendie (1816) gave the weight of his authority to this view that the eye has a universal focus. He claimed that the sharpness of the image on the sclerotica of the extirpated eye of the white rabbit does not change with the distance of the imaged object from the eye, but Helmholtz remarked later that the sclerotic coat of the eye is a very poor screen on which to define a sharp image. Magendie believed that the crystalline is not a lens but has other functions in the eye.

Finally, there was Treviranus, who published in 1828 quantitative data on the dimensions of the eyes of man and of many kinds of animals. He argued that, because it has many layers of differing density from the front to the back, the lens has a universal focus and would act differently for objects at different distances, especially if the contraction of the pupil in near vision removes the

outer portions of the lens from optical functioning. His was a complicated theory and never won support. Having measured so many eyes so accurately, Treviranus was perhaps loath to admit too much freedom of movement within the eye.

(b) *The contraction of the pupil in near vision is the mechanism of accommodation.* The *camera obscura* with a pin-hole aperture has a universal focus, and the larger the hole the less sharp the image. That the eye is analogous to the *camera obscura* is the assumption of this second theory. The lens is supposed to be permanently adjusted for far vision, a fact which can seemingly be recognized in the relaxed state of the eye with remote fixation. The blurring in near vision would then be reduced by the contraction of the pupil. Scheiner's experiment seems to disprove the assertion that pupillary contraction is the sole mechanism of accommodation; nevertheless it was Scheiner himself who discovered that the pupil contracts in near vision, and who suggested this function for the contraction. He also believed that the lens moves in accommodation, but the great physiologist Haller (1763) held to the contraction theory alone. Various other writers supported this view after Haller, the most important of them and the latest being the Dutch physiologist, Donders, in 1846, ten years before Helmholtz brought together the evidence that the lens changes shape in accommodation. Helmholtz (1856) refuted this theory by pointing out that accommodation is not noticeably impaired by the use of an artificial pupil, a hole in front of the real pupil and smaller than the smallest size of the real pupil.

(c) *The external eye-muscles elongate the eye-ball in near vision.* This theory was for several reasons a natural proposal. The external muscles are obvious; muscle fibers inside the eye were unknown; and the familiar method of focusing a telescope was by changing the length of its tube. The view was first put forth by Sturm in 1697 and, according to Helmholtz, was supported by almost a score of others before Listing, who argued for it in Wagner's *Handwörterbuch der Physiologie* in 1853. For a century and a half it was the most widely held of the incorrect theories.

We have already seen (pp. 109 f.) that Thomas Young, who in 1801 sought to test this view, came to a negative conclusion. Young turned his eye inward toward his nose, forcing the smooth ring of a key between the eye-ball and the conjunctiva in such a manner that he could look over the bridge of his nose through the

ring of the key, while the anterior surface of his eye-ball was prevented from moving forward. Then he forced the ring of a small key in behind the eye between the eye-ball and the bone. The resultant pressure led to dark images on the retina, and, since these images reached to the center of the field of vision, Young concluded that he actually did have his eye-ball firmly constrained. Nevertheless he was able to alter his accommodation for near and far. Helmholtz, always a champion of Young, described this paper as "a work of astonishing perspicacity and originality, which was qualified to settle the question as to accommodation even at that time," a paper which Listing and the others may have missed because, in Helmholtz's phrase, "on account of its conciseness, it is often hard to follow, and, moreover, it presupposes the most thorough knowledge of mathematical optics."

(d) *The curvature of the cornea changes in accommodation.* This view is closely related to the preceding one, since the curvature of the cornea can only be changed by action of the eye-muscles, and elongation of the eye-ball would be likely to change the curvature of the cornea in the right direction. Though the theory is said first to have been put forward by Lobé in 1742, its most elaborate support came from Home in 1795. John Hunter had been appointed to give the Croonian Lecture in 1794 and had been intending to show that the lens changes shape in accommodation, when he died. Home not only undertook to present Hunter's views for him as well as possible, but, when the next year he was himself appointed Croonian Lecturer, he continued the topic, arguing that the observed change of curvature is in the cornea and not the lens. He reported observations performed by himself with a microscope and confirmed by two colleagues, observations that showed the cornea bulging out in near fixation. Others, however, failed later to confirm these experiments. Helmholtz was of the opinion that they were refuted by Thomas Young's demonstration that accommodation is not noticeably impaired when the eye is covered with water, since the refraction between the cornea and water is very much less than the refraction between the cornea and air.

(e) *The lens shifts back and forth in accommodation.* This is the original theory, the one that Kepler proposed in 1611. Scheiner seconded it in 1619, supposing that a forward shift of the lens in near vision is accompanied by pupillary contraction. Although

Descartes presently introduced the view that the lens changes shape and does not move back and forth, many investigators continued to believe that Kepler had been right. Johannes Müller in his monograph on the comparative physiology of vision (1826) held to this view, as did Burow in an important text on physiological optics that he published in 1841. Undoubtedly the theory persisted because there were insufficient grounds for a decision. One important line of evidence is the relative movement, with changing accommodation, of the images reflected from the front and rear surfaces of the lens—Sanson's images. It is true that these images were described by Purkinje in 1823 and by Sanson in 1838; nevertheless their true importance was not generally realized until Helmholtz emphasized it in 1856. That the lens should change position would seem simpler than that it should change shape; and scientists often hesitate to accept the less simple theory until there is strong evidence in its favor.

(f) *The lens changes shape in accommodation.* We have seen that Descartes originated this view and that John Hunter had been intending, when he died, to support it in his Croonian Lecture in 1794, basing his argument on the structure of the lens. Thomas Young's investigation of the mechanism of the eye (1801) came out, as we have also noted, with this conclusion. Purkinje supported the theory (1825), and Helmholtz championed the view so vigorously that the idea is sometimes said to be his. The most important evidence in its support is found in the behavior of the images reflected from the surfaces of the lens. These images Purkinje described in 1823. Sanson, having used them in 1838 in his method for the diagnosis of cataract, much later lent his name to them. Both Purkinje and Sanson employed the reflection of a candle flame (Fig. 40), which by its shape shows when it is erect or inverted. Cramer in 1851 improved the technique, using a light through a tube to cast a pear-shaped image, which could be judged both as to size and as to whether it was erect or inverted. Helmholtz improved the technique further by using two separated squares of light (Fig. 40), which, although they do not clearly indicate inversion, show the change of size by their altered separation. Helmholtz attributed the discovery of the images to Langenbeck in 1850 (missing Purkinje and Sanson, although he cited Sanson in other contexts), and discovered Cramer's papers only after he had independently discovered the images. Cramer was ex-

plicit in his argument for accommodation as a change in curvature of the lens, but it took Helmholtz's authority to get the theory accepted.

To observe these images (Fig. 40) the experimenter must place his eye so that he can see in the pupil of a subject the reflected images of a candle flame or some other light. As the subject changes his accommodation, the change in size and position of the images is noted. The erect bright image at the left (in the figure)

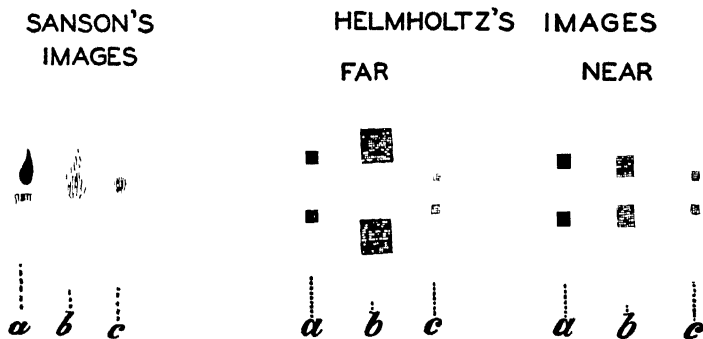


FIG. 40. SANSON'S (1838) AND HELMHOLTZ'S (1856) IMAGES

The left figure shows the reflections of a candle flame as Sanson reproduced them. The right figures are Helmholtz's reflections of a pair of illuminated squares for far and near fixation. *a* = erect reflection from cornea; *b* = erect reflection front anterior surface of lens; *c* = inverted reflection from posterior surface of lens. In near fixation the images from both surfaces of the lens are smaller because of greater convexity of the lens, and nearer together.

is from the cornea; the erect, large faint image in the middle is from the front surface of the lens; the very small inverted faint image at the right is from the posterior surface of the lens, and is inverted because it is seen through the lens. The relative sizes appear better when the reflected object is the two squares of light at the right of Fig. 40. The change of accommodation from far to near makes the middle image decrease notably in size, since the size of an image reflected from the convex surface of the lens is proportional to the radius of curvature of the surface. The image from the back of the lens gets a trifle smaller in near fixation, showing that the posterior surface of the lens bulges a little in near accommodation, yet only a little as compared with the front surface. This experiment also shows that the cornea has its convexity

increased slightly in near vision, but not enough to account for the facts of normal accommodation.

Helmholtz's only original contribution to what became the accepted theory of accommodation was to provide an explanation of the means by which the lens can change shape. He suggested that the lens is elastic and under tension. By itself, consequently, it would tend toward a spherical shape and thus toward near accommodation. It is, however, also under tension from the ciliary process which, attached to the lens and also to the walls of the eyeball, transmits a pull that tends to flatten the lens for far accommodation. This is the resting position of the eye with two unfelt antagonistic tensions. In near accommodation the muscular fibers in the ciliary process contract, with a sensation of effort, to release the tension and allow the lens to bulge. It was the plausibility of this mechanism that went far toward winning assent to the direct evidence of Sanson's images.

TABLE I. WUNDT'S EXPERIMENT ON THE PERCEPTION OF DISTANCE
(1859-1861)

Just noticeable differences, with binocular and monocular vision, in the distance of a thread receding from or approaching the observer's eyes. All figures in cm. The table abbreviates Wundt's data slightly.

<i>Distance of thread from eyes</i>	<i>Just noticeable differences</i>			
	<i>Binocular vision</i>		<i>Monocular vision</i>	
	<i>Approach</i>	<i>Recession</i>	<i>Approach</i>	<i>Recession</i>
50	1	1	4.5	6.5
80	2	2	5	7
100	—	—	8	11
110	2	2	—	—
150	3	3	—	—
180	3.5	5	8	12
200	—	—	8	12

3. *Relation of Accommodation and Convergence.* Wundt was the first person to submit to experimental measurement the functions of accommodation and convergence in the perception of distance. It was, in fact, this problem that constituted Wundt's maiden effort in experimental psychology in 1859-1861. The observer in his experiment, looking through a tube at a distant white background, saw the field of view crossed by a suspended black thread, which could be moved back and forth from or toward him without either the upper or lower end becoming visible. The observer looked at the background and then at the thread, noting its distance; immediately thereafter, while he turned his head to one

side, the thread was moved either from or toward him; then finally the observer, looking through the tube again, first at the background and then at the thread, judged whether it had receded from him or approached him during the interval. The nature of Wundt's results for both binocular and monocular vision is shown in Table I, where are given the just noticeable differences (*Unterscheidungsgrenzen*) for approach and recession of the thread.

That convergence is more important than accommodation in the perception of depth was Wundt's conclusion, because the just noticeable differences for binocular vision were so much smaller than the ones for monocular vision. Wundt also noted that the thresholds for recession were much larger than the thresholds for approach in monocular vision, although in binocular vision there was no difference. This observation led him to conclude that the subject can make more accurate discrimination in innervating the muscles of accommodation than he can in relaxing them, a view that was the beginning of Wundt's belief in the existence of sensations of innervation. In fact, this difference in the monocular thresholds led him to suppose that accommodation functions not at all in recession, but that the judgments in recession are based upon the diminution in the apparent size of the thread.

These somewhat meager experimental results remained unchallenged until 1894 when Hillebrand, a protégé of Hering's, criticized them and published the results of a new investigation. Hillebrand eliminated the effect of the apparent size of the perceptual object by using a line that had no breadth, the sharply cut edge of a cardboard screen which, in front of the remote background, filled half of the field of vision. For sudden changes of distance the screen on one side could be removed and another screen on the other side and at a different distance immediately introduced. For gradual changes a single screen could be moved slowly along the visual axis. In order to eliminate the double images that might provide cues when both eyes see the field, Hillebrand worked with monocular vision only. He argued that convergence is so related to accommodation that the eyes converge even when one of them is screened from the perceptual object.

Hillebrand was unable to obtain thresholds with gradual change. The observers could not tell which way the screen was being moved. He concluded, therefore, that accommodation (and therefore also convergence, as he argued) does not mediate the

perception of distance. When the change of distance was abrupt, he obtained large thresholds due, he thought, to the dispersion circles that occur with faulty accommodation and their elimination by trial and error.

TABLE II. BAIRD'S EXPERIMENT ON THE PERCEPTION OF DISTANCE (1903)

Just noticeable differences, with binocular and monocular vision and with abrupt and gradual change, in the distance of a line of no thickness receding from or approaching the observer's eyes. The j.n.d. are in percentages of the standard distance from the eyes, which is given in cm. Av. = average of 4 observers with normal vision. B = one observer with very slow accommodation.

Standard distance from eyes		Just noticeable differences in per cent					
		Binocular		Monocular		Monocular	
		Abrupt		Abrupt		Gradual	
		App.	Rec.	App.	Rec.	App.	Rec.
28.6	Av.	1.8	1.9	8.7	13.4	25	51
	B	2.1	2.5	12.6	47.0	36	206
33.3	Av.	1.7	2.0	8.7	15.5	24	49
	B	1.5	1.5	10.8	22.0	41	105
40.0	Av.	1.8	1.7	9.7	17.4	25	46
	B	1.9	2.5	8.5	25.0	51	88
50.0	Av.	1.7	1.6	9.3	14.4	26	36
	B	1.4	2.0	5.4	35.0	53	77
66.7	Av.	2.0	1.8	10.6	14.5	24	29
	B	2.1	1.9	8.6	16.2	64	142+*

* The range of the apparatus, 95 cm., was not great enough to measure this j.n.d.

There was some controversy. Arrer repeated Wundt's experiment and defended him. Hillebrand replied. Then in 1903 Baird conducted a thorough investigation of the whole matter. Although he used Hillebrand's form of apparatus, he followed Wundt in employing both binocular and monocular vision. His results appear in Table II as the items labeled "Av." It is clear that the thresholds for binocular vision are much smaller than for monocular vision, also that there is no difference in the thresholds for approach and for recession in binocular vision, but that the thresholds for recession are much greater in monocular vision than the thresholds for approach. Thus Baird concluded that convergence furnishes the most important cue to the perception of distance. Hillebrand's notion that the doubling of images might furnish the basis of the judgment he rejected for the reason that double images are not specific as to direction. Since he was able to obtain thresholds with gradual movement of the screen—although they were

naturally much larger than the thresholds got with abrupt movement—he decided that accommodation plays a secondary role which becomes significant in monocular vision. Abrupt change always favors comparison. The fact that the limens of approach were less than the limens of recession in monocular vision seemed to mean that innervation of the mechanism of accommodation is a better cue to change of distance than is relaxation, and that was essentially Wundt's view.

By a lucky accident Baird had one observer, *B*, who could accommodate his eyes only very slowly. His thresholds are given separately in Table II. It will be seen that in monocular vision they are nearly always larger than the thresholds for the other observers, and that in the case of recession they are always larger. In binocular vision, on the other hand, there is no significant difference between *B* and the other observers. Thus it appears that faulty accommodation increases the difference between monocular and binocular perception of distance, thus reenforcing the argument that accommodation normally furnishes in monocular vision a cue for distance. The slow accommodation of *B* also showed, against Hillebrand, that convergence can occur and function accurately in advance of accommodation.

Stereoscopy

The two eyes, being separated in the head by a distance between 60 and 70 mm. and having their lines of sight converging upon the fixated object, see slightly different views of the object. Although this binocular parallax furnishes a cue to the perception of depth, very little was made of it before Wheatstone's inspiration, about 1833, that the consequent disparity in the two retinal images can be a reason for the perception of depth. Before Wheatstone the fact of binocular parallax was recognized, and it was known that parallax must give disparate retinal images. The theory of corresponding points, however, required that such disparity should mean a doubling or confusion in the perception. Since the perception of doubling is not identical with the perception of depth, the matter remained for a long time an unexplored puzzle, with the possible explanation that the doubling is ignored and that depth comes into perception in other ways.

Euclid noted the fact that a single eye sees less than half a

sphere, since the tangents forming the angle that subtends the sphere are not tangent at opposite ends of a diameter. He also noted that, if the sphere is smaller than the distance between the eyes, the two eyes together may see more than a hemisphere. With the sphere, of course, the problem of disparity between the two

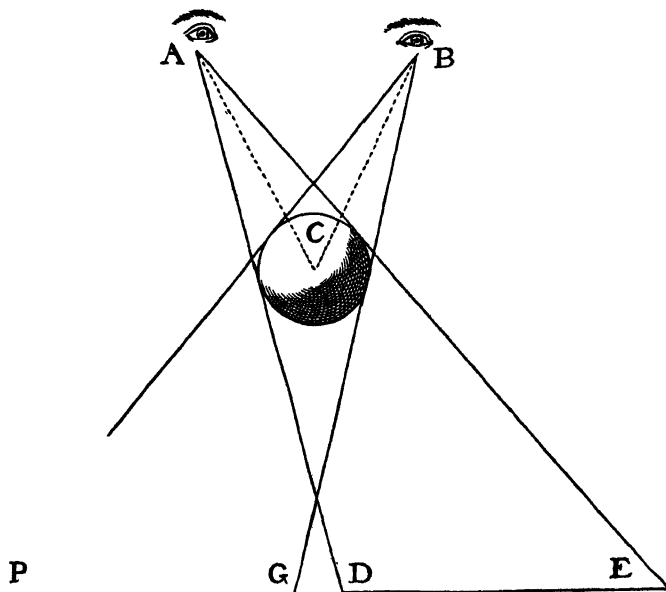


FIG. 41. LEONARDO'S PARADOX (ante 1519)

The eye at A sees the background PD, and the eye at B the background GE; hence both eyes see all the background and the opaque object C is transparent to binocular vision. Hence a painting cannot reproduce accurately what is apparent to binocular vision. This drawing was made by L. B. Alberti in the publication of Leonardo's notes in 1651.

images does not arise. Galen (ca. 175 A.D.), on the other hand, described the differences in the perception of a column, with the left eye, with the right, and with both. He seemed, however, chiefly interested in the fact that more features of an object can be seen with two eyes than with either eye alone.

Among his manuscripts Leonardo da Vinci (d. 1519) left a note on the transparency of objects. "If nature is seen with two eyes, it will be impossible to imitate it upon a picture so as to appear with the same relief, though the lines, the lights, shades and color be

perfectly imitated." He presented a sketch like that of Fig. 41, showing that the sphere "C becomes as it were transparent according to the definition of transparent bodies, behind which nothing is hidden." The eye at *A* sees the background *PD*, the eye at *B* sees *GE*, and *PD + GE* includes the whole of the background. Wheatstone remarked that, if Leonardo had used a cube instead of a sphere, he would have become aware of the fact of retinal disparity. Brewster, on the other hand, insisted that Leonardo, excellent observer that he was, must as a matter of course have been aware of the disparity of the images. The fact would seem to be that, even if Leonardo noted the necessary disparity of the two images, he might not have been interested in them. His interest lay in the accurate reproduction of the appearance of nature in painting. He was already noting the paradox that opaque objects may also be transparent to binocular regard. It would not have helped naturalism in art to observe that what should have been a confused superposition of two disparate images is in reality a clearly outlined solid image, although it would indeed have been an achievement in phenomenological observation.

On the other hand, Aguilonius in 1613 noted the problem, but missed the implication that it could lead to an understanding of the perception of depth. It was natural that the inventor of the horopter should be puzzled as to why solid objects, which cannot, of course, lie completely within the line of the horopter, should nevertheless appear neither confused nor double. He wondered "how it happens that bodies seen by both eyes are not all confused and shapeless, though we view them with optical axes fixed on the bodies themselves. . . . Since the images in each eye are dissimilar, the representation of the object must appear confused and disturbed to the primary sense. . . ." Aguilonius then went on to say that what is "truly wonderful" is that objects actually are "seen clearly and distinctly with both eyes when the optic axes are converged upon them." How, he asked, is this contradiction explained? By the action of "the common sense." "Whatever body . . . each eye sees with the eyes conjoined, the common sense makes a single notion, not composed of the two which belong to each eye, but belonging and accommodated to the imaginative faculty to which the common sense assigns it." Aguilonius appealed to the common sense as we today would appeal to meaning, saying perhaps that what is confused in sensation is neverthe-

less clear in perceptual meaning. But he did not make the last step. He explained disparity away, without realizing that it is disparity which under the constitution of the organism becomes perceived depth.

The men who in the first part of the eighteenth century wrote on physiological optics—men like Robert Smith and Porterfield—discussed the visual disparities due to binocular parallax, yet without discovering that disparity yields depth. Thus it would seem that

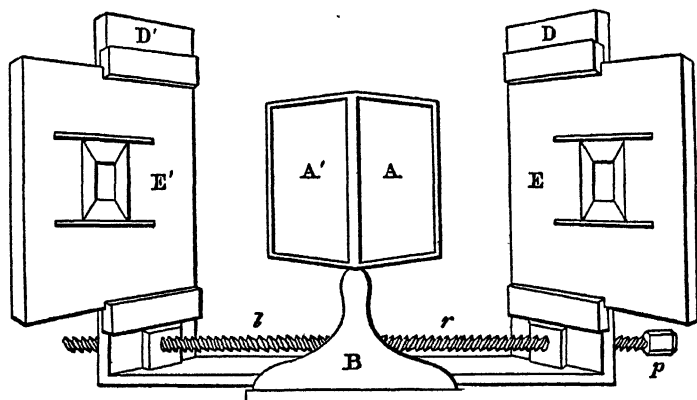


FIG. 42. WHEATSTONE'S STEREOSCOPE (1838)

The panels *EE'* bear the disparate drawings which are reflected by the mirrors *AA'*, one into each eye as the observer faces the stereoscope. Adjustments are made by sliding *EE'* in the uprights *DD'*, and changing the distance between *DD'* and *AA'* with the screw *p*.

this notion may be original with Joseph Harris, who said in his *Treatise of Opticks* in 1775: "And by the parallax on account of the distance betwixt our eyes, we can distinguish besides the front, part of the two sides of a near object, not thicker than the said distance; and this gives a visible *relievo* to such objects, which helps greatly to raise or detach them from the plane, on which they lie: Thus, the nose on a face, is the more remarkably raised, by our seeing each side of it at once."

We may say, then, that the fact of stereoscopic vision was ready to be discovered and established. Wheatstone seized upon disparity as an essential condition of the perception of depth. For all that he never knew why disparate images should yield a single

clear perception in three dimensions, he nonetheless realized that they do. He proved his point, moreover, by inventing what he called a stereoscope (Fig. 42), a device in which two mirrors, A and A', reflect respectively to the eyes the disparate drawings on the slides E and E'. So striking is the resulting synthetic perception of depth that it is no wonder the phenomenon commanded attention after Wheatstone had published his complete description of it in 1838. The date of discovery is generally said to be 1833, for it was then that Mayo included in his *Physiology* a very brief mention of the phenomenon, but it was not until five years later that the facts received the notice that they deserved.

Wheatstone's discovery was remarkable for two reasons. In the first place, it isolated one criterion of depth from all the others and showed that it could be exceedingly effective alone. Without the aid of convergence or accommodation or any of the 'secondary

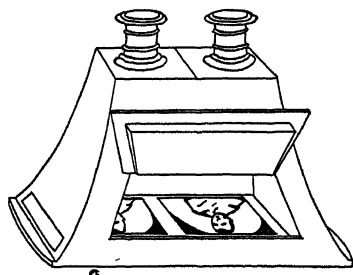


FIG. 43. BREWSTER'S REFRACTING STEREOSCOPE (1849)

The first form of Sir David Brewster's refracting stereoscope with lenticular prisms. In 1843 Brewster described the principle, and in 1849 manufactured this stereoscope, which came into general use in the early 1850's.

criteria' for the perceptual differentiation of depth, simple geometrical figures, perceived in the stereoscope, stood out with immutable insistence, often with a dimension of depth that exceeded their horizontal and vertical dimensions. In the second place, the stereoscopic experiment was convincing because it was synthetic. It was not necessary to infer disparity of images from binocular parallax, and then to wonder how the disparity of images from a unitary object could give the perception of a unitary object. In the stereoscope the disparity was actually

given in the drawings selected for use, and the nature of the perceived depth could always be predicted from the nature of the disparities chosen.

Once the principle of the stereoscope was recognized, it became possible to apply it in various ways, and to construct instruments in a variety of forms. Sir David Brewster, having in 1843 outlined the principle of a refracting stereoscope which uses lenticular

prisms, proceeded to construct one in 1849. In the 1850's many of these stereoscopes were built and sold in England and France for the edification of the public (Fig. 43). Still other optical systems were proposed. In 1856 Brewster was able to describe eleven different kinds. The hand stereoscope with the hood (Fig. 44), an accessory of the Victorian parlor, was invented by the physician-author-poet, Oliver Wendell Holmes, in 1861. The use of photography—daguerreotypes and calotypes—for making stereograms also became common in the 1850's.

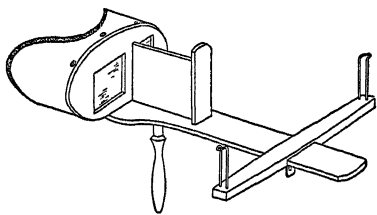


FIG. 44. HAND STEREOSCOPE (1861)

This type of stereoscope with the hood was first designed by Oliver Wendell Holmes.

In 1852 Wheatstone described a refracting pseudoscope, an instrument with an optical system that reverses the parallax, as if the left eye were placed to the right of the right eye. With drawings or photograms the pseudoscopic effect is got simply by reversing the stereograms, left for right. The refracting pseudoscope, however, makes it possible to reverse even simple objects, convex for concave. Now it became apparent that the stereoscopic effect occurs only when other cues to space do not interfere. Stereograms with perspective cues in them may not reverse pseudoscopically, nor is it possible with a pseudoscope to turn a living human face concave.

In 1857 Helmholtz described the telestereoscope, an instrument in which the eyes see a view with the parallax of two mirrors set far apart. A telestereoscope used at the window may thus resolve a landscape into depth. Helmholtz also built a telestereoscope combined with a double telescope, so that distant objects could be magnified at the same time that the binocular parallax is increased for them.

Wheatstone regarded his discovery as a direct challenge to the theory of corresponding points, the basic theory that accounts for singleness of vision with the two eyes. Brücke suggested almost immediately (1841) that a solid object is perceived with a horopter that shifts as the eyes explore the object. The object is not, he thought, seen solid at the first instant, but only as the eyes converge first upon this part and then upon that. The parts of the ob-

ject lying outside the instantaneous horopter are not seen double because they are ignored. Dove, however, exploded this plausible theory by showing that a stereogram can be perceived as a solid object when illuminated instantaneously by an electric spark. Such an illumination, which lasts less than a millionth of a second, does not allow time for the eyes to move at all.

Panum in 1858 took Dove's finding to mean that space is native and not generated (as Lotze had argued in 1852) by eye-movements. He introduced the notion of corresponding sensory circles, based on Weber's conception of sensory circles on the skin. Weber held that double stimulation within a cutaneous sensory circle would be perceived as single; Panum held that similar contours falling within corresponding sensory circles on the two retinas would be perceived as single. From this point he went on to discuss the "sensations of binocular parallax" that could give rise to the perception of depth, being mostly concerned with getting away from empiricism and eye-movements to nativism and a non-kinesthetic visual space.

Hering's nativistic theory of visual space was influenced greatly by Panum. Hering, it will be remembered (p. 29), supposed every retinal point to have three local signs—a sign for depth in addition to those for the vertical and horizontal dimensions. Thus it would be possible for images in the two retinas to be disparate in respect of the local signs for depth, and yet otherwise identical. This line of theorizing, however, has led nowhere, except insofar as it can be argued that Gestalt psychology takes its nativism from Hering. To its view that the perception of depth depends upon the organization of phenomenal data we shall return presently (pp. 299–303).

Size and Distance

As an object recedes from an observer its retinal image diminishes in proportion to its distance from the observer. In perception, however, the object appears to lose size much less rapidly, or it may even be "perceived" as constant in size. Thus the Gestalt psychologists, being anxious to call attention to the fact that size in the perception is not solely dependent upon the size of the retinal image, have called this relationship, not without exaggeration, "size constancy," a special case of the "constancy phenomenon" (cf. pp. 254–256).

The history of this phenomenon is more a history of the clarification of concepts than a history of empirical discovery. We must, therefore, first clarify the concepts in order to understand the history afterward.

There are three different kinds of size to be considered. This distinction was made quite clearly by Hering, the phenomenologist, in 1879, although the modern terminology, which follows, does not exactly repeat his phrasing:

1. Visual angle or retinal size, the size of the image upon the retina, therefore physiological size or the size of the proximal stimulus;
2. Phenomenal size or apparent size, the size of the sensory datum in perception;
3. Estimated size, the size as partially determined by judgment, inference, past experience, or other processes inclusive of more than the proximal stimulus.

It is easy to distinguish retinal size from the other two conceptions, but not easy to state the difference between phenomenal size and estimated size. Helmholtz (1866), recognizing that perception depends upon past experience as well as present stimulus and that its form is what might be expected in an inference as to the nature of a constant object, held that perception depends upon an instantaneous unconscious inference (*unbewusster Schluss*). Since an inference is ostensibly a conscious process and can therefore be neither unconscious nor immediate, this view was rejected as self-contradictory. The ultimate reason, however, for rejecting unconscious inference was the belief of the schools of both Hering and Wundt in the existence of immediate experience. The business of the experimental psychology of the late nineteenth century was, essentially, the determination of the relation between the stimulus and the phenomenal experience—in this case between the visual angle and the *Sehgrösse*, as Hering called it. In those days everyone believed that, since phenomenal experience is not the result of inference, apparent size and estimated size must, therefore, be different. In the present century the Gestalt psychologists find a distinction between these two concepts at a different level. No longer insistent that phenomenal data are meaningless and not interpretative, they hold that the perception nevertheless depends upon the operation of psychophysiological dynamical forces which determine size and other characteristics. These forces are not de-

rived from past experience, they believe, but arise out of the native constitution of the organism. They think, consequently, of phenomenal size as dependent upon some process in the brain which is isomorphic with the perception as to size. Estimated size would also depend upon a neural process, since inference must have a neural basis, but the genesis and form of the brain process would be different. It thus becomes plain that the distinction between apparent size and estimated size is still quite speculative, as it was when the issue existed between Hering and Helmholtz. Granting that some of this history still lies in the future, let us now turn to the past.

If anyone ever seemed to establish firmly the equivalence of visual angle to apparent size, it was Euclid, for in his *Optica* he dealt with the problem of size in just such a geometrical way. Naturally, then, he could point out that the nearer of equal magnitudes appears larger; but he also modified his strict geometry by noting that equal magnitudes at unequal distances do not appear in sizes inversely proportional to their distances. Thus it appears that, in moments of phenomenological insight, the distinction between apparent size and visual angle goes back to the very beginning.

✓ In the eighteenth century there arose what came much later to be called the alley problem. If one looks down the vista of an avenue of trees, the two rows of trees, actually parallel, appear to approach each other in the distance. Bouguer (ante 1758) discussed this problem at length. Porterfield in 1759 held that the phenomenon is due to the progressive underestimation of more and more remote distances. He assumed that, if the angle between a pair of trees on the two sides of the avenue diminished with the perceived distance, then the rows of trees would be seen as parallel; if, however, the distance is underestimated, then the angle between the pair seems too small for its apparent distance (since actually it belongs to a greater distance). Thus the rows of trees appear to converge. Priestley considered the problem further in 1772, pointing out that no philosopher had yet discovered how the trees of the avenue could be spaced so as to appear parallel and without convergence when viewed from a predetermined point at one end. If the eighteenth century had had railroads, the fact that apparent size follows neither a law of constancy nor of visual angle would have been even more apparent to its scientists.

Railroad tracks converge in the distance, but their separation does not diminish at the rate at which distance increases.

As happens so often in science, in the nineteenth century incompatible views existed contemporaneously. The size of the retinal image was regarded as the primary determinant of perceived size, with the optical illusions special cases requiring special explanations. This view, which worked well enough in bidimensional vision, where distance is constant, failed for changing distance because it overemphasized visual angle as the determinant of apparent size. The sophisticates who considered size in relation to distance all knew that proportionality of perceived size to retinal size does not occur exactly; nevertheless it took the definite campaign of Gestalt psychology to make this incompatibility clear to all. ✓

In 1842 H. Meyer observed that wall-paper patterns with repetitive designs can be viewed with different degrees of convergence. The eyes can be converged enough so that they combine adjacent figures in the pattern, or even more, so that figures next but one or next but two are combined. In such cases the figure is seen at the point upon which the eyes are converged—nearer, and, since it still subtends the same visual angle, smaller. The greater the convergence, the nearer and smaller the figure. Ludwig, the physiologist, made the converse observation in 1852: if one fixates a window, holding a pencil in the hand, and then shifts fixation to the pencil, the window gets smaller. Presumably also the pencil gets larger. Later Panum (1859) undertook to bring all these observations together. He cited Meyer's and Ludwig's observations; he noted that the image of a distant object, seen near on the screen of a *camera lucida*, appears much smaller than the object itself in the distance; he pointed out that the size of an after-image varies with the distance of the ground upon which it is projected, thus anticipating Emmert's law. That apparent size varies somehow with perceived distance, even when retinal size remains constant, was getting pretty clear.

Fechner and Hering also contributed to the discussion at this time. Fechner (1860) held two compass points, with a certain separation between them, a certain distance in front of his eyes. Then, when he held another compass with twice the separation between its points at twice the distance away, he found that the second pair of points had to be brought closer to each other in

order to give the same apparent separation (*Augenmass*) as the first. Hering (1861) observed the apparent size (*Sehgrösse*) of the window in his room as he paid attention to his own hand, held near and in front of his face. When he moved his hand away from his face toward the window, the window got larger; and conversely. The implication is that the object attended to tends to remain constant although its distance changes, and that other objects, therefore, having a fixed relative size in the visual pattern, may vary. Because the retinal size of the hand gets smaller while the retinal size of the window remains constant, the window relative to the hand gets larger as far as its retinal representation is concerned. If the apparent size of the hand shrinks less than the corresponding retinal image, then the whole field may remain proportioned in accordance with the relative retinal sizes; hence the window appears to get bigger because the hand does not appear to shrink as much as its retinal image.

It was not until 1881 that Emmert formulated that law of the geometry of after-images known by his name. If O is the linear size of an object, and d is its distance from the observer; and if N is the linear size of the after-image (*Nachbild*) of the object, and D the distance from the observer of the ground of projection of the after-image; then Emmert's law states:

$$O : N :: d : D$$

In other words, the apparent size of the after-image is directly proportional to its apparent distance from the observer. This law, although it deals with the inconstancy of apparent size when retinal size is constant, nevertheless does indirectly imply its converse, 'size constancy.' The principle of size constancy asserts, if one stops to think it through, that apparent size remains constant when retinal size diminishes in direct proportion to the distance of the object from the observer. Emmert's law states that apparent size increases when retinal size remains constant in direct proportion to the distance of the object from the observer. In other words, if the distance is doubled and the angle halved, the apparent size remains the same, i.e. is doubled in proportion to the angle (size constancy); and, if the distance is doubled and the angle constant, the apparent size is doubled, i.e. is doubled in proportion to the angle (Emmert's law). If both size constancy and Emmert's law were exactly true—as they are not—it can be shown that appar-

ent size would always be proportional to the product of the visual angle of the perceived object by its distance from the observer. (See p. 310.)

The first systematic experiments on this problem were made, not under Hering's influence as one might expect, but in Wundt's

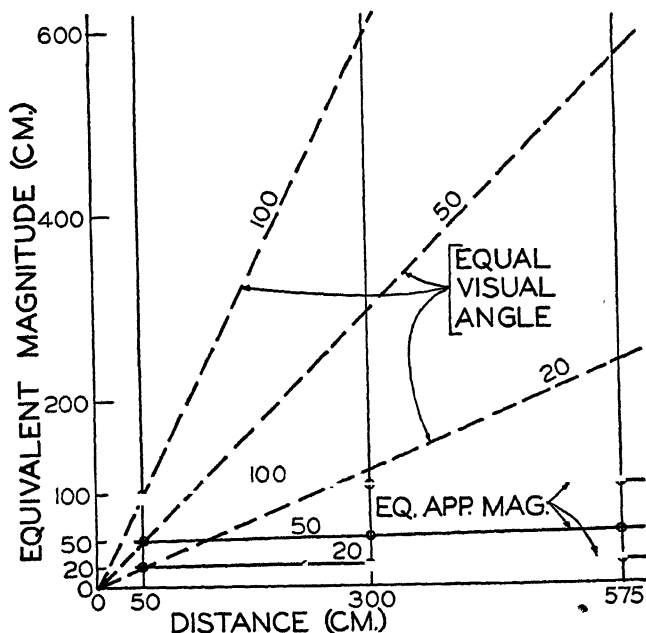


FIG. 45. SIZE CONSTANCY: MARTIUS' EXPERIMENT (1889)

The standard stimuli were rods 100, 50 and 20 cm. long, viewed at a distance of 50 cm. The lines for Equivalent Apparent Magnitude show the equivalent magnitudes at distances of 300 and 575 cm. That these lines are almost horizontal shows that size remained practically constant with changing distance. The dashed lines show how the magnitude of the stimulus would have to change with distance if equal apparent magnitudes required equal visual angles. These data are for Martius as observer.

laboratory by Martius in 1889. He had wooden rods, 20, 50 and 100 cm. in length, which he suspended, one at a time, as standard stimuli 50 cm. in front of the observer. Then at distances of 300 and 575 cm. he had the observer choose, under a definite psychophysical procedure, a rod of length equal to the standard. The results for Martius himself as observer are shown in Table III and in Fig. 45. There is practically phenomenal constancy. The 20-cm.

rod at 50 cm. was equated to a rod of 20.6 cm. at 300 cm. and to a rod of 21.7 at 575 cm. If the observer had been equating visual angles, these rods should have been 120 and 230 cm. long. Hence the data seem to prove size constancy. But at once the question arises as to whether there is a valid distinction between phenomenal size and estimated size. Might the more distant rods have

TABLE III. MARTIUS'S EXPERIMENT ON SIZE CONSTANCY (1889)

The data are for Martius as observer; the results from other observers are similar.

<i>Standard Size at 50 cm.</i>	<i>Equivalent size</i>			
	<i>As observed at 300 cm.</i>	<i>at 575 cm.</i>	<i>If visual angle constant</i>	
			<i>at 300 cm.</i>	<i>at 575 cm.</i>
20 cm.	20.6 cm.	21.7 cm.	120 cm.	230 cm.
50	53.9	57.6	300	575
100	107.7	106.6	600	1150

appeared smaller, and the observer, with his knowledge of how distance tends to diminish size, have been able to correct for the phenomenal change? Could he perhaps make this correction immediately? and how many seconds may elapse if the judgment is still to be called immediate? Questions of this sort Martius's experiment did not settle.

In 1902 Hillebrand, working under the influence of Hering, revived with his alley experiment the interest in the old problem of the vista. He seated his observer at the end of a table, 4 m. long and 1 m. wide, covered by white paper with the axis prominently marked down the middle. At the far end of the table the ends of two black threads were attached, equidistant from the axis and separated by distances varying from 39 cm. to 10 cm. The observer, who held the other ends of the threads, was asked to adjust them laterally until they appeared to be parallel. The results showed what the eighteenth century philosophers also knew, that the threads must diverge as they recede from the observer if they are to appear parallel: they must diverge enough to counteract the tendency of true parallels to appear to converge. Hillebrand also found that the divergence was greater for wider alleys than for smaller, but that it never approached the divergence of lines which give a constant visual angle—not within the limits with which he worked. Very narrow alleys approach the constant separation of true parallels, a result which might have been predicted, since a

very small separation at the far end gives little chance for convergence at the near end.

Though the use of threads presupposes that apparently parallel sides of an alley should be straight, that assumption is not neces-

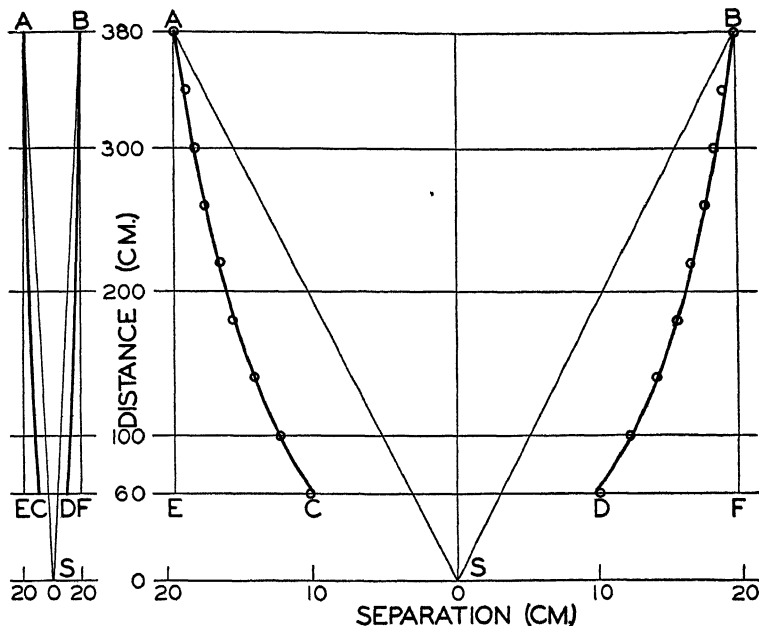


FIG. 46. SIZE CONSTANCY: HILLEBRAND'S ALLEY EXPERIMENT (1902)

AB = standard distance at 380 cm. from observer; it is here 39 cm., or 19.5 cm. on each side of the median line. The observer, at S , adjusts suspended threads to form two apparently parallel lines through A and B . AC , BD = loci of threads for apparent parallels. AE , BF = geometrical parallels. AS , BS = locus of what parallels would be if visual angle had to remain equal for parallels to seem always equidistant from each other. The figure at the left is drawn to scale and shows the shape of the alley with sides that appear parallel. The figure at the right is the same as the one at the left, except that the horizontal scale is magnified to show the curvature of the alley's sides. These data are for Hillebrand as observer.

sarily valid. Hillebrand, therefore, repeated his experiment with many threads suspended vertically at each side to form the alley walls. The separations between pairs of opposite threads could be adjusted, and the shape of the apparently parallel walls thus discovered. The results for Hillebrand himself as observer are shown in Fig. 46. The diagram at the left is drawn to scale: it shows the

true parallels drawn from the separation of 39 cm. at the far end of the alley, the visual angle that subtends 39 cm. at the far end, and the loci of the apparently parallel sides of the alley. The right of the figure is the same as the left except that the scale for the breadth of the alley has been increased ten-fold to show how the sides actually curve for apparently equal separations.

The alley experiment was repeated later by Poppelreuter (1911) with vertical rods fixed on simple bases, in such a way that they could be shoved in toward the median line or pulled out from it to make an actual vista. In general his results confirmed Hillebrand's: to appear as determining parallel vertical surfaces, the surfaces formed by the rods are not parallel, nor do they converge so much as the visual angle, nor are they planes. They are somewhat curved, lying intermediately between the locus of the constant angle and the locus of the constant separation (parallels).

Blumenfeld (1913) sought to eliminate the visual frame of reference for these judgments—the room, the rectangular table. He arranged tiny gas jets with faint blue flames that could be pushed in and out with respect to the median axis. The observer, in the dark except for these flames, looked between the two rows, or in other experiments looked somewhat down upon the two rows, and arranged the points of flame to form apparent parallels. The interesting thing about this experiment is that the locus of the apparent parallels, although it lies intermediate between the locus of true parallels and the locus of constant visual angle, is very much nearer the constant visual angle than was the case for Hillebrand or Poppelreuter. In other words, this is, in Katz's sense, a reduction experiment. The perceptual cues are reduced in the dark more nearly to bare unassisted retinal size. It seems obvious that if all cues to distance could be removed and the points that determine the alley perceived at entirely indeterminate distances, then visual angle, the only remaining determinant of size, would control, and lines diverging from the observer to give a constant angle would seem everywhere equidistant.

The recent history of this problem has been partly factual and partly systematic. Let us turn to the factual contribution first.

That chimpanzees can perceive objective size independently of retinal size was demonstrated by Köhler in 1915. Götz showed in 1926 that the same principle holds for chickens. He trained the chickens to take larger grains of corn and to reject smaller, when

the linear sizes of the larger to the smaller grains had a ratio as small as 1.1:1.0; that is to say, the chickens were perceiving a difference in size of 10 per cent. When the small grains were placed only 15 cm. away from them and the large grains as much as 73 cm. away, the chickens still singled out the larger grains, choosing in the larger object a visual angle that was about one-fifth the size of the visual angle for the smaller but nearer object. These results have been interpreted as meaning that the chickens made the choice on the basis of phenomenal size rather than estimated size, yet that conclusion would be valid only if an estimate is an elaborate inferential process, which a chicken would not be likely to employ. On the other hand, if by *estimate* is meant a practical judgment of objects as things and not an 'introspective' judgment of immediate experience, then the chickens were probably making 'estimates.' It can be said that, in Helmholtz's terms, an inference (*Schluss*) was required of the chickens, and we may assume that in a chicken such an 'inference' would be unconscious (*unbewusst*).

Beryl, publishing also in 1926, had adults and children equate the sizes of boxes and also of disks at different distances, ranging from 1 to 11 m. The adults showed almost exact constancy in their judgments of objective size, but the children exhibited a tendency to need a larger remote object as a match for the smaller near object. For instance, children two years old matched a 7-cm. box at 1 m. to an 11-cm. box at 11 m., whereas children six years old showed a little more constancy, matching the 7-cm. box at 1 m., to 9-cm. box at 11 m. Such results do not show size constancy, although of course they also do not come anywhere near constancy of visual angle. The experiment makes it seem, however, that learning or maturation has something to do with this approach toward constancy.

Both Thouless (1932) and Sheehan (1938) have studied the occurrence of individual differences in this function. Thouless found the degree of constancy to vary with training and with intelligence, and hence sometimes with age. Sheehan found that it was dependent upon a number of objective and subjective factors. The amount of visible details makes a difference, as does also the deviation of the object from normal appearance. The introduction of misleading contexts naturally works against constancy, just as the deliberate use of cues to objective size works for it. Suggestion,

contrast and attitudinal sets all affect the function. Such results imply that the phenomenon is not a simple resultant of a few physiological forces, but the resolution of a complex process which, even though it be unconscious, is fully as involved as an estimate or inference.

Holaday (1933), working under the stimulus of Brunswik in Vienna, has produced the best general analysis of the conditions that underlie this 'constancy phenomenon.' The greatest constancy, he showed, is achieved by trying to achieve it; in short, the observer does best when he takes an active objective attitude of common sense, betting that one pair of differently distant objects is more nearly the same size than another pair. Such an attitude works best when there are provided cues for its use. Holaday found that constancy was more nearly achieved when cues to the distances were provided by placing many objects in the intervening space, and conversely that the falsification of the impression of distance reduced constancy. Thus we come back essentially to Bishop Berkeley's position (1709) that the perception of the size of an object depends on the perception of the distance to the object.

On the systematic side there have been two successive movements: the first, by the Gestalt psychologists, to show that size, when distance varies, does not change with the visual angle but tends to remain constant; the second, by Brunswik and others, to inventory, and appraise the various forces which determine apparent size at values intermediate between retinal size and the constant size of an object.

Gestalt psychology, like all vigorous movements—the author must be forgiven for his reiteration of this statement—began as a protest against an established order in systematic psychology. In this order, as we have seen, there was imbedded the prevailing notion that apparent size is determined by retinal size, a belief which had persisted in spite of the fact that it was definitely known, as we have also seen, not to be true when the distance of the stimulus object varies. The Gestalt psychologists were determined to show that there is no fixed relation between the stimulus and the perception, since the properties of the perception depend as much upon the operation of dynamical forces in the central nervous system as upon the stimulus. If one has been thinking that apparent size varies with visual angle and then suddenly notes that the man 10

feet away in the hall is not nearly twice so large as the man 20 feet away (or if one merely chances upon Martius's experimental results), then one is struck by the fact that size is practically constant when distance changes—practically constant as compared with what would happen if size changed with visual angle. It was, therefore, a pardonable hyperbole for the Gestalt psychologists to speak of "size constancy" as if the appearance of objects did not shrink with distance—this in spite of the actual fact that apparent size does usually diminish with increasing distance, although much less rapidly than the visual angle.

Thouless (1931) and Brunswik (1933) both gave formulas for measuring this tendency away from the stimulus value and toward constancy. Thouless spoke of the phenomenon as "regression to [toward] the real object." Brunswik wrote about the transformation to the intermediate object (*Zwischengegenstand*). Both men pictured the organism as correcting the immediate impression of the stimulus in the direction of objective constancy and thus effecting a compromise between the two. Their formulas measured the weight of each component in the resultant.

As a matter of fact, Brunswik, with Holaday and his other associates, has changed the modern form of this problem. Brunswik pictures the organism as trying to perceive objects which are only more or less accessible to it; it bases its perception upon all the data available. Apparent distance is thus, as Holaday showed, one of the determinants of apparent size. With enough cues the *Zwischengegenstand* may approximate the real object, but the organism can nevertheless satisfy its own needs sufficiently without fully achieving its intentions. This point of view, born in the functionalism of Vienna, is so close to the functionalism of America, the psychology that examines mental functions in respect of their use to the organism, that it clarifies the problem of size and distance to a great number of modern psychologists.

Tridimensional Dynamics

It is to Gestalt psychology that the modern psychologist turns for his systematic framework when he deals with perception. Thus it is not surprising today to find depth and distance regarded simply as a dimension of a perception organized under dynamical laws in a tridimensional field. There are really five points which

the Gestalt school, speaking here most explicitly with the voice of Koffka, makes about the perception of the third dimension. They are these.

(1) Visual perception is essentially tridimensional. It does not copy the retinal image, not even to the extent of being, like the retinal image, bidimensional. Instead, it approximately reconstitutes the stimulus-object; and stimulus-objects, like everything else in the material world, are solid. The tridimensionality of the visual field becomes especially obvious, however, in those cases where a flat stimulus yields a solid perception, and Koffka has

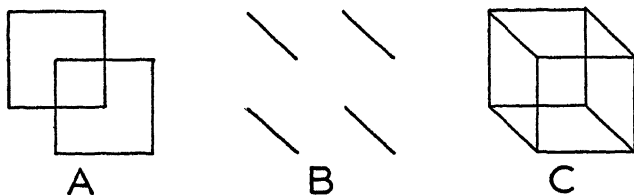


FIG. 47. DYNAMICS OF THE TRIDIMENSIONAL VISUAL FIELD

The plane stimuli, A and B, yielding each a bidimensional visual form, give, when added together in the plane stimulus C, a tridimensional visual form. It is practically impossible to perceive C as what it is, 12 lines lying in the plane of the paper.

listed a number of these instances. (a) Fig. 47A is a flat stimulus which gives a flat perception. Fig 47B yields perception of four lines in a plane. But, when the lines of these two figures are combined in Fig. 47C, solidity at once results. It is practically impossible to see the cube as twelve lines lying flat in a plane. (b) When the lines *a* and *a'* in Fig. 48A alternate tachistoscopically, the line *b* being absent, one gets apparent movement of a line back and forth between positions *a* and *a'*. Then, if the line *b* is introduced as a constant, the fluctuating line seems to pass under *b* between *a* and *a'*—to be forced out of the plane, as it were. (c) If the angles *aba* and *ab'a* in Fig. 48B alternate tachistoscopically, the perceived movement is the movement of an angle rotating in the third dimension about the pivots *aa*. This is the simplest movement that could bring the angle from position *aba* to position *ab'a*. (d) If blue vanes of an episcotister whirl in front of a yellow disk on a black background and one views a part of the field through a 'reduction' tube, then there is color mixture between the blue and

the yellow: if the blue vanes are made the right size, one may even see a gray. But without the tube one sees, not a mixture, but the yellow through a transparent blue: both colors are seen in the same direction at different distances, a case of what Koffka has called "double representation."

Here, of course, the stimulus is not flat, but gives a flat perception when the colors mix, and a perception of depth when they do not.

(2) The second point is that such excursions of the perception into the third dimension are due to the operation of dynamical principles of organization exactly like the principles that operate in two-dimensional space. The perception of the cube tends toward symmetry (Koffka), and thus tends to reconstitute the object (Brunswik). Because movement tends toward least action, the angles flop over in space instead of twisting around and moving up and down, as they would have to do if they remained in the plane. The line moves from one position to the other

through the plane when there is no hindrance, but, when a barrier is introduced between the two positions, it moves around the barrier and thus out of the plane. When the blue-vaned episcotister and the yellow disk are seen entire, their integrity as objects localized at different distances prevails, and the yellow is seen through the blue and at a different distance. With the reduction tube to eliminate the object-character of the two things, however, the simplest event occurs: they fuse. This sort of speculation about the forces which determine perceptual form is called *field theory*. The field, the matrix of perception, is obviously tridimensional, and Gestalt psychologists also infer a tridimensional neural process.

(3) Stereoscopic vision thus becomes simply a special case of field theory, and the key to an understanding of it is the fact that stereoscopic vision is a process of the formation of objects. If the left eye views the stereogram A in Fig. 49 and the right eye the

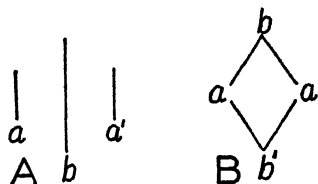


FIG. 48. DYNAMICS OF THE TRIDIMENSIONAL VISUAL FIELD

A: Tachistoscopic alternation of a and a' without b makes it appear that a line moves back and forth between positions a and a' in the plane of a and a' , but with the introduction of b the line appears to move out of the plane behind b .

B: Tachistoscopic alternation of aba and $ab'a$ does not result in the apparent movement of an angle between the two positions in the same plane, but in an apparent rotation of the angle out of the plane about the pivots aa .

stereogram B, then the combined field would be what is shown at C with bb' double. One does not, however, ordinarily see bb' double, but rather a single line a in front of a single line b . There is an attraction between b and b' , and they form a single line. Why do they pull together? Because they are near? Yes. Because they are both black? No. Koffka has cited Helmholtz's observation that

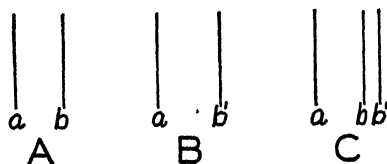


FIG. 49. DYNAMICS OF THE TRIDIMENSIONAL VISUAL FIELD

A: stereogram for left eye. B: stereogram for right eye. C: superposition of A and B, the way the binocular field would look if the law of corresponding points were absolute. Actually b and b' attract each other, and one sees a single line b farther away than a .

a black stereogram on a white ground can combine to give depth with a slightly disparate white stereogram on a black ground. They pull together because in that way they reconstitute a simple objective organization of two lines at different distances. It is not the color of the lines, but the fact that they are *lines*, objects, that makes the black line b combine with the black line b' instead of with the white ground near b' , a ground which is in

the position corresponding to the position of b , but which is not an object. Koffka has given examples of how these forces of combination operate in the binocular field, always in the interest of the formation of objects; it is, indeed, well known that the stereoscopic constitution of genuine objects and scenes is much easier than the combination of the geometrical skeletons of objects that are used for experimental demonstrations.

(4) Convergence and accommodation in this theory lose much of their importance. They remain, of course, optical mechanisms necessary for obtaining clear retinal images of objects, just as the retinal image is still basic to visual perception in spite of all the depreciation it has received. Do convergence and accommodation, then, not enter into the distance of field theory at all? Not much. Schur has suggested that the shrinkage of the horizon moon as it rises toward the zenith might be due to the muscular effort of elevating the eyes, but that is merely speculation. Although we know that convergence (Meyer's experiment) alters perceived distance and size, that result might come from the accompanying alteration of retinal disparity. It is certainly true that these two

mechanisms for the perception of distance have interested psychologists less in the 1930's than in any one of the preceding ten decades.

(5) Field theory is bolstered up by the theory of isomorphism. Köhler's axiom that "all experienced order in space is a true representation of a corresponding order in the underlying dynamical context of physiological processes" and that there is an organization in a brain field corresponding to the organization of the perceptual field, has been extended to all three dimensions of perceptual space. If the perception of a closed figure means that there is some kind of closed system of excitation in the brain, then the perception of a solid figure means that the excitation in the brain must be extended into new dimensions. This theory, which is still speculative, is not essential to field theory; yet there can be no doubt that it has given great support to field theory in that it provides a concrete place—the brain—where the field forces can be supposed to operate.

In this matter of the dynamics of perception Brunswik has gone somewhat beyond the Gestalt school. The Gestalt psychologists seek the laws of object-formation ostensibly in the perceptual field, but also—by the implication of isomorphism—in the brain. Brunswik, with a less physiological and more biological approach, ignores the brain and shows how the organism 'tries' to reconstitute the objects from the somewhat insufficient sensory information that it receives from them. The Gestalt theory is one of dynamics; Brunswik's is one of functional use, a view which is, in fact, a modern equivalent of *unbewusster Schluss*.

Notes

On the perception of the visual third dimension in general, see H. A. Carr, *An Introduction to Space Perception*, 1935, 159–288; R. S. Woodworth, *Experimental Psychology*, 1938, 651–680. Carr gives the more orthodox presentation; Woodworth gives the modern view in which convergence and accommodation become secondary to other visual factors. On the other hand, Woodworth goes a little into the history and quotes Leonardo at length. Since a history cannot also be a modern

handbook, the reader is referred to these sources for most of what happened in the present century.

Secondary Criteria of Distance

On the history of the use of perspective in art, see the histories of art, for example, R. V. Cole, *Perspective*, 1921, 216–242, esp. 216–224.

Leonardo da Vinci, one of the greatest geniuses the world has

known, brought his facile mind to bear upon the problems of the technique of painting. While he probably never published his projected treatise on painting, he left at his death in 1519 many notes which have been combined into a *Trattato della Pittura* and published in various editions and translations from 1651 to 1939. The latest volumes add new items from the manuscripts, items that have never been published before: J. P. Richter and I. A. Richter, *The Literary Works of Leonardo da Vinci*, 2 ed., 1939, 2 vols. In Vol. I are reprinted in Italian and Eng. trans. 700 notes—dicta, precepts, observations, technics, sketches, plans—which constitute the modern and enlarged *Treatise on Painting*.

Leonardo, coming to power as he did at the height of the naturalistic movement in art, lent his observational powers and his representative techniques to support that movement. Perspective had been 'discovered' again by the Italians, and his discussion of linear perspective (Richter edition, 1939, I, 125-161) is important in showing how fully its principles were then understood and accepted by the artists of that time.

Leonardo's discussion of the use of light and shade in painting is classical. The Richters give what has been found in the manuscripts of Leonardo's six books on light and shade (I, 162-207, 213 f.). These sections indicate the extent of knowledge at the beginning of the 16th century—extent rather than discovery, for the fundamental principles must have been established long before.

On the reversal of relief, see D. Rittenhouse, Explanation of an optical deception, *Trans. Amer. phil. Soc.*, 2, 1786, 37-42; J. J. Oppel, Ueber ein Anaglyptoskop, *Ann. Phys. Chem.*, 175, 1856, 466-469; H. Schröder, Ueber eine optische Inversion bei Betrachtung verkehrter, durch optische Vorrichtung entworfen,

physischer Bilder, *ibid.*, 181, 1858, 298-311. For a modern discussion of light and shade in the determination of visually perceived depth, see W. Metzger, *Gesetze des Sehens*, 1936, Chaps. 6 and 9. Most psychologists know the photograph of the turret of the monitor *Lehigh*, dented after having been used as a target. The depth, given solely by light and shade, is reversed (dents become bulges), if the photograph is turned upside down or if the direction of the light is reversed in imagination from downward to upward. If both imagination and photograph are reversed together, the depth remains, of course, the same.

For Leonardo on aerial perspective, see the Richter edition of 1939, *op. cit.*, I, 210-212, 234-241 (for the quotation, 236 f.). For G. Berkeley on the moon illusion, see his *An Essay towards a New Theory of Vision*, 1709, sects. 67-77. For H. v. Helmholtz's discussion of the problem, see his *Physiological Optics*, 1866, Eng. trans., 1925, III, 360-362, where also is to be found a statement of Ptolemy's theory of filled and empty distance.

Helmholtz's discussion of the parallax of movement occurs in the *Physiological Optics* [1866], Eng. trans., 1925, III, 295-297. He mentions a brief note on the subject by H. W. Dove, Ueber eine optische Täuschung bei dem Fahren auf der Eisenbahn, *Ann. Phys. Chem.*, 147, 1847, 118; the increase of speed of locomotion that came with the invention of the railroad must have made the criterion more obvious. Although the author does not know where early references to this principle occur, it is impossible that all the natural philosophers were unaware of it. The principle is obvious to the habitual observer, but one does not always put the obvious into print.

On the reversible perspectives, see L. A. Necker, Observations on some

remarkable phaenomena seen in Switzerland; and an optical phaenomenon which occurs on viewing of a crystal or geometrical solid, *Phil. Mag.*, 3 ser., 1, 1832, 329-337, esp. 336 f.; Schröder, *op. cit.*, 311 and Plate III; W. J. Sinsteden, Ueber ein neues pseudoskopisches Bewegungsphaenomen, *Ann. Phys. Chem.*, 187, 1860, 336-339; Helmholtz, *op. cit.* (Eng.), III, 285 f. It is, of course, probable that the perspective reversal of windmills is often observed where windmills are numerous. Although Sinsteden and Helmholtz did not know it, the phenomenon had been described by W. Porterfield, *A Treatise on the Eye*, 1759, II, 384. On the fluctuations of reversible perspectives and their possible relation to the fluctuations of 'attention,' see N. Lange, Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, *Phil. Stud.*, 4, 1888, 390-422, esp. 405-412; K. Marbe, Die Schwankungen der Gesichtsempfindungen, *ibid.*, 8, 1893, 615-637, esp. 616 f., 635 f.

Convergence and Accommodation

For the history of the theories of the roles of accommodation and convergence in the visual perception of distance, see J. W. Baird, The influence of accommodation and convergence upon the perception of depth, *Amer. J. Psychol.*, 14, 1903, 150-200, esp. 151-170.

For F. Aguilonius on the horopter, see his *Opticorum libri sex philosophis juxta ac mathematicis utiles*, 1613, II, 146-150.

For R. Descartes on convergence and accommodation as the means of the perception of distance and the pictures of a blind person feeling out a distance with staves, see his *La dioptrique*, 1637, chap. 6 (first part); *De homine*, 1664, paragraphs 18-21, pp. 43-65, or *L'homme*, 1662,

articles 37-51, pp. 37-55. It is in *De homine*, p. 64, and *L'homme*, p. 54, that Descartes says that accommodation is effective up to distances of 3 or 4 feet and convergence up to distances of 15 or 20 feet. J. Priestley, *History and Present State of Discoveries Relating to Vision, Light and Colours*, 1772, 115, cites this passage and says that Descartes elsewhere assigned an effective distance of 100 to 200 feet for convergence.

G. Berkeley's discussion of convergence as a cue to the perception of distance is in his *An Essay towards a New Theory of Vision*, 1709, sects. 16-20. For Berkeley on accommodation, see sects. 21-29.

E. B. Titchener's context theory of meaning was formulated in the interests of the problem of thought but necessarily applied from the first also to perception. See his *Lectures on the Experimental Psychology of the Thought-Processes*, 1909, 174-180; *A Text-Book of Psychology*, 1910, 367-371.

For histories and classifications of theories of accommodation, see J. Muller, *Handbuch der Physiologie des Menschen*, 1838, Bk. V, sect. i, Chap. 2, iii (Eng. trans., II, 1136-1150); and esp. H. v. Helmholtz, *Handbuch der physiologischen Optik*, I, 1856, sect. 12 (Eng. trans., I, 158-168). The text mentions only a few of the more important papers.

For Scheiner's experiment, see C. Scheiner, *Oculus*, 1619, Bk. III, 163, [n.v.]. The author has not seen the original; this and seven of the 'n.v.' references below are taken from Helmholtz, who is not always accurate in his citations.

On the theory that there is no mechanism of accommodation, see P. de la Hire, *Dissertation sur la conformation de l'oeil*, *J. des Sçavans*, 1685, 196-202, 219-224; F. Magendie, *Précis élémentaire de physiologie*, 1816, I, 73, [n.v.], (in the 4 ed., 1836, I, 64 f., 74-76); G. R.

Treviranus, *Beiträge zur Anatomie und Physiologie der Sinneswerkzeuge des Menschen und der Thiere*, 1828, I, 46-52.

On the theory that pupillary contraction is the (or a) mechanism of accommodation, see Scheiner, *op. cit.*, 1619, III, 31, [n.v.]; A. v. Haller, *Elementa physiologiae corporis humani*, V, 1763, 516-518; F. C. Donders, in C. G. T. Reute, *Leerboek der Ophthalmologie*, 1846, 110, [n.v.].

On the elongation of the eye as a mechanism of accommodation, see J. C. Sturm, *Dissertatio de presbyopia et myopia*, 1697, [n.v.]; J. B. Listing, *Dioptrik des Auges*, in R. Wagner, *Handwörterbuch der Physiologie*, 1853, IV, 451-508, esp. 498-500; and, against the theory, T. Young, On the mechanism of the eye, *Phil. Trans.*, 91, 1801, 23-88; or G. Peacock, *Miscellaneous Works of the Late Thomas Young*, 1855, I, 12-63. For Helmholtz on Young, see Helmholtz, *loc. cit.*, Eng. trans., 158 f., 167.

As to whether a change of curvature of the cornea mediates accommodation, see J. P. Lobé (Albinus), *Dissertatio de oculo humano*, 1742, 119, [n.v.]; E. Home, The Croonian Lecture on muscular motion, *Phil. Trans.*, 1795, I, 1-23. For a vigorous modern objection to the Helmholtz theory, and the contention that the external muscles of the eye accommodate vision by elongation of the axis or increasing the curvature of the cornea, see W. H. Bates, *The Cure of Imperfect Sight by Treatment without Glasses*, 1920.

That the lens shifts back and forth in accommodation was the theory of J. Kepler, *Dioptrice*, 1611, proposition 26; Scheiner, *op. cit.*, 1619, III, 163, [n.v.]; J. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und Thiere*, 209-216, esp. 212; C. A. Burow, *Beiträge zur Physiologie und Physik des menschlichen Auges*, 1841, 94-177;

and at least half a dozen others in this period.

For the theory that the lens changes shape in accommodation, see Descartes, 1637, *loc. cit.*; J. Hunter as reported by E. Home, Some facts relative to the late Mr. John Hunter's preparation for the Croonian Lecture, *Phil. Trans.*, 1794, I, 21-27; T. Young, 1801, *loc. cit.*; J. E. Purkinje, *Beobachtungen und Versuche zur Physiologie der Sinne*, II, 1825, 128; Helmholtz, Ueber eine bisher unbekannte Veränderung am menschlichen Auge bei veränderter Accommodation, *Monatsber. preuss. Akad. Wiss.*, 1853, 137-139 (or *Wissenschaftliche Abhandlungen*, II, 1883, 280-282); Ueber die Accommodation des Auges, *Arch. Ophthal. Berlin*, I (2), 1856, 1-74 (or *Wiss. Abhandl.*, II, 1883, 283-345); *Handbuch der physiologischen Optik*, I, 1856, sect. 12 (Eng. trans., I, 143-166).

On Sanson's images—the "reflex images" as they were first called—see Purkinje, *De examine physiologico organi visus et systematis cutanei*, 1823, 21-29, who actually described and pictured four images of a candle flame, the images from the front and back surfaces of the cornea and from the front and back surfaces of the lens, L. J. Sanson, *Leçons sur les maladies des yeux*, 1838, 28-30; M. A. Langenbeck, *Klinische Beiträge aus dem Gebiete der Chirurgie und Ophthalmologie*, II, 1850, 105-107; A. Cramer, whose first publication on the images in Dutch is *Mededeelingen uit het gebied der ophthalmologie (over den stand der iris)*, *Tijdschr. d. Nederland. maatschapp. tot bevord. d. geneeskunst*, 2, Pt. 2, 1851, 99-119, but whose full discussion of the matter is in his Dutch text of 1853, Chap. 3, so see the German trans., *Physiologische Abhandlung über das Accommodations-Vermögen der Augen*, 1855, Chap. 3, 27-43; Helmholtz, *loc. cit.* Bates, *op. cit.*, 54,

said that the image from the front of the lens is not clear enough to establish the theory of Cramer and Helmholtz.

For W. Wundt's experiments on the perception of distance, see his *Beiträge zur Theorie der Sinneswahrnehmung*, 1862, 105-134, esp. 105-118, and 182-199. These two portions of the *Beiträge* were originally published in 1859 and 1861 respectively.

F. Hillebrand's papers are *Das Verhältnis von Accommodation und Konvergenz zur Tiefenlokalisation*, *Z. Psychol.*, 7, 1894, 97-151; In Sachen der optischen Tiefenlokalisation, *ibid.*, 16, 1897, 71-151. The main controversial issues of that time tended to divide German experimental psychologists into two groups: Helmholtz, Wundt, the Leipzig laboratory, the *Phil. Stud.*, empiricism on one side; and Hering, Stumpf, the *Z. Psychol.*, nativism and phenomenology on the other. Hillebrand in these papers was Hering's mouthpiece. Wundt replied through M. Arrer, *Ueber die Bedeutung der Konvergenz- und Accommodationsbewegungen für Tiefenwahrnehmungen*, *Phil. Stud.*, 13, 1896-1897, 116-161, 222-304. Hillebrand also excited an experimental paper in England, E. T. Dixon, *On the relation of accommodation and convergence to our sense of depth*, *Mind*, n.s. 4, 1895, 195-212.

Another reason why Hillebrand disbelieved in the effectiveness of accommodation for the perception of depth was his discovery that an Aubert diaphragm, which was made to approach the observer while its aperture was rapidly diminished in size, was perceived as receding although really approaching. Actually the experiment shows, however, merely that change of size can be more effective than change of accommodation.

For J. W. Baird's experiment and the fuller history of this research, see his *The influence of accommoda-*

tion and convergence upon the perception of depth, *Amer. J. Psychol.*, 14, 1903, 150-200. Observer B in Baird's experiment was Madison Bentley. The averages of Table II show significant differences, since the individual cases do not scatter widely. For a later repetition of Wundt's and Hillebrand's experiments with improved technique, see J. Bappert, *Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe*, *Z. Psychol.*, 90, 1922, 167-203.

Stereoscopy

On the history of the stereoscope, see Sir David Brewster, *The Stereoscope, Its History, Theory, and Construction*, 1856, 5-37. This book also exhibits the importance of the stereoscope for the public in the midst of the century that was noted for displaying the wonders of science. On the kinds of stereoscopes, see 53-75, 107-130; but the familiar hand stereoscope with a hood was invented later by Oliver Wendell Holmes, *Sun-painting and sun-sculpture*, *Atlantic Monthly*, 8, 1861, 13-29, esp. 29, with which cf. E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 269. Holmes' article also envisages a great future for the stereoscope, a future in which, form having been divorced from matter, the destruction of the Pantheon would not be a great loss because any one could go to a stereoscopic library and view it again in all its original corporeality! On the history, see also H. v. Helmholtz, *Handbuch der physiologischen Optik*, III, 1866, sect. 30 (Eng. trans., III, 300-312, 350-369).

On binocular parallax and disparity of images as understood by the ancients, see Euclid, *Optica et Catoptrica*, theorems 23-28 (the 1577 ed. gives both the Greek and a Latin trans.); C. Galen, *De usu partium*

corporis humani, Bk. 10, Chap. 12 (pp. 592 f. of the 1550 ed.).

Leonardo da Vinci's paradox of the opaque object that is binocularly transparent is given in most collections of his notes on painting, but is omitted from the Richter edition, *op. cit.*, 1939. See, for example, sect. 348 of the J. F. Rigaud Eng. trans. of Leonardo, *A Treatise on Painting*, 1802 *et seq.*, which is based on the Italian collection by R. du Fresne, 1651. Closely related to the passage in question is another which is not so dramatically put: in the Rigaud trans., sect. 124; in the 1939 Richter ed., I, item 28.

On the combining of double images outside the horopter by the *sensus communis* (Aristotle's concept of the perceptive faculty which combines disparate sensations), see F. Agulonius, *Opticorum libri sex philosophis juxta ac mathematicis utiles*, 1613, Bk. II, sect. 38, pp. 140 f.

For an early statement that disparity of images may yield relief, see J. Harris, *A Treatise of Opticks*, 1775, 171.

The first but brief mention of C. Wheatstone's discovery of stereoscopy is by H. Mayo, *Outlines of Human Physiology*, 3 ed., 1833, 288, but Wheatstone's own and full description is his *Contributions to the physiology of vision*, On some remarkable, and hitherto unobserved, phenomena of binocular vision, I, *Phil. Trans.*, Pt. I, 1838, 371-394; and *idem*, II, *Phil. Mag.*, ser. 4, 3, 1852, 504-523. The dates become important because James Elliot is said to have thought of the same instrument in 1834 and to have made one in 1839 (see Brewster, *op. cit.*, 18-22; but Brewster is not quite fair to Wheatstone, having himself had a controversy with him, for which see 22-36). Wheatstone described and pictured his reflecting stereoscope in 1838; he described a refracting stereoscope and also the pseudoscope in 1852. He also mentioned in 1852 that

photographic stereograms (calotypes or talbotypes) were prepared for the stereoscope by W. H. F. Talbot as early as 1839.

For H. Helmholtz's invention of the telestereoscope, see his *Das Stereoskop*, *Ann. Phys. Chem.*, 178, 1857, 167-175 (*Wissenschaftliche Abhandlungen*, II, 484-491; and Eng. trans., *Phil. Mag.*, ser. 4, 15, 1857, 19-24).

For E. Brücke's notion of the perception of solidity with a shifting horopter, see his *Ueber die stereoskopischen Erscheinungen und Wheatstone's Angriff auf die Lehre von den identischen Stellen der Netzhäute*, *Arch. Anat. Physiol. Lpz.*, 1841, 459-476; and for H. A. Dove's annihilation of Brücke's theory see his *Die Combination der Eindrücke beider Ohren und beider Augen zu einem Eindruck*, *Ber. preuss. Akad. Wiss.*, 1841, 251 f. For the relation of all this to nativism, a theory of stereoscopy and the facts of rivalry and binocular mixture, see P. L. Panum, *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1858, esp. 63-88.

A modern analysis of the resolution of conflicting cues to distance (e.g., linear perspective and shading may together overcome the normal effect of binocular parallax) is W. Schrieffer, *Experimentelle Studien über stereoskopisches Sehen*, *Z. Psychol.*, 96, 1925, 113-170.

Size and Distance

On the early history of the relation of size to distance and also of the alley problem, see W. Blumenfeld, *Untersuchungen über die scheinbare Grösse in Sehraume*, *Z. Psychol.*, 65, 1913, 241-404, esp. 243-274; on the later history of size constancy, see M. R. Sheehan, *A study of individual consistency in phenomenal constancy*, *Arch. Psychol.* N.Y., no. 222, 1938, 14-19, esp. 14-16; and also the general discussion of researches by K. Koffka, *Principles*

of *Gestalt Psychology*, 1935, 211-264, and esp. 87-95, 235-240.

For E. Hering's distinction of the three kinds of sizes, see his discussion in L. Hermann's *Lehrbuch der Physiologie*, III, i, 1879, 541 f. Hering said *scheinbare Grösse* for visual angle or retinal size, *Sehgrösse* for phenomenal or apparent size, and *geschätzte oder gedachte Grösse* for estimated size. In current German writing *scheinbare Grösse* can be translated correctly as apparent size, and *Sehgrösse* as phenomenal size.

On Euclid's distinction between apparent size and visual angle, see J. Hirschberg, *Die Optik der alten Griechen*, *Z. Psychol.*, 16, 1898, 321-351, esp. 326 f.

On the converging vista in the avenue of trees, see W. Porterfield, *A Treatise on the Eye*, 1759, II, 381-384; J. Priestley, *The History and Present State of Discoveries Relating to Vision, Light and Colours*, 1772, 700-704. It is Priestley who discusses at length the views of the prolific P. Bouguer (1698-1758) on this matter, but he gives no reference and the argument is not in Bouguer's *Essai d'optique*, 1729, or his *Traité d'optique*, 1760.

For the middle nineteenth century observations on the relation of apparent size to apparent distance as convergence or accommodation varies, see H. Meyer, *Ueber einige Täuschungen in der Entfernung und Grösse der Gesichtsobjekte*, *Arch. physiol. Heilk.*, 1, 1842, 316-326, esp. 316 f.; *Ueber die Schätzung der Grösse und der Entfernung der Gesichtsobjekt aus der Konvergenz der Augenaxen*, *Ann. Phys. Chem.*, 161, 1852, 198-207; C. Ludwig, *Lehrbuch der Physiologie des Menschen*, I, 1852, 252 f.; P. L. Panum, *Die scheinbare Grösse der gesehenen Objekte*, *Arch. Ophthalm. Berlin*, 5, 1859, 1-36, esp. 14-36; G. T. Fechner, *Elemente der Psychophysik*, II, 1860, 311-313; E. Hering, *Beiträge zur Physiologie*, 1861, 13-16. E. C.

Sanford, *A Course in Experimental Psychology*, 1898, 273, notes, as a variant of Meyer's observation with wall-paper patterns, that one can hold up a cane-seated chair, converge the eyes so as to combine other than identical units of the repetitive pattern of the seat, and thus perceive the seat as bulging out in front of its frame (cf. E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 262; also R. S. Woodworth, *Experimental Psychology*, 1938, 676). A modern study that confirms these early experiments in more systematic form is H. Frank, *Ueber den Einfluss inadäquater Konvergenz und Akkommodation auf die Sehgrösse*, *Psychol. Forsch.*, 13, 1930, 135-144.

The text makes no mention of the Aubert-Foerster phenomenon, the fact that the total spread of that field of vision (measured in visual angle) whose boundaries are the limits of any given degree of acuity (measured also in visual angle), is less for a far field than a near; that is to say, the separation of two objects by one minute of arc can be distinguished farther out from the center of the retina when the objects are near than when they are far. Such a rule is not inconsistent with the principle of size-constancy. The law of size-constancy implies that at greater distances less of the retina functions for the same apparent size. The Aubert-Foerster law implies that, in spite of size-constancy, the total field of vision (for any degree of acuity) does not increase with distance, nor does the resolving power of the retina (in visual angle) increase with distance. The consequence is that an object on receding should lose some of its detail, although it appears to remain the same size and to occupy the same proportion of the total field. See H. Aubert and R. Foerster, *Ueber den Raumsinn der Netzhaut*, *Jber. schles. Ges. vaterl. Kult.*, 43, 1856, 33 f. (brief but first); *Untersuchungen über den Raumsinn der Retina*,

Arch. Ophthalm. Berlin, 3 (2), 1857, 1-37 (the main exposition); E. R. Jaensch, *Zur Analyse der Gesichtswahrnehmungen*, *Z. Psychol.*, Ergbd. 4, 1909 (practically all about the phenomenon); E. Freeman, *Untersuchungen über das indirekte Sehen*, *Psychol. Forsch.*, 14, 1931, 332-379 (historical account, 339-341).

On Emmert's law, see E. Emmert, *Größenverhältnisse der Nachbilder*, *Klin. Mbl. Augenheilk.*, 19, 1881, 443-450. The relation of Emmert's law to size constancy is as follows. Let s = perceived linear size; r = retinal size; d = distance. Then:

$$s \sim d, \text{ if } r = \text{const.}, \quad (1)$$

which is Emmert's law. Moreover,

$$s \sim r, \text{ if } d = \text{const.}, \quad (2)$$

a statement which is implicit in the definition of s , since s is measured by the actual size of a comparison stimulus at a constant distance. Hence

$$s \sim rd, \quad (3)$$

which is the general formula for apparent size mentioned in the text. This formula implies

$$s = \text{const.}, \text{ if } rd = \text{const.}, \quad (4)$$

which is the law of size constancy. In this sense size constancy (4) becomes a special case of Emmert's law (1); but it is also possible to derive (3) from (4) and (2) so that Emmert's law becomes the special case of size-constancy. On this relationship, see E. G. Boring, *Size constancy and Emmert's law*, *Amer. J. Psychol.*, 53, 1940, 293-295.

G. Martius's experiment is *Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder*, *Phil. Stud.*, 5, 1889, 601-617.

On the alley experiments, see F. Hillebrand, *Theorie der scheinbaren Grösse bei binocularem Sehen*, *Denkschr. Akad. Wiss. Wien., math.-nat. Kl.*, 72, 1902, 255-307; W. Poppelreuter, *Beiträge zur Raumpsiologie*, *Z. Psychol.*, 58, 1911, 200-262; F. Schubotz, *Beiträge zur Kenntnis des Sehraumes auf Grund der Ehrfahrung*, *Arch. ges. Psychol.*,

20, 1911, 101-149; Blumenfeld, *op. cit.*, 1913. It should be noted that the text omits mention of many small differences in these researches. Different investigators found different forms and directions of curvature in the loci of the apparent parallels. Blumenfeld got slightly different results when his observers tried to make the lights form parallel lines and when they tried to make them everywhere equidistant.

On the perception of size by chimpanzees, see W. Kohler, *Optische Untersuchungen am Schimpansen und am Haushuhn*, *Abh. preuss. Akad. Wiss., phys.-math. Kl.*, 1915, no. 3, esp. 18-139; in chickens, see W. Götz, *Experimentelle Untersuchungen zum Problem der Sehgrössenkonstanz beim Haushuhn*, *Z. Psychol.*, 99, 1926, 247-260, esp. 254 f. On the relation to age, see F. Beryl, *Ueber die Grössenauffassung bei Kindern*, *ibid.*, 100, 1926, 344-371, esp. 364; and also H. Frank, *Die Sehgrössenkonstanz bei Kindern*, *Psychol. Forsch.*, 10, 1928, 102-106. On individual differences, see R. H. Thouless, *Individual differences in phenomenal regression*, *Brit. J. Psychol.*, 22, 1932, 216-241; M. R. Sheehan, *op. cit.*, esp. 39-43. For a general analysis of the conditions, see B. E. Holaday, *Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen*, *Arch. ges. Psychol.*, 88, 1933, 419-486.

The best exposition of size constancy from the point of view of Gestalt psychology is Koffka's, *locc. cit.*, and esp. Chap. 3, where Koffka combats the "interpretative theory" of the perceived object in the interests of the phenomenal object formed by field forces. That Koffka's belief in "size constancy" is a measure of his disbelief in the constancy of size for constant visual angles is clearly indicated by his comment that Beryl's data "reveal an amazing degree of constancy if compared [with what]

represents the sizes of boxes which would have produced a constant retinal image" (p. 92). Beryl's data would be amazing only to one who expects constant apparent size for constant visual angle. This concept of phenomenal constancy was first introduced by Kohler in 1915, *loc. cit.*; he did not then, however, speak of *Sehgrößenkonstanz*, but only of relative constancy. In 1917 he used the terms *Farbenkonstanz* and *Größenkonstanz* casually, *Die Farbe der Sehdinge beim Schimpansen und beim Haushuhn*, *Z. Psychol.*, 77, 1917, 248-255, esp. 254. By 1926 Miss Frank, as we have seen, regarded the concept as established and put *Sehgrößenkonstanz* in her title. *Size constancy* came into English as a translation later.

A terminological confusion arises here because the constancy of relation between the form of the perception and the form of the stimulus has been called the constancy hypothesis—casually by Köhler, who inveighed against its use, *Ueber unbemerkte Empfindungen und Urteilstäuschungen*, *Z. Psychol.*, 66, 1913, 51-80, and more specifically by his colleagues later. The Gestalt psychologist rejects the constancy hypothesis because it is incompatible with phenomenal constancy. It is better to forget the first term, although it is interesting to see that Brunswik's *Zwischengegenstand* is a compromise between these two opposed constancies.

For E. Brunswik's systematic view,

see his *Die Zugänglichkeit von Gegenständen für die Wahrnehmung*, *Arch. ges. Psychol.*, 88, 1933, 377-418, for the formula and degrees of *Dingkonstanz*, 387-411; *Wahrnehmung und Gegenstandswelt*, 1934, esp. 48-92; also Holaday, *op. cit.* For R. H. Thouless's experiments and measures of regression, see his *Phenomenal regression to the real object*, *Brit. J. Psychol.*, 21, 1931, 339-359 (formula, 344); 22, 1931, 1-30.

For a recent analysis of the perception and its 'reduction,' almost to the law of the visual angle, by the removal of various cues to the perception of distance, see A. H. Holway and E. G. Boring, *Determinants of apparent visual size with distance variant*, *Amer. J. Psychol.*, 54, 1941, 21-37.

Tridimensional Dynamics

The best account of the Gestalt theory of the perception of depth and distance is K. Koffka, *Some problems of space perception*, *Psychologies of 1930*, 1930, 161-187. Cf. also Koffka, *Principles of Gestalt Psychology*, 1935, 265-280. Koffka gives the references to the illustrations cited (on his behalf) in the text. The reference to E. Schur is *Mondtäuschung und Sehgrößenkonstanz*, *Psychol. Forsch.*, 7, 1926, 44-80, esp. 75 f. For W. Köhler on isomorphism, see his *Gestalt Psychology*, 1929, 60-64; *The Place of Value in a World of Facts*, 1938, 185-232; also other references cited on p. 96.

Chapter 9

PSYCHOPHYSICS OF TONE

ABOUT hearing there is less to say in modern psychology than about vision, and the history of the psychology of audition is correspondingly less voluminous and complicated. There are, for one thing, fewer auditory relations to describe than visual, partly because the spatial dimensions are not added to the qualitative in hearing, and partly because the ear is mechanically so arranged that there are fewer after-effects of stimulation. On the other hand, the history of music, like the history of pictorial art, goes back into prehistoric antiquity, and it was in connection with music that the stimulus to tone was first defined. The establishment of the notes in definite relation to each other came first, a process forwarded by Pythagoras and continued through many centuries up to the adoption of equal temperament in the nineteenth century. In the seventeenth century the physics of sound began—the knowledge about frequencies, media of transmission and velocities. The psychophysics of tone began in the eighteenth century, although there was no sustained effort in that field until experimental psychology appeared in the latter half of the nineteenth century, a period which Helmholtz introduced with his *Die Lehre von den Tonempfindungen* in 1863. That text, written and published in between the second and the third volumes of his *Handbuch der physiologischen Optik*, was, and is, like the *Optik*, the classic in its field. In the twentieth century, however, the pattern has completely changed. The development of the thermionic vacuum tube has given the investigator such a control of acoustic stimuli, that the resulting wave of research has brought the auditory problems well past the stage where phenomenological description is any longer in itself an important goal.

The Origin of Musical Notes

Because tones, unlike colors, do not mix to provide an infinitude of tones from the combination of a few primaries, it has come

about that music differs from painting in its employment of discrete elements instead of the qualitative continua that the laws of color mixture make available. Thus the scale of western music—the most highly developed tonal scale—still carries in itself the limitations of the fixed pitches of the musical instruments of the Greeks—the lyre and the flute. Had it been possible with the seven-stringed lyre to get intermediate pure tones from the mixture of tones lying on either side of the resultant—like orange from red and yellow—the development of music would have been altered enormously.

If we are to have a tonal scale of discrete fixed pitches, then we must know about the intervals between the pitches. We must know, for example, whether there are any native intervals determined by perceptual properties of the human organism. It is quite possible that the octave is in some sense 'native,' for voices of different register tend in singing to fall into unison with octave separations, as, indeed, Aristotle remarked. There may, in fact, be something about the human ear that accounts for the similarity of tones when the frequency of one is just twice the frequency of the other, or it may be that this similarity comes about because all the harmonics of the octave of a tone are also harmonics of that tone. We know now that a fundamental tone is never heard without its partials. The octave was, however, whether 'natural' or not, too large an interval to enter into early music which, besides being vocal, melodic and simple, was quite limited in range.

Besides the octave, the interval of the fourth has been regarded, at least by some students of the history of music, as a native interval; in descending cadence it is, they say, the natural drop for the voice in making a statement. The recitative is thus based upon the descending fourth. Yet it seems probable that what is natural about the fourth is only its descent, which gives it the feeling of finality or repose. It was the Greeks who, after approximating the fourth, at first fixed it as the basic interval when Pythagoras invented the monochord and discovered the relation of pitch to the length of the string.

That there are any other native elements in tones, besides the octave similarity and the relaxation of a falling cadence, the history of music seems to deny. There have, indeed, been many other kinds of scales in eastern music, and the history of our own occidental scale from the Greeks to the present is a history of decision

and change with regard to the notes established as proper at any given time. Let us examine the origin of this scale among the Greeks.

The primitive base of Greek music was, then, the descending cadence of approximately a fourth, two notes elaborated by a third intermediate note to provide the rudimentary possibilities of melody. In modern notation we might represent such a cadence by A-G \flat -E. This sequence, however, presently became complicated by the addition of a leading note just above the final note, and the adjustment of the position of the intermediate note to a place halfway between the initial and the leading notes; thus, A-G-F-E, which is the diatonic tetrachord basic to early Greek music. It is interesting to see that the direction here is downward, that the leading note leads down into the final. Since descent is obviously the correct way to get the perception of finality, it is puzzling to know how the modern sequence got itself reversed so that scales are thought of as ascending and the leading note as always pointing up to the tonic. The Greeks also came to use two other forms of the tetrachord: the chromatic tetrachord, in which the middle note was moved down so that it was practically a leading note to the leading note (as A-G \flat -F-E); and the enharmonic tetrachord, a descending fourth with two notes before the lower separated by quarter-tones (like A-F- ϵ -E, if ϵ signifies a quarter-tone above E). These three kinds of tetrachords were different genera and were used contemporaneously. To each genus different musical effects were ascribed: the diatonic was said to be manly and austere; the chromatic, sweet and pathetic; the enharmonic, animating and mild.

Somewhat later a second tetrachord was added, continuing beyond the first, perhaps by Terpander (ca. 700 B.C.) who is generally regarded as the founder of classical Greek music. In the resultant heptachord the upper note of the lower tetrachord was used as the lower note of the upper tetrachord; hence the sequence was much like D-C-B-A-G-F-E, with A, common to both tetrachords, the "middle string." The note of this "middle string," much like the tonic in modern music, came to be an orienting point for the melodies. The seven-stringed lyre, on which this music was played and which was the accepted instrument for a century or more until Pythagoras successfully added the eighth string, actually did not contain the octave. Thus it happened that music was

not at first based on that 'natural' interval, the octave, which was introduced only later when the range of melody was extended.

Various Greek musicians tried to increase the lyre's strings to eight or even eleven, but they met with opposition. Among the Spartans, who had a law against more than seven strings, judgment was sometimes passed requiring the executioner to cut the additional strings from a lyre. Pythagoras (ca. 530 B.C.), however, was able to bring music under his philosophy of number, and thus to begin a long association between music and mathematics. He originated the monochord, a stretched string whose length can be altered by a movable bridge. The first to establish the octave as determined by lengths of string in the ratio 2:1, he went further to lay down the rule that simple ratios yield the best consonances. There is no doubt that the properties of number were more important to Pythagoras in his consideration of music than the properties of auditory sensation; only long afterwards did Aristoxenos (ca. 318 B.C.), in opposition to the Pythagoreans' preoccupation with number in music, found a school that championed the importance of sound as validating the musical canons. In terms of the ratios of string-lengths, however, Pythagoras was able to regularize the scale. He separated the two tetrachords of the lyre, extending them to fill out the octave and adding the hitherto objectionable eighth string. The scale then became approximately (E-D-C-B)-(A-G-F-E), a sequence which constitutes the notes of our modern scale, although organized about the "middle string," A, instead of the tonic, C.

It was in this way, by the necessity of establishing fixed pitches in musical instruments, that the system of tones became defined as discrete units, the notes of scales, and not as continua, like the colors of painting. The important thing to realize is that, although historically scales have been basic in the understanding of tone, the intervals of the scales are not 'native' and inevitable; rather, they have been set up, one way and another, in a long history of experimentation. The first interval to be fixed was the fourth, the limits of the tetrachord. Its strings gave the *soni stabiles* which were not changed, and upon them all other intervals were based.

After Pythagoras there was, all the way on into the nineteenth century, a complicated development of scales and intervals. Music had to face a series of inevitably unsatisfactory compromises. The difficulty was this. On the one hand, the Pythagorean notion that

the best intervals are those with the simplest ratios was right; these intervals, having the largest number of coincident harmonics, were best suited for the polyphonic music that developed after the tenth century and for harmony proper, which may be said to have begun in the sixteenth century. On the other hand, there was an insuperable mathematical difficulty in the fact that the product of two simple ratios is not usually an equally simple ratio. If only $4/3 \times 4/3$ equaled 2 instead of $16/9$, for then two perfect fourths would make an octave! Actually they make a minor seventh. In the Pythagorean scale each tetrachord is meant to be two whole tones and a semi-tone, and the tetrachords are separated by a whole tone, so that six whole tones ought to make an octave. Euclid discovered, however, that six whole tones exceed the octave by what is about an eighth of a whole tone. Didymos (ca. 63 B.C.—ca. 10 A.D.) worked out the best solution. He made each tetrachord consist of a major tone ($9/8$), a minor tone ($10/9$) and a semi-tone ($16/15$), with a major tone between the tetrachords. This arrangement gives the octave, for $9/8 \times 10/9 \times 16/15 \times 9/8 \times 10/9 \times 9/8 \times 16/15 = 2$ exactly. The resultant scale is diagrammed as the scale of just temperament at the left of Fig. 50. Receiving the sanction of Ptolemy (ca. 130 A.D.), it persisted for a long time as the ancestor of the modern occidental scale. It is, in fact, what has been called the 'natural' scale, although its naturalness is more mathematical than physiological. Newton believed in it: he tried to make a heptachord of the spectrum with seven discrete colors spaced according to these specifications of Didymos (see pp. 104 f.)!

The great difficulty with this just temperament is that the seven notes suffice only for one key. Every note in the scale has a simple ratio to the key-note: from the major second to the octave, the ratios are $9/8$, $5/4$, $4/3$, $3/2$, $5/3$, $15/8$, $2/1$. Unfortunately, however, the ratio between pairs of notes that do not include the key-note are not all simple. If the fifth, C-G, has its proper ratio $3/2$, then what ought also to be a fifth, D-A, has the ratio $40/27$. And the tritone would be $45/32$ when it is F-B, and $1125/1024$ when it is B-f. Even when the eight notes of the octave are increased to twelve by dividing the major tones and the minor tones (Fig. 50, left), it is still not possible to play accurately in more than one key. Starting the sequence of intervals in just temperament at any new point throws the successive notes out of step with the original series. In fact, to get just temperament in any one of twelve

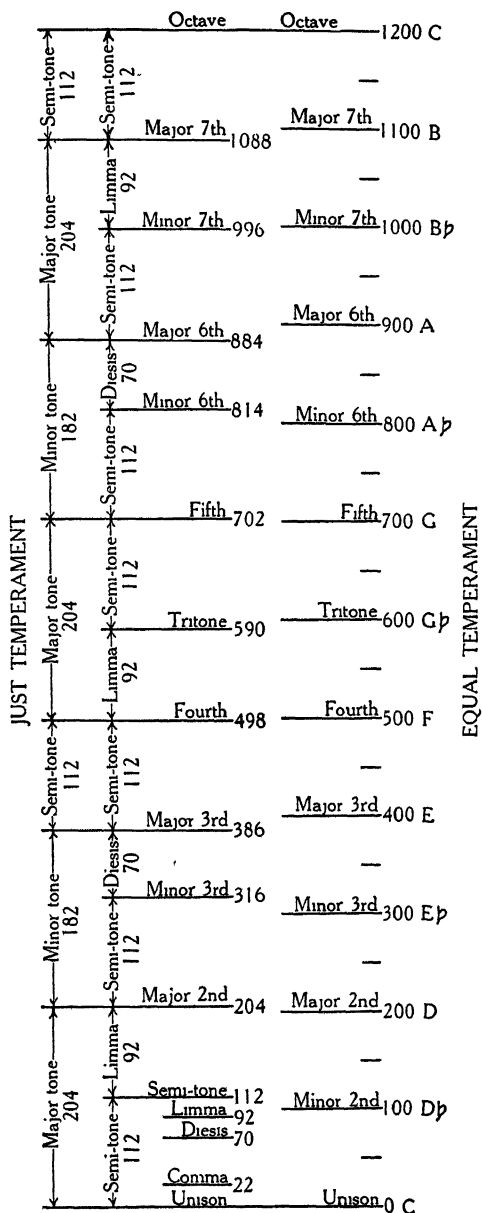


FIG. 50. MUSICAL SCALES: JUST AND EQUAL TEMPERAMENT

All figures are musical cents. 1 cent = $\frac{1}{100}$ of a semitone in equal temperament = $\frac{1}{1200}$ of an octave.

Just temperament (the 'natural' scale) at the left of the figure is the scale of Didymos and Ptolemy. Based on the simplest ratios, it is excellent in harmony but does not permit change of key without retuning of the instruments with fixed pitches. Note the relationships: a major tone differs from a minor tone by a comma; a minor tone differs from a semitone by a diesis; all intervals are combinations of the semitone, diesis and comma. This scale of just temperament is oversimplified, since there should really be additional augmented intervals besides the minor intervals, e.g., C \sharp is lower than D \flat .

Equal temperament at the right is the modern compromise in which all semitones are equal and in which the intervals are displaced from just temperament by as much as 16 cents.

keys would require 34 notes in the octave, a complication much too great for an organ or other instruments in which the pitches are fixed. Thus eventually further compromise became necessary.

The history of musical temperament is important for psychology merely in this way. It shows how the 'natural' intervals are unique only in the sense that, having many coincident partials, they are most consonant in harmony. The various scales that have had serious use in occidental music are as follows.

(1) *Just temperament*, the Pythagorean octave compromised by Didymos and Ptolemy (Fig. 50) to give simple ratios, and hence called in the nineteenth century the "natural scale." It can be used in a *cappella* singing, and the Tonic Sol-fa Association of London (founded 1873) urged its use; but it cannot be employed, as we have seen, on most musical instruments if there is to be a change of key.

(2) *Pythagorean temperament*, a series of perfect fifths, not much used in western music. In it the octave is not a perfect simple ratio.

(3) *Meantone temperament*, invented by F. Salinas in 1577 to make the 'whole tone' a mean between the major tone and the minor tone (cf. Fig. 50). Actually it requires 27 notes to the octave, but the organ builders, because it was impracticable to interpolate more than five notes in their proper places, kept the octave down to a total of 12 notes: C, C \sharp , D, E \flat , E, F, F \sharp , G, G \sharp , A, B \flat , B \sharp , making only six keys possible, viz., the keys of C, G, D, A, F, and B \flat . If an organist got into the key of E \flat and wanted to sound the chord A \flat -C-E \flat , he had to strike G \sharp instead of A \flat , and G \sharp , being almost a quarter-tone flatter than A \flat , created a discord which came to be known as "the howling of the wolves." Until its displacement by equal temperament in 1841-1846, meantone temperament, nevertheless, was used almost universally on organs, claviers and pianos.

(4) *Equal temperament*, which has long been found in Chinese music, was suggested by Aristoxenos (ca. 320 B.C.), and supported formally by the arguments of G. Zarlino (1558). It was favored by J. S. Bach, who wrote for it the *Wohltemperirtes Klavier* (1722), containing twenty-four preludes and fugues, one in each of the twelve major and the twelve minor keys. The general adoption of equal temperament waited, however, until the middle of the nine-

teenth century. Fig. 50 shows how much it varies from just temperament.

So it turns out that the stabilization of the musical notes is a relatively recent occurrence in the history of music, and the reason for this tardy development is not hard to find. Music had to wait upon physics. When Salinas invented meantone temperament in 1577, there were no exact means for measuring the stimulus. The variation of pitch in musical instruments was controlled by altering the lengths of strings or of pipes. Although Pythagoras knew that the shorter strings vibrate more rapidly, the measurement of frequency and the law of its relation to pitch awaited Galileo's research about 1636. Even then there was no simple method for testing the tuning of an organ in terms of frequency. For almost three centuries the tuners aimed at meantone temperament; yet there is little evidence that for the first half of this period they struck very near their goal. The meantone 'wolves' might howl in certain keys, but undoubtedly mistuning led to other discords even when the 'wolves' were silent.

Shortly after the coronation of George I in 1714, John Shore, who had been sergeant trumpeter at the coronation and had been made Lutenist in the Chapel Royal, invented a tuning-fork—a "pitch-fork," as he humorously called it—by which to tune his lute. The law of beats was discovered about the same time (1700). Thus there actually existed, before Bach wrote his music for the well-tempered clavier, a possibility of accurate tuning, although it was not generally utilized until later.

It was in the middle of the nineteenth century—the 'century of science'—that accurate calibration of musical notes was undertaken. By then the tuning fork had developed into an instrument of precision. Thus in 1834 J. H. Scheibler was able to construct a tonometer of 56 tuning forks for the purpose of calibrating musical instruments. Marloye, a famous instrument-maker in Paris, put the tuning forks on resonance boxes in 1839, and his even more famous successor, Rudolph Koenig, who took over his business in 1858, developed the precision of the tuning fork to such a degree that Koenig forks came to be known throughout the scientific world and cherished in physical and psychological laboratories. Meanwhile, in 1840, A. Seebeck constructed a siren which proved a convenient instrument of calibration because its fre-

quency was easily determined by its speed of rotation and the number of holes in its rotating disk. Subsequently, in 1866, A. Toepler employed a stroboscopic method for the study of the vibration of singing flames, and A. Kundt devised the dust method for computing the frequencies of tones. Altogether, the forty years from 1830 to 1870 constituted a period in which great advances were made in the control of the frequency of the tonal stimulus. That the unsatisfactoriness of meantone temperament should have been admitted at this time was, thus, no accident, or that equal temperament should have come into general use in the 1840's.

This same period saw the establishment of absolute standards of pitch. Scheibler, using his tuning-fork tonometer to study the pitches of concert pianos in Vienna, finally recommended to a congress of physicists at Stuttgart in 1834 that a standard of $A = 440$ c.p.s. be adopted. This standard has since been called the Stuttgart Pitch. The pitch at the Paris Opera, which had been $A = 404$ c.p.s. in 1699, had risen to $A = 423$ in 1810 and to $A = 456$ in 1858. A commission of the French government then decided to establish the standard at $A = 435$, and the physicist Lissajous prepared a standard fork of this frequency for preservation. (Koenig in 1880 found that the fork was, however, almost half a vibration in error.) Ellis, who has been at great pains to discover all the pitches that have ever been established and used (sometimes he constructed model organ pipes to historical specifications in order to find out the frequency), has listed 188 pitches employed from 1615 to 1885. The standards range through the interval of a fifth, from $A = 373$ to $A = 567$!

Thus it came about that music, aided by recent physics, was able to furnish the new experimental psychology in the middle of the nineteenth century with pretty good specifications for the tonal stimuli. Helmholtz's *Tonempfindungen* in 1863 dealt fully as much with musicology as with psychophysiological acoustics. Until well into the 1920's the tonal stimuli were designated by psychologists as notes in some standard scale for some standard frequency of A . The occurrence that took the psychology of hearing away from the musical specifications of the stimulus was the development of the control of electrical frequencies by the electronic vacuum tube. With electrical oscillators the control of pitch became independent of musical instruments and of fixed pitches in general. Because frequency could at last be varied easily and

continuously by changing the electrical properties of a circuit, the artificial emphasis upon a fixed number of notes—with equal temperament music uses only about 100 notes, nine octaves—was lost. A frequency of 1000 c.p.s. was accepted as the specification for a reference tone, and the only trace of the musical intervals in the research of the 1930's comes about accidentally, inasmuch as it is easier to deal with the logarithms of frequencies than with frequencies proper.

Physical Acoustics

At this point we must turn to the history of the auditory stimulus, a part of the history of the physics of sound.

The Acoustic Medium. Although it was obvious from the first that sound can travel through the air, it was not clear that the air itself is the essential conductor. It was Aristotle's suggestion that a sounding body displaces the adjacent air, which in turn moves the air next to it, to the end that the spread of sound occurs by the propagation of the motion of the air. There could, however, be no crucial test of such a theory until means were found for creating vacuums.

Vacuums could be had after Torricelli had invented the barometer in 1643. The barometer is an inverted tube in which atmospheric pressure supports a column of mercury, with a vacuum above the mercury below the sealed upper end of the tube. In 1650 Father Kircher used such a tube with a glass globe, containing a bell, sealed to one end. He filled it with mercury, inverted it in a dish, letting the mercury level drop until it was sustained by atmospheric pressure, and thus had the bell in a Torricellian vacuum. Unfortunately for the cause of truth, when he actuated the bell from outside by a lodestone, the sound was audible—presumably because the support of the bell conducted the sound to the glass.

A few years later the Italian Academy in Florence failed in a similar experiment, but presently Guericke, having invented the air-pump in 1650, utilized it for this experiment. After suspending by a thread in a jar a bell that would ring by clockwork, he noted that, as he pumped the air from the jar, the sound of the bell got fainter until it ceased, and that, on the other hand, the sound became audible and then louder as the air was again let in. This experiment, carried out successfully by Guericke about 1657,

was verified by Robert Boyle in 1660. Thereafter various other investigators showed that water, solids and gases other than air will conduct sound, thus explaining Father Kircher's failure. It is interesting to note that seventeenth-century music could be so highly developed—well beyond the stage of meantone temperament in organs—before anyone was certain that air is the usual conductor of sound.

The Velocity of Sound. Another fundamental problem interesting the natural philosophers of the seventeenth century was the velocity of sound. It was apparent that, when a cannon is fired at a distance, the flash is seen before the sound is heard. Since the transmission of light is at these distances practically instantaneous, the measurement of the time between the flash and the noise will give, in conjunction with the distance, the velocity of the sound. Gassendi (1624) was the first to employ this method. Wishing to show Aristotle mistaken in his assertion that high-pitched sounds travel faster than low, he compared the times for the sound of a cannon and of a musket and found no great difference.

Mersenne (1636), Galileo's pupil, repeated Gassendi's experiment, and there were numerous subsequent repetitions. Everybody got a different velocity, but Gassendi's figure, which was 1473 feet per second, was lowered by Mersenne to 1380 feet per second, and by Borelli and Viviani (1656) to 1077 feet per second. (The correct value is about 1128 feet per second in dry air at 20°C.) Mersenne also used an echo. He found that he could shout *Benedicam Dominum* in just one second, and that an echo from a wall 519 feet away began just as his utterance ended, so that his voice and the echo together said *BenedicamDominum-BenedicamDominum*. The velocity of sound he therefore set down at 1038 feet per second.

Newton in 1687 calculated the velocity from theoretical considerations, but he got a figure of only 968, against which was set an official determination at the Greenwich Observatory (in 1708) of 1142 feet per second. It was not until 1740 that Bianconi measured the great effect of temperature upon the velocity, thus explaining some of these earlier discrepancies. Between the results of an experiment in summer and one in winter (36°C. difference), Bianconi found a difference of about 42 feet per second.

Pitch and Frequency. From the psychologist's point of view the

great acoustic event of the seventeenth century was Galileo's discovery of the dependence of pitch upon frequency. Pythagoras had worked out the laws of the dependence of pitch upon the length of strings; he knew that decreasing the length of the string to one-half raises the pitch an octave, and he had determined the other simple ratios for the intervals of the fourth and the fifth. He also knew from the observation of long strings in which the vibration is immediately apparent, that shortening the string increases the speed of vibration: yet of relative frequencies he had no quantitative knowledge.

Galileo found, in scraping a brass plate with an iron chisel, that the operation sometimes made a whistling noise and that, whenever this tonal sound was created, the chisel left a series of small parallel streaks upon the brass. He experimented. First he noticed that, the higher the pitch, the more numerous the streaks. Then he measured the pitches by resonance. He got a series of streaks to the sound of which one string on an instrument responded, and after that another series to which another string responded. The two strings were tuned a musical fifth apart. By counting the scratches in equal distances on the brass he found 45 scratches for the higher string and 30 for the lower, that is to say, the ratio 3:2, the proper ratio for the fifth. In other words, pitch depends upon frequency, and frequency varies with the length of the string. After that Galileo, old and frustrated by the Inquisition, left the problem to Mersenne.

Mersenne began with a taut hemp rope, over ninety feet long, in which the vibrations were slow enough to be counted. Then he extended his studies to brass wires, and was able finally to lay down the chief laws of the stretched string: that the frequency of the string is directly proportional (a) to its length, (b) to the square root of the weight which stretches it, (c) to the square root of the weight of the string itself, (d) to the reciprocal of its diameter, and (e) to the reciprocal of its specific gravity. These relationships, first put in a more general formula by Brook Taylor in 1713, were given their modern expression by Euler in 1739:

$$\text{Frequency} = \frac{1}{2 \times \text{length}} \sqrt{\frac{\text{tension}}{\text{mass}}}$$

Meanwhile Shore had invented the tuning fork (post 1714), and Galileo's discovery was beginning to become important to the

musicians. Robert Hooke, for instance, in 1681 arranged to rotate a set of toothed brass wheels allowing an object to strike the teeth. Rapid rotation produced a musical sound. Doubling the speed gave an octave, increasing it one-third gave the fourth, and so on. Much later this principle was used by Savart (1830) for the determination of the limits of hearing. The general principle—that discrete impulses whose rate is mechanically controlled will yield pitches which vary with the rate—was also employed in Seebeck's siren (1841).

The Galilean law of pitch as a function of frequency stood as a basic canon of psychological acoustics until Zurmühl showed in 1930 that pitch is also to a lesser degree dependent upon intensity. Most textbooks of physics identify pitch with frequency as if there were no difference; yet, if that be true, what was it that Galileo discovered?

Resonance and Harmonics. Even though the story of Galileo's sudden insight into the principle of the pendulum, as he watched the swinging lamp in the cathedral at Pisa in 1583, may be apocryphal, still there is Galileo's own word for it that many times he had observed in the swinging of lamps in churches—how each lamp had its own rate of vibration, depending upon its length and not at all upon the amount of its excursion. Thus Galileo conceived the notion that objects may have natural periods of vibration, and this principle of the pendulum may be transferred to the vibrating string. He even represented consonant intervals by suspending from the same horizontal rod pendulums 16, 9 and 4 units long, so that they would vibrate with rates proportional to 4, 3 and 2. If such pendulums start in coincidence, they fall into coincidence again at every second beat of the longest (or every fourth beat of the shortest), illustrating the consonant relations of the octave (4:2), the fifth (3:2) and the fourth (4:3).

It was Mersenne who noticed that a vibrating string produces overtones, that, as the fundamental pitch fades out, higher components of the sound sometimes emerge. Descartes suggested to him that these overtones might result from the string's vibrating in parts, each part on its own account, but Mersenne had no conclusive proof of what was going on in the string.

Not until 1677 was this matter cleared up. Then John Wallis described and discussed two independent experiments that had been made at Oxford in 1673, one by William Noble and the other by

Thomas Pigot. Wallis first noted the fact of resonance in strings: if two strings be tuned to the same pitch and one be plucked, the other is thrown into vibration. He also observed that a string will vibrate if its pitch is sounded on an organ, and he repeated from hearsay the story that "a thin fine Venice glass cracked with the strong and lasting sound of a trumpet or cornet near it, sounding in unison" with the natural tone of the glass. The discovery of Noble and Pigot was that when consonant strings, not in unison, are together, the sounding of the shorter will cause the longer to vibrate in parts—as they noted by wrapping a bit of paper around the string and moving it along to different points so that they could easily see the vibrations. Wallis satisfied himself that the string always divides thus into aliquot parts—halves, thirds, quarters, etc., responding respectively to the octave, the twelfth, the double octave, etc.

Joseph Sauveur (1701) rounded out and established the theory of overtones, coining the words *fundamental* to apply to the basic frequency and *harmonic* for the overtones that are even multiples of the fundamental. The harmonics can be brought out, he showed, if a vibrating string be touched lightly at some even fraction of its length, that is to say at a *node*, as he called the point that divides from each other the separately vibrating parts of the string. If the string be touched at one-third of its length, for instance, then its fundamental frequency is damped, as are also the second, fourth, fifth, seventh, eighth harmonics; but the third harmonic (along with the sixth and ninth) remains strong, sounding the pitch which corresponds to three times the frequency of the fundamental. This discovery meant that the harmonic overtones of a fundamental frequency are really *partials*, as they were named later by Helmholtz; that is to say, the total vibration of the string is to be regarded as the sum of all these partial vibrations going on simultaneously. Although the upper partials may not be heard in the total mass of sound, they come out clearly when the lower components have been damped out by Sauveur's method. Thus it was more than a century after Salinas had prescribed meantone temperament for organs before the physics of harmony was understood well enough to furnish the valid argument for just temperament.

Analysis. In 1822 Baron Fourier, French statesman-scientist-mathematician, made, in the course of a study of the theory

of heat, a remarkable mathematical discovery. He found that any continuous function or curve, no matter what its shape or how irregular (provided it does not return on itself), can be represented as the sum of a series of sine curves, in which the separate terms vary in length (period), in height (amplitude) and in phase relation to one another, and in which the wave-lengths are even fractions ($1/2$, $1/3$, $1/4$, etc.) of the wave-length of the function being represented. He proved that this infinite series is convergent: the original function can be represented with any degree of approximation by adding enough terms in the series. In addition he showed that there is possible no other such general analysis.

Because it provided a mathematical theory of the vibrating string, this discovery had presently a great effect upon physical acoustics. While Sauveur had made it certain that a string vibrates in a great many different ways at once—as a whole, in halves, in thirds, etc.—no one had made it clear how the composition of so many modes of vibration takes place. Now Fourier's series provided an analogue: the pattern of the vibration would be the pattern given by the algebraic sum of all the harmonic (sine) components that represent the various harmonics involved. Nor was this new principle only synthetic; it was also analytic, for it showed that any periodic motion at all can be reduced to the sum of a series of simple harmonic vibrations. In the case of sound the demonstration of Fourier's theorem meant that any irregular wave, no matter how it has been produced, can be regarded as complex, because it can be analyzed into sine components. If the form of the sound wave is known, then the analysis can be made mathematically by the application of Fourier's series; or the same components can be identified by resonance if an adequate series of resonators is available. A resonator—be it pendulum or taut string—has a natural period of simple harmonic vibration with which it responds to the same vibration occurring elsewhere, either simply or as a harmonic partial of a complex motion. In other words, analysis by resonance and by Fourier's series must, by the identity of their natures, give the same result.

This kind of analysis was given a psychophysiological meaning by G. S. Ohm in 1843. (This was the Ohm who lent his name to the law of simple electric circuits and to the unit of electrical resistance.) Ohm argued—quite elaborately and at length—that for any complex wave-form the ear 'hears out' the simple harmonic

components, the same components that Fourier's analysis or resonance would give. Ohm's acoustic law thus asserts that it is possible to pay separate attention to the simple harmonic components of any irregular sound wave. The law has been generally accepted. Providing the basis for Helmholtz's resonance theory of hearing, Ohm's law along with Helmholtz's theory dominated all theoretical discussion in the field of audition for at least sixty years (1863 *et seq.*). It seemed for a time as if the law broke down in the case of difference tones, but now it is realized that these difference components are added by distortion in the ear and that the hearing of them further substantiates the law. All in all, the analysis of irregular wave-forms into harmonic components remains the obvious and important fact about the perception of tones.

So laborious is the mathematics involved in the use of Fourier's theorem that various machines have been invented to lessen the labor. Lord Kelvin invented a mechanical harmonic synthesizer (primarily to use in predicting tides) in 1872 and a harmonic analyzer in 1876, and other and better machines have been built since. If it is a sound which is to be analyzed by such a machine, that sound has first to be turned into a curve by an oscillograph. This process has, however, changed since the development of the thermionic vacuum tubes in connection with the radio. Nowadays the sound is turned into an electric wave which is analyzed by electric resonance. Wave-analyzers are built so that an amplifier can be tuned to respond to one or another of the components of the wave and the strength and frequency of each component read off from a dial.

Sometimes the question is asked: Why is an irregular wave-form called 'complex' when its 'components' usually have never had independent existence until they are separated by analysis? D. C. Miller has published the Fourier analysis of a girl's profile into the sum of eighteen sine curves: are these 'components' really parts of the girl's face? The obvious answer is that an irregular periodic curve is 'complex' in the sense that it can be analyzed into harmonic components (a) mathematically (Fourier's series), (b) physically (resonance), and (c) psychophysiologicaly (Ohm's law) with approximately the same results—presumably because Fourier's series gives the results of resonance and because the ear acts like a resonator. There is, however, beyond this obvious answer, an historical reason as to why it is natural to identify *irregular* with *complex* in

dealing with sound waves. The order of discovery was synthetic, not analytic. Galileo was sure about the simple natural periods of strings first. It was only later that Sauveur established the fact of the compounding of these different partial vibrations. The irregular wave-forms were first known as compounds and not until later as irregular. It was a full century after Fourier before anyone realized that (on account of subjective harmonics) a pure tone is so rare as to be almost non-existent, that tones are in fact nothing more than ideal analytical elements, primarily useful for simplifying description.

Apparatus. There were many sources of tone in the nineteenth century. The siren had the advantage of being easy to control and calibrate, since its frequency is simply the number of holes passing the air-jet in a second. The tuning fork was most precise as to frequency, the most elaborately developed, the most elegant. The resonator increased the fork's intensity and assured its purity. Organ pipes operating on large wind-chests and reed tonometers with small wind-chests of their own were employed where purity of tone was not desired. Blown bottles were often used, while the Stern variators, which were brass bottles with piston bottoms, provided the best method for continuous change of frequency. The Galton whistle and the Koenig cylinders (short suspended bars struck with a metal hammer) were used to determine the upper limit of hearing, and lamellas and wire forks for the slow frequencies at the lower limit. The most famous makers of these instruments were Antoine Appunn and Rudolph Koenig, both of Paris. Koenig was closely associated with Helmholtz: he built Helmholtz's ideas into apparatus, and Helmholtz built at least one piece of Koenig's apparatus (the vowel synthesizer) into a theory.

The siren was developed before the tuning fork. If pitch be frequency, as Galileo had seemed to show, then the way to get a pitch would be to create a frequency. That is essentially what Hooke did in 1681 when he let an object strike the teeth of a revolving wheel: given the number of teeth and the speed of the wheel, the pitch is known. A better method, however, is to have a blast of air strike a series of holes in a revolving disk; such a device is a siren. Cagniard de la Tour, who made the first siren in 1819, employed two superposed horizontal disks, with corresponding rings of holes. The lower disk was fixed to a wind-chest; the upper could rotate above it. The holes of the two disks were drilled at different angles,

so that the air, coming through the holes of the lower disk, caused the upper disk to rotate and thus to interrupt the air stream regularly. This type of siren was later perfected by Helmholtz and Koenig. Another type, invented by Seebeck in 1841, consisted of a single rotating disk, mounted in a vertical plane and with an air-blast directed at sets of holes drilled circumferentially in rings around the center. Later these disks were made up by Koenig and other instrument makers to illustrate a great variety of tonal phenomena.

Shore's invention of the tuning fork (post 1714), we have already noted along with Scheibler's tuning-fork tonometer (1834) and Marloye's putting of resonance boxes on tuning forks (1839). The triumph of the tuning fork was, however, chiefly the achievement of Rudolph Koenig (1832-1901) who, taking over Marloye's instrument business in 1858, devoted his life—unmarried and making a shop of his house on the Isle St. Louis in Paris—to the manufacture of acoustical apparatus of a precision and quality that made his tuning forks the revered possessions of many laboratories throughout the world and a classical tradition in the psychological laboratories. The height of Koenig's fame was reached when, in 1876, he exhibited at the Philadelphia Centennial Exhibition where he received a special citation of honor, a gold medal, and the plaudits of the scientists, who tried with imperfect success to raise enough money to purchase his apparatus for the United States. At the exposition he exhibited 670 tuning forks, ranging from giant forks for 16 c.p.s., five feet high, weighing 200 lbs. each, and having resonators eight feet long, down to the small forks for the upper limit of hearing near 20,000 c.p.s. At home he had—it was not for sale—a precision tonometer, modeled after Scheibler's but more extensive and more accurate. It consisted of 150 forks ranging from 16 to 21,845 c.p.s.

Koenig's finest instrument was his clock-fork, made for the purpose of calibrating other forks by ultimate reference to a standard clock. It was a clock with its escapement engaged, not by a pendulum, but by a tuning fork. The frequency of this fork could be determined with an error of less than 0.0001 c.p.s. by comparing the clock that it drove with a standard clock over a period of several days. (The fork would make more than 16,000,000 vibrations in three days.) One prong of the fork carried the objective of a microscope whose eye-piece was fixed immovably to the frame of the

apparatus. The fork to be tested was set at right angles to the standard fork, and a spot upon it viewed through the microscope while both forks were kept vibrating. This arrangement forms a Lissajous comparator; the application of the principle to the calibration of forks was Helmholtz's idea. If the two forks are exactly in unison, then the double motion causes an observer to see an ellipse (or the limiting cases of an ellipse, a line or a circle) through the microscope. If the frequencies differ, then the Lissajous figure moves across the field, and the difference in frequency can be determined from its rate.

What else did Koenig do? He constructed a manometric flame apparatus for viewing acoustic wave forms. He invented the adjustable resonator and the wave siren. He assembled the famous set of tuning forks for synthesizing clangs by controlling the intensity and relative phase of ten harmonics—the apparatus that Helmholtz used, as he thought, to synthesize vowels (Fig. 51). He improved the phonautograph (invented in 1857) before Edison got at it and improved it more. With Helmholtz he devised the electromagnetic control of tuning forks (ca. 1860), the means by which a fork is kept in continuous vibration. But mostly he made beautiful accurate forks and other acoustic instruments, working as an artist works, without a primary motive of profit, putting what money he made back into further creation or experimentation.

The tuning fork was valued because it gave an approximately pure tone of accurate pitch—provided it was not struck too vigorously. The fork, however, had the disadvantage that, while accurate as to frequency, it was not easily controlled in intensity. Psychologists of the nineteenth century, however, stressing sensory quality as more important than intensity, scarcely recognized this gross defect. It is true that they could control intensity after a fashion by stopping off the fork's resonator; yet the studies of Weber's function could not be made with forks at all. For such research they had to depend upon the noise of steel balls falling on an ebony plate or of a hard rubber pendulum dropping against an ebony block, not (as nowadays) upon energy measured at the ear.

By the development of the thermionic vacuum tube the twentieth century presently changed all this. Although this is not the place to tell adequately the history of that development in physics, the chief of many events were these. Guthrie (1873) found that a ball heated to a white heat discharges a positively charged elec-

troscope. That is because hot metal emits negative charges in the form of electrons. Edison found (1883) that a current will flow between a metal plate in a lamp and the positive end of the lamp's

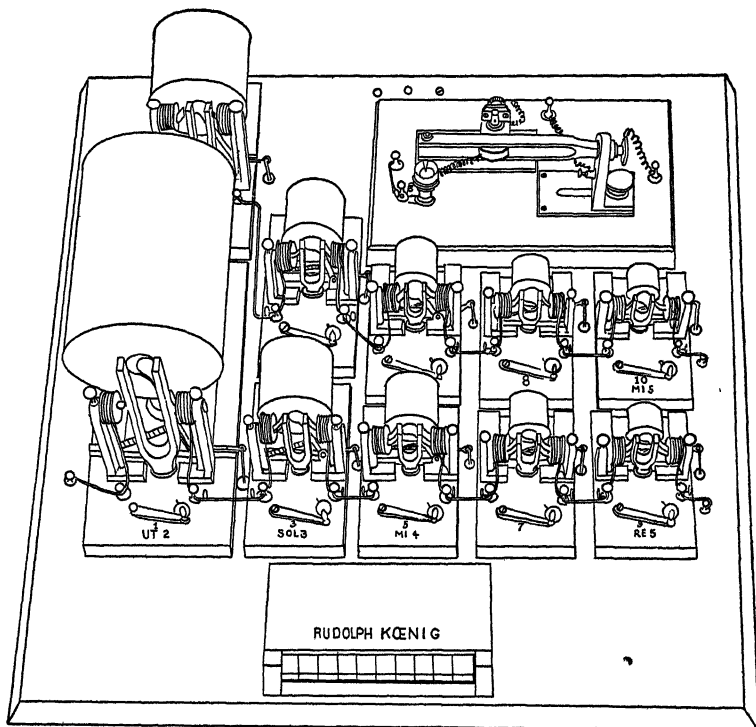


FIG. 51. KOENIG'S FORKS FOR THE SYNTHESIS OF CLANGS

Ten forks, the first ten harmonics of 128 c.p.s., each with a resonator and all actuated electromagnetically by a 128-c.p.s. master fork (shown at the rear right of the apparatus) which is properly placed in another room. The keys control stops in front of the resonator openings. Various timbres can thus be created synthetically. Helmholtz used the apparatus in his attempt to synthesize vowels.

filament if the two are connected, because the positively charged plate will attract the negative electrons from the hot filament; but no such current will flow from the plate to the negative pole of the filament. By 1896 Fleming had utilized this principle to make a valve through which a current would pass only one way. The Fleming valve rectifies an alternating current—*rectification* being

the first important function (except for the ordinary incandescent light) that vacuum tubes provided. De Forest (1907) added the grid to the tubes. A weak voltage impressed on the grid controls the facility with which a strong current flows through the valve. Thus the strong current takes on the wave-pattern of the weak voltage, and the weak voltage is said to be amplified. *Amplification* is the second important discovery. No other advance was so important until the discovery of *oscillation* in 1913 when several men—there were five claimants of priority for this idea—thought of leading the amplified current back through a tuned circuit to the grid. The electrical properties of the circuit give it a natural period of resonance, so that the amplifier “boosts” itself periodically at the rate to which the circuit is tuned. Oscillation of a precision that Koenig would have envied is the result. After 1913 the improvements in this field of physics, greatly stimulated by radio broadcasting which began about 1921, have been numerous, but mostly in the direction of greater range and stabilization of the fundamental processes just described.

Thus it came about that the radio age unfrocked Koenig in his sanctum, the psychological laboratory. Nowadays the audio-oscillator replaces the tuning fork. Intensity, being easily controlled, has at last become an important variable of the stimulus. The psychologist can now get any frequency at any intensity in the perfect quiet of an electrical potential, changing it at need into sound at the ear-drum by means of ear-phones or a loud-speaker.

The Limits of Hearing

The musical scale is limited. It is a series of pitches picked out arbitrarily from a much longer series. Although it is true that there are deep rumbles below the bass of the organ and shrill whistles above its treble, yet the series does not continue on indefinitely in either direction. Frequency, on the other hand, falls into an infinite series, for there is no rate so slow that there might not be a slower, nor so fast that it could not be faster. So it was that, when Galileo established the dependence of pitch on frequency, he also implicitly raised the problem of the lowest and the highest audible frequencies—limits which the scientists ultimately undertook to determine. Sauveur in 1700 was the first: by a study of organ pipes he set the lower limit at 12.5 c.p.s. and the upper at 6400 c.p.s.; but

the concentrated attack upon the problem did not come until much later.

During the nineteenth century there was a score of determinations of each of these limits. What strikes one first in reviewing them is their variability. The lower limit was set as low as 8 c.p.s. (Savart, 1831) and as high as 48 c.p.s. (Despretz, 1845). The upper limit was set as high as 55,000 (Bezold, 1897) and as low as 4096 (Biot, 1817). From the lowest lower limit to the highest upper limit is almost thirteen octaves, whereas from the highest lower limit to the lowest upper limit is less than seven octaves. Many of the values for the lower limit, however, fell between 16 and 20 c.p.s., and for the upper limit between 20,000 and 25,000—a total range from lowest to highest of about ten octaves, which has been only slightly reduced by the more accurate work of the twentieth century.

There were three sources of error in the work of the nineteenth century. (a) The limits of hearing are not clear cut. It was hard to decide when the successive throbs of a faint rumble became the full continuity of the lowest tone, or when the tiny bead of the highest tone faded out in the swish of the blown whistle. (b) The harmonics made no end of trouble. One might hear an instrument calibrated for 8 c.p.s. because the second partial (16 c.p.s.) was present, or the fourth (32 c.p.s.). At the upper limit the harmonics, which, being inaudible, might have been supposed to be safely out of the way, might nevertheless affect the calibration. The limits in the neighborhood of 50,000, found with the Galton whistle around the year 1900, had twice their proper frequencies because the whistle, calibrated by the Kundt dust method, impressed on the dust the pattern of the second harmonic instead of the first. The second harmonic was more intense than the first (as the dust showed), but the first was louder than the second (to the ear, which could not respond to the second at all). (c) And then error was introduced because intensity could not be accurately controlled, although, as we now know, the range of hearing increases with increase of intensity.

Sauveur set the lower limit in 1700 at 12.5 c.p.s. because he decided that the 40-ft. organ pipe was the longest to give a continuous sound which was not the successive throbs of a rumble. Both Chladni (1802) and Biot (1817) put the limit at about 16, each basing his judgment on the observation of the tones of organ pipes

and of weighted strings. In 1831 Savart, having reduced his toothed wheel to a rotating hub with two spokes which he let hit against cards or thin boards, concluded that he heard a continuous low tone at seven or eight strokes per second; accordingly, he set the threshold there. Despretz (1845), on the other hand, with the same instrument fixed the limit much higher, perhaps as high as 48 c.p.s. None of these investigators, however, had excluded the possibility that the upper harmonics were heard instead of the fundamental, a neglect which accounts for the lowest values found.

Of the problem of the harmonics Helmholtz was fully aware. First (1863) he put the limit at about 37 c.p.s., observing the tone of a weighted string which had, besides the fundamental, only high harmonics from which he thought he could abstract. Later (1870) Koenig had made him a giant fork, which—so it seemed to him—gave a low tone near 30 c.p.s. Preyer for his part used loaded reeds (1876) and a fork made by Appunn (1879) to establish the limit variously between 16 and 24 c.p.s. Later Appunn made a lamella, a steel tongue gripped in a vise and caused to vibrate by being pulled to one side and released. It tended to give very low values; Wundt (1893) reported 8 c.p.s.

Another scheme for getting rid of harmonics was to use difference tones. The 8-ft. organ-pipe for C = 64 c.p.s. and the pipe for D = 72 c.p.s. gave, according to Wundt's observation (1874) a single difference tone which corresponded to 8 c.p.s. Preyer (1876), employing the difference tones of reeds, put the limit near 20 c.p.s. The method was, however, not safe, because, even though difference tones may have no harmonics, the harmonics of their generators have difference tones. The second harmonics of Wundt's pipes of 64 and 72 c.p.s. would have had a difference tone of 16 c.p.s., and the fourth harmonics a difference tone of 32 c.p.s.

The story of the attempt to establish the upper limit of hearing is similar. Because the highest pitch he could get was from a pipe that was $\frac{1}{64}$ as long as his standard organ pipe for 100 c.p.s., Sauveur (1700) set the limit at 6400 c.p.s. Most of the early results with organ pipes were too low (Chladni, 1802, 8192 c.p.s.; Biot, 1817, 4096 c.p.s.), for the reason that an organ pipe is not adapted to give the highest audible pitches. Savart, using a wheel with 720 teeth against which he held a card or thin wooden wedge (see Fig. 52), set the limit at 24,000 c.p.s. or higher. That was more nearly correct, for the frequency was determined by the

number of teeth and the speed of the wheel, and the many upper harmonics were inaudible. Much later Koenig built the short suspended steel cylinders which, when struck with a metal hammer, give off a tiny bead of tone in the midst of the thud from the hammer. These gave 20,000 c.p.s. for the upper limit (Turnbull, 1874; Blake, 1878). Then Appunn made a set of high forks for this purpose, but unfortunately they were incorrectly calibrated and regis-

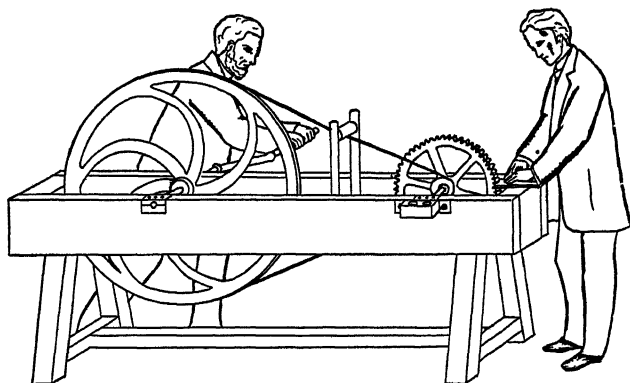


FIG. 52. SAVART'S WHEEL (1830)

A card or wedge of thin wood was held against the rotating teeth, and a note was heard, increasing in pitch as the wheel was speeded up. As there were 720 teeth, 33 r.p.s. would give 24,000 c.p.s., which Savart set as the lowest value of the upper limit of hearing. The picture is from W. H. Stone, *Elementary Lessons on Sound*, 1879, 78.

tered much too high. Preyer (1876) got with them a limit of 40,960 c.p.s.

For forty years after 1880 the Galton whistle was the standard instrument for the upper limit. Francis Galton first described the whistle in 1876. It was calibrated for frequencies from 6500 to 84,000 c.p.s. Galton contented himself with showing differences in the audibility of the high tones for persons of different ages and for different kinds of animals. With the whistle built into the end of a cane, he used it on the streets and in zoological gardens. There were several determinations of the upper limit with the Galton whistle from 1878 to 1897, all in the region of 20,000 c.p.s. for young people and less for older persons. Then Edelmann made an improved form of the whistle, in which, however, as we have noted above, some of the calibrations were defective in that they

depended upon the second harmonic. The result was that the limits were found either in the region of 20,000 to 25,000 or of 40,000 to 50,000, but not in between. As late as 1910 Titchener could state that the upper limit is 50,000, but by 1920 it was pretty well recognized that 20,000 c.p.s. is nearer the truth.

Now the fact is that the extreme frequencies for hearing depend

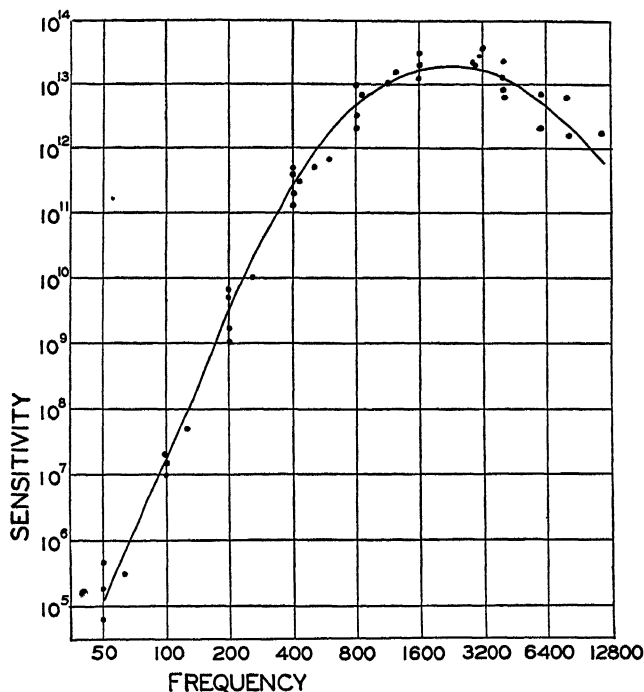


FIG. 53. WIEN'S AUDIBILITY CURVE (1903)

The function shows the relation of the sensitivities of the ear (reciprocals of threshold intensities) at various frequencies. The curve is analogous to the luminosity or visibility curve in vision.

upon intensity, although intensity was not carefully controlled in any of these experiments. Some investigator with a happy insight might, even before Galton invented his whistle, have put two and two together and seen the difficulty, for as early as 1870 Toepler and Boltzmann had found that auditory sensitivity is low at the extremes of the audible range and very high (actually rivaling the eye) in the musical region of frequencies. Lord Rayleigh

showed in 1894 how the minimum current audible in a telephone varies when the frequency changes from 128 to 768 c.p.s. Then Wien in 1903 determined an audibility curve, i.e., the function for auditory sensitivity that is analogous to the luminosity curve for vision (see pp. 177-182). Wien generated his sounds electrically, rendering them phonic with a telephone receiver. He could easily tell how much electrical energy he put into the receiver, but he had to determine the sensitivity curve of its diaphragm before he could state what energy had been delivered to the ear. He did, however, in this way determine the absolute intensive threshold of hearing at frequencies ranging from 50 to 12,000 c.p.s. The audibility curve of Fig. 53 is his plot of the reciprocals of these threshold energies, showing maximal sensitivity near 2200 c.p.s. It took nearly twenty years for the significance of this research to become apparent.

About 1922 the Bell Telephone Laboratories, under the direction of Harvey Fletcher, began a long and valuable series of investigations in psychophysical acoustics. They developed the modern audiometer, an instrument using thermionic oscillators and designed so that the intensity of each of a large number of different frequencies can be varied from inaudibility up to an intensity so great that tactual sensations, generally unpleasant, are elicited by the sound. The intensity at which these tactual components are introduced was called the threshold of feeling. One of the first papers making use of this apparatus and these terms was by Wegel (1922). Having determined minimum audibility or the threshold of hearing at various frequencies for a representative set of observers, he plotted the averages as in the lower curve of Fig. 54. He also determined the values for the average threshold of feeling, the upper curve of the figure. It will be noted that these two curves cross at each end, enclosing an area in which lie all normal sounds—for beneath the lower curve the sounds are normally inaudible and above the upper they are so loud as to be uncomfortable. Wegel proposed to see how much this area of normal sensitivity is reduced in a given case of auditory defect, and what part of it is lost, thus coming at both a quantitative and a qualitative description of auditory deficiency.

As a by-product of the creation of the audiogram the problem of the limits of hearing received a new solution. The lower curve of Fig. 54 defines, not only the threshold for intensity at any fre-

quency, but also the two limits of frequency at any intensity. Thus, reading the curve horizontally, we see that the upper and lower limits of hearing at various intensities are approximately as follows:

At .001 dynes per sq. cm., lower limit = 860 c.p.s., upper limit = 6,500 c.p.s.
 At .01 " " " " , " " = 180 " , " " = 12,400 "
 At .1 " " " " , " " = 74 " , " " = 17,500 "
 At 1. " " " " , " " = 32 " , " " = 19,000 "
 At threshold of feeling, " " = 20 " , " " = 20,000 "

Wegel's data in this way tended to fix the maximum range of hearing (without feeling) at the limits which emerged from the

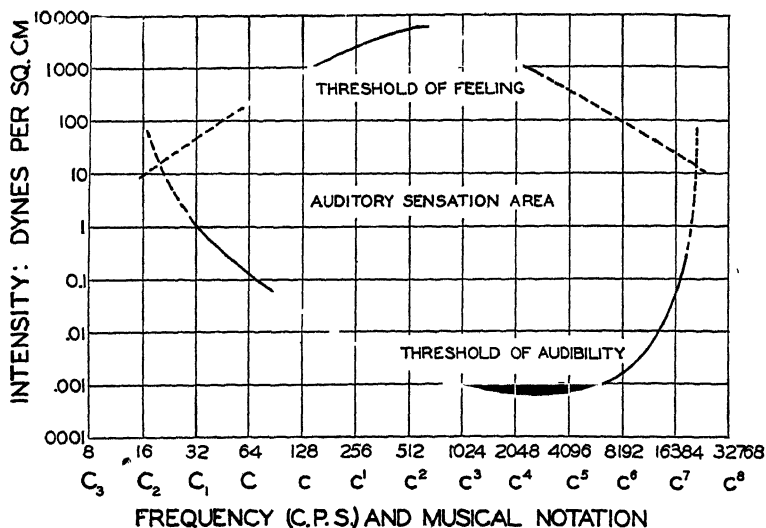


FIG. 54. WEGEL'S AUDIOGRAM (1922)

This drawing is for Wegel's data as drafted by Fletcher (1923).

old determinations—20 and 20,000 c.p.s. Numerous subsequent investigations have, however, changed the form of these early curves, especially the function for the threshold of feeling. Some studies have made it appear that frequencies well below 20 c.p.s. may be audible at intensities which are still below the threshold of feeling. Such details get settled only with time. The important modern advance in this matter of the limits of hearing was the discovery that frequency thresholds and intensive thresholds cannot be considered separately. There are more loud pitches than faint,

The curve for the absolute intensive thresholds shows that the ear (like the eye; see p. 182) is indeed a very sensitive organ. The function obtained by Wilska in 1935 showed that at 3600 c.p.s., the frequency at which sensitivity was greatest, a tone could be heard when the drum-skin of the ear moved only 4.5 thousandths of a millionth of a millimeter (4.5×10^{-10} centimeters). That amplitude of vibration is less than two per cent of the diameter of a molecule of hydrogen!

Differential Sensitivity

The fundamental problem of what range of frequencies is audible was supplemented fairly soon by the problem of differential perception: how many tonal differences are discriminable within the audible range? That this question should be raised first with respect to pitch was natural, since pitch was the dimension for which the limits had been determined. Loudness was not until recently a primary differentia of tone, and, moreover, loudness could not in the early days be controlled so as to form a satisfactory basis of investigation. Later, of course, with the rise of the interest in Weber's law, the loudness of noises was studied; and then finally after 1920 the thermionic tubes made it possible accurately to control the loudness of tones.

Apparently the first experiment on the differential threshold for pitch was made by Sauveur in 1700 when he decided that a change of $\frac{1}{2000}$ in the length of the string of a monochord makes a distinguishable difference in pitch. That difference is very small, being less than one musical cent ($\frac{1}{100}$ of a semitone in equal temperament); yet it is no less than the limen that Luft found later in the region of 500 c.p.s. Sauveur did not say to what frequency his string was tuned.

In 1827 Delezenne, studying the musical intervals, took under consideration the statement that the major and minor tonal intervals of just temperament are not noticeably different. He had a string which was 1147 mm. long and which vibrated at a rate of 120 c.p.s. At its exact middle he placed a moveable bridge, so that he had two halves, each vibrating at 240 c.p.s. By moving the bridge 1 mm. toward one end, he got from the two halves of the string tones that were just barely distinguishable by a person with a 'delicate' ear who shut his eyes and avoided all distractions. A 2-mm. displacement of the bridge gave a difference readily ap-

parent even to unpracticed observers. In other words, at 240 c.p.s. the threshold for unpracticed observers is about .84 c.p.s., whereas the extreme threshold under the most favorable circumstances is about .42 c.p.s. It interested Delezenne that even the greater of these just noticeable differences is equivalent to only about half a comma, the comma being the difference in interval between the major and minor tonal intervals (see p. 317). Hence he concluded that these two basic intervals of just temperament are distinguishable even to unmusical persons.

There were after Delezenne several casual determinations of pitch discrimination. Seebeck, for instance, in 1846 put the thresholds at .36, .30 and .50 c.p.s. for 440, 500 and 1000 c.p.s., respectively, and Preyer, using Appunn reeds in 1876, verified these last two quantities. There was, however, no systematic determination of the limen as a function of frequency until Luft, in Wundt's laboratory in 1888, used tuning forks to get the differential thresholds for frequencies ranging from 32 to 1024 c.p.s. His threshold values were all very small; they lay between .15 and .44 c.p.s. Max Meyer in 1898 found comparably small values for Stumpf's ear. So for a time it was the general belief that the differential limen, at least in the middle of the musical range, is less than half a cycle per second.

Later the limens got larger! Vance with tuning forks in 1914 reported values more than ten times the size of Luft's, just as Stücker in 1907 had also found large limens. Then, with the substitution of the thermionic tubes for the tuning forks, the limens seemed to get a little larger still. The determinations of Shower and Biddulph (1931) are the standard for this last period of investigation. Here for comparison are three classical determinations of the threshold (in c.p.s.):

<i>Standard frequency:</i>	62	125	250	500	1000	2000
Luft (1888)	.15	.16	.23	.25	.22	.36
Vance (1914)	3.40	1.40	1.40	1.80	3.30	5.70
Shower & Biddulph at 40 db, (1931)	2.64	3.10	2.64	2.60	3.60	3.80

That these discrepancies are a function of the method of investigation seems now pretty clear. There are great individual differences in sensitivity, due to differences in the sensitivity of the ear and also in practice. Practised musical observers generally have small limens. Because they were averages of a group, Vance's

limens are too high to represent maximal sensitivity. Luft's limens, on the other hand, are plainly much too low, and this for the following reasons. His two tones for comparison were not well controlled, because he used hand-struck tuning forks, a technique that favors differences. He used, moreover, the method of limits, in which differences of any kind tend to be reported as differences in the fundamental pitch, and so to make the limen smaller. Vance used a method of constant stimuli where this error does not occur. Hand-struck tuning forks, furthermore, have in them a pattern of overtones or transient tones, many of which are the pitches for which the ear is most sensitive (around 2000 c.p.s.), although the fundamentals may be lower (30-1000 c.p.s.). In such cases the observer may make a discrimination of these higher transients that lie in the region most sensitive to difference of frequency, with the result that the limen comes out too low. Shower and Biddulph tried to get away from this difficulty by arranging to have the electrical frequency, which generated the tones, changed gradually in accordance with a sinusoidal function from the one frequency to the other. That method, to be sure, minimized the transients and consequently raised the limen, but it also introduced another artifact, for it is harder to observe continuous change than abrupt change. Presumably maximal sensitivity is represented by limens somewhat smaller than Shower and Biddulph's, and yet larger than Luft's.

When the differential thresholds are known, it is natural to try to count the j.n.d. of pitch. The total number of j.n.d. has sometimes been considered important for Helmholtz's resonance theory of hearing, which required a separate resonator with its independent nerve-fiber for every discriminable pitch. In 1863 Helmholtz could note only that the rods of Corti might be the resonators, since the 3000 rods would provide fifty resonators for every semitone within the seven musical octaves, leaving over 200 for the upper ranges. That was not quite enough rods, for Helmholtz took the differential limen of pitch to be about $\frac{1}{1000}$, which he figured would require some 64 discriminations per semitone. Thus he concluded, just as he had previously in his color theory, that intermediate qualities must depend on the ratio of excitation of adjacent receptors. Such reasoning seems to be necessary when the qualitative differentiation of sensation exceeds the spatial differentiation of the sense-organ. By 1870 new histological researches

had improved the situation, for Helmholtz could then say that there were 4500 outer rods of Corti or seventy-five per semitone, and also that the basilar membrane was probably the resonator.

It was Külpe who in 1893 took Luft's determinations and computed the total number of j.n.d. at about 11,000, a total for which the 4500 rods of Corti were clearly too few. Külpe noted, however, that there are in the inner ear between 16,000 and 20,000 hair cells to which the nerve-fibers run, and that is, of course, ample anatomical differentiation to take care of 11,000 tones. Nowadays it appears, because Luft's limens are too small, that 11,000 pitches is too large a requirement for maximal sensitivity. On the other hand, the 1500 j.n.d. provided by Shower and Biddulph's data are presumably not enough. The minimal differentiation of the ear must lie somewhere between these two values, presumably nearer the smaller.

Differential sensitivity to acoustic intensity is the second principal psychophysical problem in hearing. At first it was not recognized as important, but, after the impetus given the investigation of Weber's law by Fechner's publication of his *Elemente der Psychophysik* in 1860, this threshold received its share of attention. Because at the start there seemed to be no good way of controlling the intensity of tones, the first work had to be done with noise. It was assumed that the intensity of noise is proportional to the height of the fall of a ball upon a plate, or, if the height be kept constant, to the weight of the ball. The first standard apparatus was the sound pendulum of A. W. Volkmann, which Fechner described. It consists of a pendulum-hammer which has a hard rubber ball for a head and which can be allowed to drop from different angles of elevation, swinging against an ebony block at the base of the support of the pendulum. Later a double pendulum was made: two pendulums, one on each side of the block, arranged so that they could be released in quick succession to provide a pair of loudnesses for comparative judgment. There also came to be used a "fall apparatus," which Wundt presently standardized. This was a frame carrying at variable heights several electromagnetic releases, so arranged that steel balls could be dropped upon iron or ebony plates, striking once and then being caught in a padded box.

The first determinations of differential sensitivity for loudness were made by Renz and Wolf (1856) working under the direction of Vierordt at Tübingen. Their research, the second to employ the

method of right and wrong cases (as the method of constant stimuli was then called), was primarily concerned with the establishment of this new method, which Vierordt had invented. As stimulus Renz and Wolf used the sound of a ticking watch, varying the intensity by changing the distance. Their results (the average for the two of them) were these:

Ratio of two intensities	1000:920	1000:846	1000:778	1000:716
Per cent of right judgments	54.9	85.1	89.1	100.0

These frequencies are, therefore, one of the first psychometric functions ever obtained. They show the threshold (the point where the function would be 75 per cent) at a ratio of about 1000:870, i.e., a difference of 13 per cent.

More than twenty years later this experiment was repeated by Nörr, who also worked in Tübingen under Vierordt with the method of constant stimuli. He dropped lead balls on an iron plate, using intensities ranging from 2500 to 800,000,000 milligram-millimeters. He got a threshold (75 per cent of the judgments right) at about 5 per cent of the stimulus intensity. Because this value did not vary greatly throughout this range, he concluded that he had verified Weber's law.

When Wundt's laboratory was formally opened at Leipzig in 1879, it was natural that some of its energies should soon be turned to testing the validity of Weber's law. Thus there were, beginning with Tischer in 1883 and ending with Kämpfe in 1893, six studies of the measurement of differences of auditory intensities, all but one of them made at Leipzig. For the most part these studies dealt, not with differential limens, but with supraliminal sense-distances. Three intensities would be set up and the middle one adjusted until it bisected the difference in loudness between the first and the third. Then, if the middle stimulus turned out to be the geometric mean of the two extremes, Weber's law was supposed to be verified; but, if it turned out to be the arithmetic mean or something still else, then the test of the law was considered to have failed. Inasmuch as Merkel (1889) got one result and Angell (1891) got the other, the research cannot be said to have been conclusive.

Most of these investigators, however, also determined differential limens, which were considered to have established Weber's law for the middle ranges of intensity. The law is validated if the Weber fraction, the ratio of the magnitude of the differential

threshold to the magnitude of the stimulus, is constant. Lorenz (1885) got values for the fraction ranging from .25 to .38; Starke (1889), from .08 to .13; Merkel (1889), from .04 to .08. What was meant by verifying Weber's law was, usually, that the fractions were all of the same order of magnitude when the stimuli were not, and that there was no clear major trend of the fractions as a function of stimulus-magnitude. Thus Merkel used stimuli ranging from .5 to 5000 with the fraction varying irregularly back and forth between .0385 and .0625. Such instability of the fraction was, of course, probably the fault of the method. The height of the fall and the weight of the ball determine the energy delivered to the plate but not the acoustic energy that gets to the ear. There was, however, no better method in the early days.

After all, noises were used *faute de mieux* in this experiment. Tones were the proper material if a method could be found for controlling their intensities. Such a method was invented, as we have already seen, by Wien in 1889. With his telephone-and-resonator technique he varied the intensity of a tone of 440 c.p.s. from a value proportional to 1.6 up to a maximum proportional to about 10^{11} . Within these limits he determined twelve differential thresholds for which the Weber fractions ranged from .108 to .350. Wien's function is much more regular than Merkel's, and ten of his twelve values are all under .178—a degree of variability which in the nineteenth century meant that Weber's law had been verified.

With the introduction of the thermionic technique, Knudsen in 1923 arranged to have a variable resistance alternately shunted in and out of an oscillator circuit, in such a way that the observer heard a tone fluctuating in loudness whenever the resistance was great enough. As determining the differential threshold he chose the point where the fluctuation could be just perceived. Riesz in 1928 invented an entirely new method for the purpose of avoiding the transients introduced by Knudsen's technique. Having first given the observer two superimposed frequencies, so close together that only a single beating tone could be perceived, he then reduced the intensity of one frequency until the beats disappeared. The Weber functions of both investigations at the frequency of 1000 c.p.s. are shown in Fig. 55. Knudsen's results approximate the straight horizontal line of Weber's law more nearly than do Riesz's. The minimal Weber fraction in one case is .09, in the other .06. Thus all the results from Renz and Wolf to Riesz are consistent

with the conventional statement that auditory sensitivity is about $\frac{1}{10}$.

Using Shower and Biddulph's data for pitch sensitivity (a maximum of 1500 discriminably different pitches) and Riesz's data for intensive sensitivity (a maximum of 325 discriminably different

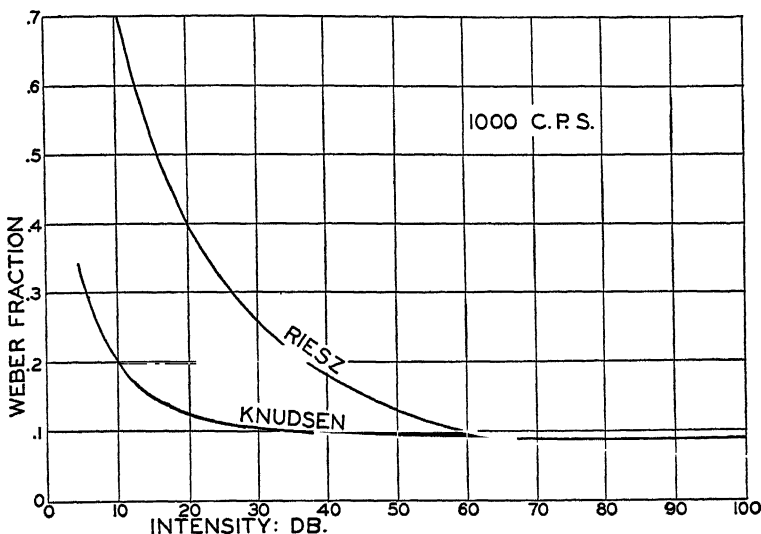


FIG. 55. AUDITORY WEBER FUNCTIONS OF KNUDSEN (1923) AND RIESZ (1928)

The abscissa-scale shows the intensity of the stimulus in decibels above the threshold of hearing. The ordinate-scale is the Weber fraction, the ratio of the differential threshold to the intensity at which it is obtained. These curves are for 1000 c.p.s.

loudnesses), Stevens got a total of about 340,000 discriminably different tones. Luft's and Knudsen's data would have given ever so many more—presumably several million. Helmholtz, being ignorant of the all-or-none action of the nerve-fiber, made no attempt to take special account of intensity in his theory of hearing. The modern problem is much more complicated than was his.

The most recent finding for differential sensitivity in both pitch and loudness is the work of Stevens, Morgan and Volkmann (1941), who have shown that sensory quanta of fixed size can be determined for each of these dimensions. Never, under their optimal conditions of observation, did they find a sensory difference when

the increment between tones was less than one quantum, and they always did find a sensory difference when the tonal increment was greater than two quanta. Under these conditions the psychometric function becomes, instead of a sigmoid curve, a straight line with abrupt corners at zero and 100 per cent.

Notes

Musical Notes

The text does scant justice to the elaborate and fascinating history of the establishment, as tonal elements, of the notes of the musical scale. See the histories of music; in particular, J. Hawkins, *General History of the Science and Practice of Music*, 5 vol., 1776; C. Burney, *A General History of Music*, 1776-1789, 4 vol., of which there is a 1935 reprint, 2 vol. (see there esp. I, 40-63, 286-294, 342-358, on Greek music); C. H. H. Parry, *The Evolution of the Art of Music*, 1918, esp. 14-46 on the scales; H. v. Helmholtz, *On the Sensations of Tone*, Eng. trans. from German, annotated by A. J. Ellis, 2 ed., 1885, esp. 234-249 (Helmholtz on the history of tonality), 310-330 and 421-513 (Helmholtz and Ellis on scales and temperament); A. J. Ellis, On the history of musical pitch, *J. Soc. Arts*, 28, 1880, 293-336, 400-403, which is more detailed than the appendices to Helmholtz. The complexity of the interval-problem—the attempt to retain simple ratios while gaining the variety of transposition afforded by equal intervals—is shown clearly by Ellis, who, in Helmholtz, *op. cit.*, 453-456, lists 155 intervals less than an octave which have musical significance. Of them 121 have actually been used in music!

On standards of pitch, see Ellis in Helmholtz, *op. cit.*, 493-513.

On the calibration of frequency, see D. C. Miller, *Anecdotal History of the Science of Sound*, 1935, 9-14, 39, 41, 55-58,

Physical Acoustics

On the history of the physics of sound and thus of the auditory stimulus, see the histories of physics, although in general they deal but briefly with acoustics. For an older point of view, see W. Whewell, *History of the Inductive Sciences*, 1837, II, Bk. 8 (also later eds.). Excellent is D. C. Miller, *op. cit.* Excellent but condensed is A. Wolf, *A History of Science, Technology, and Philosophy in the 16th and 17th Centuries*, 1925, 281-289; *idem*, in the *Eighteenth Century*, 1939, 172-176. Relevant is F. Cajori, *A History of Physics*, 2 ed., 1933, 35-43, 72-78, 104 f., 141, 283-289, 383 f.

Besides these secondary sources, see, on the vibration of strings and the relation of pitch to frequency, Galileo Galilei, *Discorsi e dimostrazioni matematiche intorno à due nuove scienze*, 1638, 138-150 (end of dialogue I); or Eng. trans., *Mathematical Discourses Concerning Two New Sciences Relating to Mechanicks and Local Motion*, 1730, 143-157, or the 1914 Eng. trans., 94-108; M. Mersenne, *Traité de la nature des sons et des mouvemens de toutes sortes de corps*, 1636, Bk. 1 and 3; *Harmonie universelle, contenant la theorie et la pratique de la musique*, 1636, Vol. I. The *Traité* gives the most acoustical (the least musical) information, but the *Harmonie* is the most frequently cited. In the volume I have seen, the *Traité*, although separately paged, is bound behind the title page of the *Harmonie*. Of course,

Galileo (1564–1642) antedates his pupil Mersenne (1588–1648), although his delayed publication was two years later. Galileo's brilliant argument for the Copernican theory in 1632, after he had promised the Pope in 1616 not to do it, brought him before the Inquisition at the age of sixty-eight in 1633, and into retirement under censure thereafter. He went blind in 1637, and sent the MS of *Two New Sciences*, which he had by him, out of Italy to the Elzevirs for printing in 1638. Four years later he died. The stringed instrument which he used for resonating to the squeaks of the chisel on the brass plate was a *cembalo*, a general term probably referring in this case to some antecedent of the harpsichord.

On the velocity of sound, see P. Gassendi, *Exercitationes paradoxicae adversus Aristoteleos*, I, 1624, [n.v.], reprinted in *Syntagma philosophicum*, 1658, [n.v.]; Mersenne, *Traitez, op. cit.*, Bk. 3 (the echo experiment is on p. 214).

On air as the medium of sound, see O. v. Guericke, *Experimenta nova (ut vocantur) Magdeburgica de vacuo spatio*, 1672, Bk. II, Chap. 15; R. Boyle, *New Experiments Physico-Mechanical, Touching the Spring of Air, and its Effects, Made, for the Most Part, in a New Pneumatical Engine*, 1660 (or Latin ed., 1661; or 2 ed., 1662), Experiment 27. Guericke precedes Boyle, in spite of the dates; in fact G. Schott published in 1657, an account of Guericke's experiments with which Boyle is supposed to have been familiar.

On resonance, harmonics and partials, see J. Wallis, Concerning a new musical discovery, *Phil. Trans.*, 12, 1677, 839–842, a trivial report which gains importance only by its priority; J. Sauveur, Des intervalles des sons, et son application à tous les systèmes et à tous les instruments de musique, *Hist. Acad. Sci. Paris*, 1701, 299–366, esp. 347–354, the first seri-

ous and thorough treatment of the problem.

For Fourier's theorem and series, see J. B. J. Fourier, *Théorie analytique de la chaleur*, 1822, Chap. 3, esp. sect. 6, (Eng. trans., 1878). Ohm's acoustic law is to be found in G. S. Ohm, Ueber die Definition des Tones, nebst daran geknüpfter Theorie der Sirene und ähnlicher tonbildender Vorrichtungen, *Ann. Phys. Chem.*, 135, 1843, 497–565. On tonal analysis in general, both physical and psychological, see H. v. Helmholtz, *Die Lehre von dem Tonempfindungen*, 1863 (or later eds., or Eng. trans.), Chap. 2–4. For a more recent mechanical analyzer and synthesizer than Lord Kelvin's, see D. C. Miller, *The Science of Musical Sounds*, 1916, 92–141 (for the 18 harmonics of a girl's face, 119). On electrical analysis by resonance, see S. S. Stevens and H. Davis, *Hearing*, 1938, 19 f., 39 f.

On the siren, see C. Cagniard de la Tour, Sur la sirène, nouvelle machine d'acoustique destinée à mesurer les vibrations de l'air qui constituent le son, *Ann. Chim. Phys.*, 2 ser., 12, 1819, 167–171; A. Seebeck, Beobachtungen über einige Bedingungen der Entstehung von Tönen, *Ann. Phys. Chem.*, 129, 1841, 417–436.

Rudolph Koenig and his work have been well described by D. C. Miller, *Anecdotal History (op. cit.)*, 85–92, et passim. See also Koenig's own collected researches in his *Quelques expériences d'acoustique*, 1882.

On the use of Lissajous figures in the calibration of tuning forks, see many textbooks of physics, e.g., W. Watson, *A Text-book of Physics*, 7 ed., 1920, 400–402; J. Duncan and S. G. Starling, *A Text Book of Physics*, 1936, 673–677. On the calibration of frequencies by Lissajous figures in the cathode ray oscillograph (with no sweep circuit and two frequencies impressed at right angles upon the electron stream), see Stevens and Davis, *Hearing*, 21–24, 40 f.

On the history of the development of thermionic vacuum tubes (1873-1929), see E. L. Chaffee, *Theory of Thermionic Vacuum Tubes*, 1933, 5-16. On the use and calibration of the tubes as audio-oscillators, see Stevens and Davis, *op. cit.*, 35-41.

Limits of Hearing

A good account of the experimental attempts from 1700 to 1900 to determine the upper and lower limits of hearing is given by E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 13 f., 24-36, 41-46. The older classical source is W. Preyer, *Ueber die Grenzen der Tonwahrnehmung*, 1876; *Akustische Untersuchungen*, 1879.

The early work of J. Sauveur on the limits is described by an anonymous reporter, *Sur la détermination d'un son fixe*, *Hist. Acad. Sci. Paris*, 1700, 134-143. Sauveur's paper on the harmonics of strings was not published until 1701. The other papers before Savart are E. F. F. Chladni (the great acoustician who discovered how to make sound pictures in dust on vibrating plates), *Die Akustik* (which Napoleon paid to have translated into French), 1802, 2, 27, 230, [n.v.]; J. B. Biot, *Précis élémentaire de physique expérimentale*, 1817, [n.v.], but see 3 ed., 1824, I, 344, 356.

F. Savart's first experiment with the 720-toothed wheel for the demonstration of the dependence of pitch on frequency and of the upper limit is *Sur la sensibilité de l'organe de l'ouïe*, *Ann. Chim. Phys.*, 2 ser., 44, 1830, 337-352 (or see the German *Ann. Phys. Chem.* 96, 1830, 290-304). His determination of the lower limit with the two-spoked wheel is *Sur la limite de la perception des sons graves*, *Ann. Chim. Phys.*, 2 ser., 47, 1831, 69-74 (or the German *Ann. Phys. Chem.*, 98, 1831, 596-600).

For H. Helmholtz on the lower limit, see his *Die Lehre von den*

Tonempfindungen, 1863, Chap. 9; or, for the use of the Koenig fork, 3 ed., 1870, or Eng. trans., Chap. 9.

For W. Wundt's determination of the lower limit by difference tones, see his *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, 362; for his use of the large Appunn fork, *idem*, 3 ed., 1887, I, 423; for his use of the Appunn lamella, *idem*, 4 ed., 1893, I, 450 f. For Preyer's determinations, see *op. cit.*, 1876, 1-25; *op. cit.*, 1879, 1-10.

F. Galton's description of his whistle in 1876 is reprinted in his *Inquiries into Human Faculty*, 1883, 375-378, where see also 38-40.

For a rather recent acceptance of 50,000 c.p.s. as the upper limit by a careful systematist, see E. B. Titchener, *A Text-book of Psychology*, 1910, 98; and for the later view that 20,000 is about right, see J. Fröbes, *Lehrbuch der experimentellen Psychologie*, 3 ed., 1923, 102.

The first suggestions of a sensitivity curve for hearing are to be found in the experimental determinations of A. Toepler and L. Boltzmann, *Ueber eine neue optische Methode, die Schwingungen tönender Luftsäulen zu analysiren*, *Ann. Phys. Chem.*, 217, 1870, 321-352, esp. 349-352; Lord Rayleigh (J. W. Strutt), *On the minimum current audible in the telephone*, *Phil. Mag.*, 38, 1894, 285-295, esp. 294 (or *Scientific Papers*, IV, 1894, 109-118, esp. 117). The first good audibility function was worked out by M. Wien, *Ueber die Empfindlichkeit des menschlichen Ohres für Töne verschiedener Höhe*, *Arch. ges. Physiol.*, 97, 1903, 1-57, esp. 20-31.

On audiograms and their implications about the limits of hearing, see R. L. Wegel, *The physical examination of hearing and binaural aids for the deaf*, *Proc. nat. Acad. Sci. Wash.*, 8, 1922, 155-160, the original paper which led off for a great many others, mostly at first from the Bell Telephone Laboratories; H. Fletcher,

Physical measurements of audition and their bearing on the theory of hearing, *J. Franklin Inst.*, 196, 1923, 289-326, esp. 291-293, a more general and clearer presentation of the results of Wegel and the Bell Laboratories; also Fletcher, *Speech and Hearing*, 1929, 132-144. On the general problem of the intensive threshold and the audibility function, see L. J. Sivian and S. D. White, On minimum audible sound fields, *J. acoust. Soc. Amer.*, 4, 1933, 288-321. For general discussion and some of the more recent results, see S. S. Stevens and H. Davis, *Hearing*, 1938, 41-68, 117-127, esp. 41-46, 55-64, 67 f., 123-127. The way in which the range of hearing increases with intensity is exactly comparable to the way in which the size of the retinal color zones increases with intensity; see pp. 173-176.

On the maximal sensitivity of the ear, see A. Wilska, Eine Methode zur Bestimmung der Hörschwellen-amplituden des Trommelfells bei verschiedenen Frequenzen, *Skand. Arch. Physiol.*, 72, 1935, 161-165.

Differential Sensitivity

On the history of differential sensitivity to frequency, see E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 232-245; T. F. Vance, Variation in pitch discrimination within the tonal range, *Psychol. Monogr.*, No. 69, 1914, 115-149, esp. 115-120. On the recent facts, see S. S. Stevens and H. Davis, *Hearing*, 1938, 84-89, 94-98.

The conventional starting point for the history of pitch discrimination is C. É. J. Delezenne, Sur les valeurs numérique des notes de la gamme, *Recueil des travaux de la Société des Sciences, de l'Agriculture et des Arts de Lille*, 1827, 1-56, esp. 4-6. The date usually given, 1826, is wrong; this volume for 1826-1827 is not arranged chronologically. The subsequent determinations mentioned

in the text are E. Luft, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Phil. Stud.*, 4, 1888, 511-540, esp. 528; M. Meyer, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Z. Psychol.*, 16, 1898, 352-372, esp. 352-361; H. Stücker, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, *Z. Sinnesphysiol.*, 42, 1908, 392-408; T. F. Vance, *op. cit.*, 1914, esp. 126 f. The first pitch limens with the thermionic tubes were got by Knudsen (*op. cit. infra*, 1923) as more or less secondary to his thresholds for loudness. The standard research on pitch limens with thermionic control is E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear, *J. acoust. Soc. Amer.*, 3, 1931, 275-287.

On the question as to how the discrepancies among the various determinations of the differential limens for pitch are to be explained as functions of experimental artifacts, see E. G. Boring, The size of the differential limen for pitch, *Amer. J. Psychol.*, 53, 1940, 450-455. On the way in which continuous change makes for larger limens than abrupt change, see L. W. Stern, *Psychologie der Veränderungsauffassung*, 1898, esp. 208-243. On the way in which the transients of abrupt change work the opposite way, see Shower and Biddulph, *op. cit.*

For H. v. Helmholtz's original suggestion that the 3000 rods of Corti would be enough resonators to provide 33 discriminations per semitone, see his *Die Lehre von den Tonempfindungen*, 1863, Chap. 6 (n.v., but p. 219 in 2 ed., 1865); or later eds. or Eng. trans., Chap. 6, for 4500 rods and the discussion of the resonating basilar membrane. For O. Külpe's computation that, on Luft's figures, there ought to be 11,064 different tones (besides 553 different noises) and for his application of the computation to the Helmholtz theory of hearing, see his *Grundriss der Psychologie*, 1893, 108-111, 114

(Eng. trans., 104-107, 110 f.). It must be remembered that this was the period when elementism in psychology was at its height; psychologists sought to enumerate the conscious elements in some sort of a psychological Mendeleëv table. Thus E. B. Titchener in 1896, seeking the total, added all the j.n.d. and discrete qualities of all the senses to get a total of "more than 44,435" conscious elements. See p. 10. It is interesting to note the confusion of thought at that time, for some psychologists recognized the fact that sensory series are continuous and yet treated the j.n.d. as if they were discrete and quantal.

For general discussion of differential intensive acoustic sensitivity, see W. Wundt, *Grundzüge der physiologischen Psychologie*, 6 ed., I, 1908, 648-656; and for recent facts, S. S. Stevens and H. Davis, *Hearing*, 1938, 136-140. About the form of the Weber function in general, see A. H. Holway and C. C. Pratt, The Weber-ratio for intensive discrimination, *Psychol. Rev.*, 43, 1936, 322-340. On Volkmann's sound pendulum, see G. T. Fechner, *Elemente der Psychophysik*, 1860, 176-178; or for the double sound pendulum, Kämpfe, *op. cit. infra*. On a Fallapparat for noise intensities see Tischer, *op. cit. infra*; on Wundt's form of the Fallapparat, see Starke, *op. cit. infra*.

The early investigations under Vierordt's direction were T. Renz and A. Wolf, Versuche über die Unterscheidung differenter Schallstärken, *Arch. physiol. Heilk.*, 15, 1856, 185-193, esp. 190-192; C. Nörr, Experimentelle Prüfung der Fechner'schen Gesetzes auf dem Gebiete der Schallstärke, *Z. Biol.*, 15, 1879, 297-318, esp. 306-314.

The investigations of noise that centered in or upon Wundt's laboratory were E. Tischer, Ueber die Unterscheidung von Schallstärken, *Phil. Stud.*, 1, 1883, 485-542; G. Lorenz, Die Methode der richtigen und falschen Fälle in ihrer Anwendung auf

Schallempfindungen, *ibid.*, 2, 1885, 394-474, esp. 470-474; P. Starke, Die Messung von Schallstärken, *ibid.*, 3, 1886, 264-304, esp. 302-304; J. Merkel, Die Abhängigkeit zwischen Reiz und Empfindung: Schallreize, *ibid.*, 5, 1889, 499-557, esp. 511-515; F. Angell, Untersuchungen über die Schätzung von Schallintensitäten nach der Methode der mittleren Abstufungen, *ibid.*, 7, 1891, 414-468, esp. 425-431; B. Kämpfe, Beiträge zur experimentellen Prüfung der Methode der richtigen und falschen Fällen, *ibid.*, 8, 1893, 511-591, esp. 584-588.

The more recent studies on tone are M. Wien, Ueber die Messung der Tonstärke, *Ann. Phys. Chem.*, 272, 1889, 834-857, esp. 845-848; V. O. Knudsen, The sensibility of the ear to small differences in intensity and frequency, *Phys. Rev.*, 21, 1923, 84-103, esp. 91-94; R. R. Riesz, Differential sensitivity of the ear for pure tones, *ibid.*, 31, 1928, 867-875, but Riesz does not give his actual values which are printed in Stevens and Davis, *op. cit.*, 140.

On the total number of discriminable pitches and intensities, see Stevens and Davis, *op. cit.*, 152 f., but cf. also Boring, *loc. cit.*

For the determination of fixed quanta for both pitch and loudness, see S. S. Stevens, C. T. Morgan, and J. Volkmann, Theory of the neural quantum in the discrimination of loudness and pitch, *Amer. J. Psychol.*, 54, 1941, 315-335.

Tonal Scales

The text omits mention here of tonal scales for the reason that the important developments are all recent. The problem goes back to Fechner, who assumed that all j.n.d. are equal and, on the basis of Weber's law, built up a scale of intensities (Fechner's law) by counting the j.n.d. from the absolute threshold up to the intensity being meas-

ured. He could not do much about pitch because there were in 1860 no good data. Weber and he did consider that pitch follows Weber's law, because the musical intervals are proportional to the differences of the logarithms of the frequencies that determine them; but much later it was realized that pitch is not intensity and, whatever the form of its functional relation to the stimulus, could not, therefore, be an instance of Weber's law. On this see E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 232-235. A test of Fechner's assumption that all j.n.d. are equal would lie in the discovery of whether sense-distances, judged equal, yield a sensory scale like that of the summed j.n.d. If they do, the bisection of a sense-distance should be given by the geometrical mean of the extreme stimuli. Most of the papers listed above in Wundt's *Phil. Stud.* attack this problem, though with different results. See Wundt, *loc. cit.* There was also the question as to whether the same musical interval in different parts of the scale represents always the same pitch difference, so

that a frequency that is the geometric mean between two other frequencies would always establish two equal pitch differences. Wundt thought not; Stumpf thought they did. About this point raged one of the bitterest controversies of this period. See Titchener, *op. cit.*, 241-245; C. C. Pratt, The bisection of tonal intervals smaller than an octave, *J. exp. Psychol.*, 6, 1923, 211-222; E. G. Boring, The psychology of controversy, *Psychol. Rev.*, 36, 1929, 97-121, esp. 107-113. On the whole, however, there has been much less use of sense distances for psychophysical measurement than the attempt to quantify psychology ought to have accomplished. Recently Stevens and his associates have been creating scales of pitch and of loudness, scales based on the equation of sense-distances and not of j.n.d. They have even created a unit of sense-distance for pitch, the *mel*, and another for loudness, the *sone*. See Stevens and Davis, *op. cit.*, 76-84, 112-123; S. S. Stevens and J. Volkman, The relation of pitch to frequency: a revised scale, *Amer. J. Psychol.*, 53, 1940, 329-353.

Chapter 10

AUDITORY PERCEPTION

IN THIS chapter we consider the history of the older problems of auditory perception: beats and combination tones, timbre and fusion, the nature of vowels, the attributes of tones and the localization of sound.

Beats and Combination Tones

Because the frequency of the first difference tone for any two generating tones is the same as the frequency of the beats for these generators, the histories of the knowledge of beats and of combination tones are related, even though psychologists since Helmholtz have had adequate evidence that the two phenomena are distinct.

We have already seen that it was Sauveur who, in 1701, first described the complex vibrations of a string and the relation of the harmonics to the fundamental (see pp. 325 f.). It was in connection with these experiments that Sauveur discovered beats. Trying to establish standards of frequency for music, he found beats for the organ pipes that were slightly out of tune. His earlier paper in 1700, *Sur la détermination d'un son fixe*, not only described beats and showed that their frequency equals the difference in frequency between the two primaries, but actually used the beats for tuning and for the measurement of mistuning. He explained the beats correctly as the alternation of reenforcement and cancellation, and, knowing nothing at that early date about Ohm's acoustic law, he found the explanation both adequate and obvious.

The difference tone was discovered, quite independently of the discovery of beats, by the Italian musician, Tartini, as he taught himself the violin in the monastery of Assisi. With double-stopping, he observed, there could be heard a *terzo suono*, a third tone additional to the proper tones of the two bowed strings, and this third tone he came to use in checking the tuning of the strings.

Tartini himself later dated his discovery in 1714, and in 1728 he founded a school for the violin in Padua, where he taught this method of tuning. It was not until 1754, however, that he described the phenomenon in print. Then he published a treatise on music, introducing the *terzi suoni* in the first chapter as basic to his theory of harmony. It was thus that these extra tones came later to be called Tartini's tones, a term which is still occasionally met.

Because Tartini did not publish anything about the third tones until twenty-six years after he had begun to teach students to notice them, it is natural that other published references to the phenomenon should precede Tartini's. Sorge, for instance, mentioned the existence of these tones in 1744 and the next year named them *grave harmonics*. He has, therefore, often been called their discoverer, but, not claiming discovery, he was probably only noting a fact that some musicians had learned from Tartini or from Tartini's students. It is, moreover, plain that a French savant, Romieu, discovered difference tones in the early 1740's without any knowledge of Tartini's teaching, although he may have learned something from others later, since, when he published in 1751, like Sorge he used the phrase *grave harmonics*. Thus the order of originality—Tartini-Romieu-Sorge—reverses the order of the publications of those three men.

Romieu, in his paper of 1751—in an obscure volume which has only recently been given its place in this history—actually sought to relate the grave harmonic to beats. He noted the obvious fact that the pitch of the harmonic corresponds to the frequency of the beats. This view, that the grave harmonic is a "beat tone," is usually attributed to Lagrange, who expounded it in 1759, noting that beats which are too fast to be heard ought to give rise to a third tone. To Thomas Young, who supported the same theory in 1800, it is probably due that the problem received in the early nineteenth century the attention it deserved.

It was Vieth who coined the term *combination tone* in 1805 when he discussed the results of Young and another investigator. The older term was *grave harmonic*, but Helmholtz ruled in favor of Vieth's phrase because it could be made to include summation tones as well as difference tones.

In 1832 Hallström extended the beat-tone theory to its logical conclusion. If two primary tones sounding together beat, and the beats are so fast that they create a beat-tone, may not this beat-

tone, itself beating with one or the other of the primaries, establish a second beat-tone? And, if there can thus be a second combination tone, why can it not beat with the first combination tone or a primary tone giving rise to a third combination tone? And is there any limit to this progression of beats and resultant combination tones? Actually Hällström specified only four terms in this series, although he wrote *und so weiter* after it. If s represents the frequency of the higher primary tone, and r the frequency of the lower primary, then the first four combinations, Hällström pointed out, are the first four differences, as follows:

Basic tones		Combination tones	
s	r	$s-r$	I
r	$s-r$	$2r-s$	II
s	$2r-s$	$2(s-r)$	III
$2r-s$	$s-r$	$3r-2s$	IV

Here the second combination tone is dependent upon the first; the third and fourth are dependent upon the second. Differences which coincide with tones already generated Hällström omitted. His experiments consisted of the study of various tonal dyads, in which he was able to identify the existence of one or two—never more than two—of these four combination tones.

Hällström published in 1832. Ohm's acoustic law was promulgated in 1843. In 1856 Helmholtz published his investigations of combination tones, a paper of primary importance for the following reasons. (a) It reported the discovery of *summation tones*—tones of which the pitch corresponds to a frequency which is the sum of the frequencies of the two primary tones. This discovery led Helmholtz to invent the term *difference tone* for the Tartini tones and to include both summation tones and difference tones under the rubric *combination tones*. The summation tones, being weaker than the more obvious difference tones, are harder to observe, but Helmholtz's discovery is fully established by modern research. (b) Helmholtz also verified the difference tones of higher orders—the tones that Hällström had described and still others based upon them. He noted how these tones would vary for the musical intervals: none for the octave, one for the fifth, two for the fourth, three for the major third and the major sixth, four for the minor third, six for the minor sixth. (c) Under certain conditions he succeeded in getting objective combination tones which, by resonators, could be picked out of a complex physical wave prior

to the intervention of the human ear. The necessary conditions for them seemed to him to be met when a large body of air is set in vibration, as in the wind-chest of a harmonium. (d) Finally, he stated correctly the physical conditions which would have to be fulfilled if combination tones are to be heard: the displacement of the vibrating body must fail to be proportional to the force actuating the displacement. Such a statement is equivalent to the modern formula for combination tones: they are added to the acoustic complex of the two primaries only when the transmitting mechanism distorts the wave-form. Herein lies the explanation as to why difference tones occur in what is really a contradiction of Ohm's law.

According to a strict and simple interpretation of Ohm's law, neither beats nor difference tones should be heard. If the external stimulus consists only of two superimposed harmonic frequencies, then we should hear only two pure tones. The fact that the amplitude of the successive waves in a combined frequency seems to vary periodically has nothing to do with the matter, although the visual appearance of the graphed waves makes it look as if beats should be heard. Beats actually occur because two frequencies act upon the same resonator—in the ear, or sometimes outside the body in a physical resonator, for tuning is never so sharp that a resonator responds only to a single frequency and not to other frequencies adjacent to the most natural period of the resonator. Combination tones actually occur, not because there are beats which, having a rate above the lower limit of hearing, produce a tone, but because at some place—in the ear and also, as Helmholtz knew, external to it—there is distortion of the wave-form, a distortion that yields difference-frequencies and summation-frequencies in Fourier's analysis, in resonance, and thus also under Ohm's law when the distorted wave is regarded as the stimulus instead of the original wave. It was quite clear to Helmholtz that subjective beats depend upon the properties of the auditory mechanism, that combination tones depend on distortion, that the two are different phenomena, and that the first difference tone is not a 'beat-tone.' Others after him had less insight.

Helmholtz explained beats, as many do today, by his resonance-place theory: the adjacent frequencies actuate adjacent regions of the organ of Corti, and the overlapping parts activate the afferent nerve-fibers with pulsating intensities. Such a special situation is

found outside the body only when the frequencies act on the same resonator, as in the amplitude modulation of ordinary radio broadcasting. Helmholtz was at considerable pains to demolish the beat-theory of the first difference tone by showing that beats up to 132 per second can be perceived clearly as roughness. If the lower limit of hearing lies at 30 c.p.s., then there must be a region between 30 and 132 where both beats and a difference tone corresponding to the beat-frequency can be heard simultaneously. This phenomenon Helmholtz established as an observed fact.

After Helmholtz there was until recently no great further insight into the nature or significance of either of these phenomena. Both Rudolph Koenig (1876) and Preyer (1879) argued that combination tones are subjective, but those were the days when the dualism of mind and matter pervaded the thought of all the wise men. To argue that a phenomenon was subjective was to imply that it was therefore not objective, even though one might seek for an explanation of the phenomenon in a physiological mechanism within the organism, a mechanism which would be wholly physical if transferred outside the subject's skin. Koenig also defended the beat-tone theory of the difference tone, the theory of Romieu, Lagrange and Young. Nevertheless, Helmholtz was right and both these others wrong, as the modern user of thermionic apparatus well knows. The beat-frequency oscillator gives electrical 'beat-tones' only because it introduces distortion into the wave that is compounded of the two primary frequencies.

How extremely complicated distortion may make the sound-wave has been shown by modern methods. Newman, Stevens and Davis (1937) used an electrical wave-analyzer to discover the components of the electrical potential occurring at the inner ear of a cat when two primary tones of 700 and 1200 c.p.s. were sounded at its outer ear. Discarding all combination tones in which the intensity was less than one per cent of the fundamental and all with a frequency above 8000 c.p.s., they nevertheless found 64 of the 68 possible additional frequencies. This number is so large because distortion adds to the primaries upper harmonics as well as combination tones, and thus makes possible combination tones between these harmonics.

In modern radio broadcasting we have both amplitude modulation and frequency modulation. In the first, the amplitude of a carrier wave of high frequency—too high a frequency to be heard

itself—is varied in accordance with the wave-form of the sound which is to be broadcast. In the latter, the frequency itself of the carrier wave is thus varied. It can be shown mathematically and physically that both forms of modulation produce similar results, electrical oscillations at the frequencies of the impressed modulation which, under Fourier's analysis or, in sound, under Ohm's law, break up into the desired components. The physics of these relationships is too involved for exposition here, although its mention is in point because it shows how far from the truth were Romieu, Hällström, Koenig and Preyer—but not Helmholtz. Beats and combination tones are physical phenomena, special cases of modulation. They are subjective only as the organism provides the physical conditions which produce them for a resonating organ in which the localization of resonance is not too sharply restricted.

The precise physiology of beats and combination tones will be clearer to the reader after the next chapter has been read. Helmholtz thought that the distortion necessary for combination tones occurred in the middle ear; recent research, however, indicates that some of it depends on the transmission mechanism of the middle ear and some on the properties of the inner ear. The only physiological theory of beats that has ever been offered is Helmholtz's in terms of his place-resonance theory (*vide supra*).

Timbre

Since any auditory stimulus can theoretically be reduced to a complex of simple harmonic vibrations, and since Ohm's law is ordinarily approximated, most of the phenomena of hearing can be understood as resultants of the interaction of separate tonal components. Beats and combination tones are consequences of such interaction, as are also the phenomena of timbre, fusion, noise and masking. With these latter four we occupy ourselves in the following sections.

The timbre of a note, its *Klangfarbe* or musical quality, depends wholly or in part, according to the way in which the word is defined, upon the pattern of harmonics that are present with the fundamental. The historical background of this problem lies in Galileo's discovery of resonance, in Mersenne's analysis of the fundamental nature of the vibrating string, in John Wallis's discovery of the harmonic character of overtones, and in Sauveur's

establishment of the relation of harmonics to the fundamental (pp. 324–327). The fact of timbre, of qualitative differences other than pitch, was recognized early; in fact the French word *timbre* and, to a lesser degree, the German *Klang*, are acoustic words which express exactly this difference. The mutual significance of all these facts could, however, hardly have been realized before the establishment of Ohm's law in 1843. It was subsequently Helmholtz who saw that, since all sounds can be reduced to a complex of harmonic components, there are only three dimensions to the auditory stimulus: the frequency, the amplitude, and the combination of different frequencies. So he looked to the pattern of partials in a musical note to explain its *Klangfarbe*. Actually his first paper on this subject (1859) was an attempt to account for the *Klangfarbe* of the different vowel sounds in terms of the relative strength of particular partials in each of the principal vowels. Later in the *Tonempfindungen* this theory led to his identification of *Klangfarbe* with the pattern of partials. His logic was simple. If musical instruments, human voices and vowels can differ in timbre from one another at any pitch and at any loudness, then there is nothing left but the pattern of the different pitches to distinguish them. This theory has stood the test of time.

The theory was not, however, quite so easily accepted as this brief statement seems to indicate. In order to test it Koenig built the clang synthetizer (Fig. 51, p. 331). There was, moreover, another dimension of the stimulus that might have entered in: the phase relation of the component tones. Helmholtz was at great pains to show that this variant did not alter the sound of clangs or of musical combinations of tones.

Helmholtz's emphasis upon the prominence of harmonics in all musical notes led the psychologists of the late nineteenth century to stress the *pure tone*. Inasmuch as the weakly struck tuning-fork or the weakly blown bottle gives a fundamental almost without harmonics, it was supposed that absolute purity could be had by hearing such a tone through a resonator tuned to the specific frequency. It is now known, however, that even a 'pure' stimulus delivered to the outer ear is distorted by transmission through the auditory mechanism and that this distortion, under Ohm's law, is equivalent to the addition of harmonics. Since with any strong activation this distortion is bound to occur, it is safe to say that no loud pure tone has ever been heard.

It is difficult to decide who discovered these *aural harmonics*. Helmholtz, who, as we have seen, was aware that the difference tone depends upon distortion, in the third edition of the *Tonempfindungen* (1870) added to the section on combination tones an appendix which shows that certain harmonics would be generated by distortion and which gives the mathematical equation that describes their occurrence; nevertheless he left the matter as if these partials were to be regarded as combination tones. Lord Rayleigh (1896) supported Helmholtz's argument, but also failed to make it clear that the aural harmonics are not necessarily difference tones or summation tones. In 1924 Wegel and Lane, also interested in the consequences of excitation by two simultaneous frequencies, were able, with two generating stimuli of 700 and 1200 c.p.s. to find in the ear altogether 19 resultant frequencies, which included the second and third harmonics of both generators. They used a third exploring stimulus, varied its frequency and noted when the subject could detect beats. Wegel and Lane did this research in the Bell Telephone Laboratories, from which in 1929 Fletcher published his *Speech and Hearing*, devoted in great part to the summary of the recent acoustical research of those Laboratories. In this book for the first time there was a clear account of the subjective harmonics which, because of the non-linear transmission of the ear, occur for a single pure generating tone without difference tones or summation tones. It was Stevens and Newman who substituted the term *aural harmonics*, since the word *subjective* seems inappropriate for a phenomenon whose physical cause is so well understood.

Tonal Fusion

Closely related to the problem of timbre is the classical problem of tonal fusion. Are some tonal complexes more unitary than others? Ordinarily the partials of a note are so intimately fused that they cannot be heard separately except by special means. That is why, until Helmholtz insisted upon it, their great importance was not recognized. Sauveur had shown how the partials can be isolated physically in a plucked string by damping the string at a node with the light contact of a brush: if one touches the string at one-third its length only the third partial, the sixth, and those for other multiples of 3 are left. Then the observer, knowing what to listen for, can hear the partial in the tonal complex

without the damping. In fact, Ohm's law would not be valid if the ear could not make this analysis.

Helmholtz did not write about fusion, but about *consonance* he had a great deal to say. The consonant intervals, according to accepted musical principles, have simple ratios; the simpler the ratio, the greater will be the number of coinciding partials for the two components. In the octave, the most consonant interval, with its frequencies in the ratio of 2:1, all the partials of the upper note coincide with partials of the lower, so that the two complexes interpenetrate each other maximally. In the fifth (ratio 2:3) one-half of the partials of the upper note coincide with one-third of the partials of the lower, whereas in the tritone, (*f-b*, ratio 32:45) only $\frac{1}{32}$ of the partials of the upper note coincide with $\frac{1}{45}$ of the partials of the lower. The simpler the ratio, the greater the degree of interpenetration. Thus it may be said that Helmholtz both discovered and advertised the intimate fusion of harmonics in a clang and in the musical consonances.

On the other hand, the formal doctrine of tonal fusion is peculiarly Stumpf's, who formulated it in 1890 in the second volume of his *Tonpsychologie*. Stumpf held that tones tend to fuse, to interpenetrate each other, with the result that the total perception becomes something different from the mere concurrence of its components (as the Gestalt psychologists also said later), and that different combinations vary in their degrees of fusion. Fusion, Stumpf thought, is not unanalyzability, because the expert observer can analyze the best fusion, noting its degree; nevertheless, it is true that unmusical persons have difficulty in analyzing the good fusions. Stumpf was willing to distinguish five gross degrees of fusion as follows. (1) The octave gives the best fusion. His unmusical observers might mistake it for a single tone three-fourths of the time. (2) The fifth is the next best fusion, being mistaken for one tone about half the time. (3) The fourth is still poorer. (4) Then come the major and minor thirds and sixths. (5) At the bottom of this series lie all the other intervals, including the seconds and sevenths, which nevertheless show some degree of fusion, being mistaken for a single tone perhaps 10 or 15 per cent of the time by unmusical persons. Now this list follows Helmholtz's order of decreasing consonance in that the number of partials common to the two components diminishes in the successive groups, but Stumpf held that the harmonics do not determine the fusion.

Stumpf's chief laws of fusion were these. (a) Degree of fusion is a function of the vibration ratio of the components. Hence fusion is independent of the region of the musical scale in which the tonal interval occurs. This law Stumpf modified with the observation that the degree of fusion is not greatly altered by slight mistuning, a modification which allows the same degrees of fusion to the just and the tempered intervals and also contradicts the statement that the simplest ratios have the greatest fusion. For instance, the just third has the ratio 4:5, but the equally tempered third has the ratio $1:\sqrt[3]{2}$ or about 1:1.259921. (b) Degree of fusion is the same for intervals beyond an octave as for intervals within an octave; that is to say, the degree is the same for the third and the tenth, the fifth and the twelfth. This rule limits the first rule to intervals within the octave. (c) Degree of fusion is not affected by timbre. Stumpf's belief that the phenomena of fusion can be demonstrated with pure tones is a direct contradiction of the belief that fusion depends on coincident harmonics, but Stumpf did not, of course, know about aural harmonics. Finally Stumpf said (d) that fusion is independent of the absolute intensity of the constituent tones and their relative intensities; (e) that it is not affected by relative location of the sources of tone; (f) that it occurs when one tone is brought to one ear and another to the other ear; and (g) that imagined tones have the same degree of fusion as perceived tones. Although in imagining two simultaneous tones, one is constrained, Stumpf said, to hear the proper degree of fusion, one may not hear at all the beats which would inevitably occur in the perception.

Stumpf was an expert musician and a phenomenologist truly in the tradition of Goethe (cf. pp. 112-115). The details of his laws depended upon his own confident observation and but little on the confusion of single tones with dyads by unmusical persons. For several decades his theory of tonal fusion stood out as the best example, in an introspective psychology that was largely analytical, of the synthetic process among conscious elements. Now his theory has lapsed, not so much from confutation as from disuse, because of its failure to relate itself significantly to other phenomena of tonal perception. The different intervals, of course, are separately recognizable to musicians; Pratt showed in 1921 how they differ from each other in their attentional analyzability, in the disparity of their two pitches, in the degree of roughness occasioned by the

beats among their harmonics (which we now know may be the aural harmonics of tones that are pure outside the ear), and in the pleasantness associated with these patterns by musical observers. Edmonds and Smith (1923) tried to find adjectives to characterize these perceptions: the octave was "smooth," the fifth "hol-low," the fourth "coarse," the thirds "mellow," the sixths "luscious," the sevenths "astringent," and the seconds "gritty." Brues (1927) showed that the degree of fusion, as determined by the comparison judgments of laboratory observers (not by the confusion of two tones with one by unmusical persons) varies continuously throughout the octave, with maxima at unison, the octave and somewhere near the middle of the octave, with two minima in between (often near the second and seventh), and with quarter-tone intervals giving about the same degree of fusion as the adjacent musical intervals. Stumpf—who never plotted a curve of degrees of fusion—would have had fairly narrow peaks with different heights, all ascending from a base level for the seconds, sevenths and non-musical intervals. In 1934 Pratt dismissed tonal fusion from psychology on the ground that Stumpf's theory was never clear, that his degrees of fusion could not be verified, and that in general all musical dyads are about equally unitary if the roughness of beats be ruled out of consideration and if the uniqueness of the octave be granted.

What has happened is that Stumpf's theory of fusion has disappeared along with the cult of expert introspective observation. The expert observer is now the experimenter, not the subject, who may indeed be a rat. Stumpf's musical training had become his prejudice; he knew too much about tones to observe their natural properties in their purity. What we have remaining is the fact that attentional analysis of tonal complexes, under Ohm's law, varies in difficulty. The components mask each other (*vide infra*). Stimuli whose effects are but little differentiated in the organ of hearing are not easily discriminated (a principle of which the place-resonance theory of hearing makes use). Stimuli constantly associated in experience may not be easily separable, and the aural harmonics establish associations—especially for the octave—which were not supposed to exist when it was thought that tones could be 'pure.' Thus the demise of this once important theory goes to show how far psychology has advanced from the phenomenonological observation of the expert observer toward the objective experimentation in

which both the human and animal subjects are relieved from the responsibility of being scientifically sophisticated.

Noise

The psychology of sound has had mostly to do with tones, although nearly all sounds in nature are noises. It was, of course, Ohm's law that created this situation, for thereby it became possible to deal with a continuative sound as a sum of tones, provided exceptions, like beats, were given special consideration. Even the combination tones, which seemed to be exceptions, fell under Ohm's law when it was realized that they are Fourier components of a distorted wave-form. Are noises also complex fusions of tones?

There were two problems. The first concerned the continuity of noise and tone. Noises may be tonal: a thud—bang—pop ascends in pitch, as does also a series of continuative noises, like a rumble—rattle—hiss. Some of these Helmholtz mentioned, together with the fact that noises can be built up from tones by striking a large number of piano keys at once. Does this continuity from the indefinite pitch of noise to the definite pitch of tone mean that the tone and the noise belong in the same class? Here was one problem. The other concerned the organs of perception. Are there different sense-organs for tone and noise?

Helmholtz, who in 1863 described the tonality of noises, was inclined to the view that there might be different organs for tone and noise, that the ampullae of the semi-circular canals or perhaps the vestibular organs might mediate the perception of noise. Holding that periodic wave-forms give rise to tones and aperiodic wave-forms to noise, he implied a continuity between noise and tone, for there can obviously be intermediates between these two extremes. In 1875 Exner, using Savart's spoke (see p. 334), found that the successive noises of a card or a piece of wood, striking the rotating spoke, could be distinguished when the interval between hits was greater than .002 sec. In other words, tonality begins to come in, as Savart's spoke speeds up, before noise completely disappears. So impressed was Helmholtz by this evidence of continuity that he changed his mind in 1877 about the organ of noise, concluding that noise like tone is mediated by the cochlea. He stressed then the fact that the aperiodic vibrations of noise are more or less indeterminate in pitch, whereas the periodic vibrations, no matter how complex,

are determinate. Stumpf in 1890, relying as usual on his expert introspection, argued that noise is not pitch, even though one of them may enter into a perception as the other passes out, and that their perceptual disparity indicates the existence of separate organs for them. Külpe (1893) supported the essentials of Helmholtz's final conclusion.

In 1913 E. R. Jaensch undertook a study of vowel sounds in which he presented the view that the vowels are noisy tones, being partially aperiodic. What he did was to construct a photoelectric siren. He cut a sine-curve into the edge of a circular disk, rotated the disk so that the undulating edge, placed in a beam of light, caused the light to vary sinusoidally, let the beam shine on a photoelectric cell (selenium), and then put the resulting current through a telephone receiver. The result was a tone. A disk with an aperiodic wave-form cut on the edge gave a noise. A disk similar to the sine-curve but with varying wave-length gave, so he maintained, a vowel; at any rate it gave a noisy tone with a pitch as indeterminate as the wave-length. The problem of vowels we consider later, but it is important here to see that Jaensch's experiment led him into what he called a *Duplicitätstheorie* of hearing: periodic vibration excites the cochlea and gives tone, whereas aperiodic vibration excites some other organ, perhaps in the vestibule of the ear, and gives rise to noise. The intermediate sounds excite both organs in varying degrees. The analogy is to the duplicity theory of von Kries for the functioning of the rods and cones in vision. Von Kries thought of the rods as forming a more primitive sense than the cones. Jaensch thought of noise as more primitive than tone.

In recent years the duplicity theory of tone and noise has disappeared because there was no physiological evidence for it, because psychologists have come to rely less upon expert introspection, and because the tonal stimuli are now known to be so complex that there seems to be no need of a separate class for noises. For one thing it became evident that masses of tones, though noisy, can nevertheless be treated as tones. Baley, for instance, in 1913 reported that a group of eight to ten tones, lying close together in frequency and occurring simultaneously, have the pitch of the mean frequency; but Ekdahl and the present author showed in 1934 that these complexes are noisy tonal masses of an approximate pitch which an observer may localize perforce at the midpoint of the mass when he is required to equate the mass to the pitch of a single

frequency. In 1929 Békésy showed that the differential limen for pitch is greatly increased when the duration of a tone (800 c.p.s) is decreased from 0.2 to 0.01 sec., thus furnishing another illustration as to how tones can vary in definiteness of pitch without ceasing to be tones.

Perhaps the most striking evidence of the relation of noise to tone lies in the perception of the thermal noise. A thermal noise is a noise produced by the random movements of the molecules of the air as the result of thermal agitation, and it is duplicated when the electrical potential, due to the random agitation of the electrons in a conductor, is amplified and impressed upon a telephone receiver. This noise is not due to aperiodic motion but has in it all frequencies in equal degree. Its sound is a *shshshsh* of fairly definite pitch. The *shshshsh* is localized, it is true, in the middle range of hearing with the extremes of pitch lacking because, since all frequencies are equally intense, the pitches for the most sensitive part of the range of frequencies predominate; but within the sound no discrimination is possible. It is certainly a noise, and its properties strengthen the conclusion that a noise is a complex fusion of tones, often brief or rapidly varying, with the indefiniteness of its pitch depending upon the range, equivocality and brevity of the determinant frequencies. Fletcher in 1938 made use of the principle that a thermal noise is all frequencies in equal intensities to study the thresholds of particular frequencies. He determined the masking effect of a thermal noise upon each frequency.

Masking

Another exception to Ohm's law is found in the phenomenon of masking. Even though it was well known that loud sounds 'drown out' faint sounds and that only the more intense harmonics in a clang are audible, the matter was slow in coming to quantitative investigation.

The first experiment was published in 1876 by A. M. Mayer, who used organ pipes for low tones and tuning forks for high tones. He sounded a pipe and a fork together in such a way that both tones were audible, making the sound of the fork especially noticeable by moving his hand back and forth over the mouth of its resonance box, so that the fork-tone fluctuated. As the vibration of the fork died down, its tone presently ceased to be audible, masked by the

tone from the organ pipe. When Mayer thereupon stopped the air to the pipe so as to remove the masking sound, immediately the fork-tone again became audible and surprisingly loud. We have seen (p. 330) that there was in the last century no good way of controlling the intensity of tones. Thus Mayer's experiment was not quantitative, nor did it lead to an exact determination of these relations until nearly fifty years later. Mayer concluded merely that low tones may obliterate high, but that this effect is not reversed. In the latter conclusion he was in error; yet he could not, with his technique, have been expected to discover the less striking masking effect of high tones upon low.

The classical paper on masking was published by Wegel and Lane in 1924. It was one of the pioneer papers in the new thermionic technique for the control of tones, a paper which we have already considered in connection with the discovery of aural harmonics. They found (a) that a given frequency tends to mask other frequencies in the same region, that the masking effect tends to be greater the closer the masked frequency is to the masking frequency, but that this general rule requires two modifications, as follows. (b) When the masked frequency is so close to the masking frequency that beats are heard, there is a slight reversal of the relationship, for audible beats will occur for a tone which, if moved out of the frequency region for beats, would have been just inaudible. (c) The inevitable presence of aural harmonics in the intense masking tone extends the masking effect far up into the higher frequencies, giving the total masking effect for a set of harmonics. This is the reason why Mayer observed masking from below up, while missing the effect from above down. Fig. 56, a modification of Wegel and Lane's schematic diagram, shows what is heard for all frequencies and intensities of the secondary (masked) tone when the primary (masking) tone is 1200 c.p.s. and 80 db. The nearer the secondary to the primary, the louder it must be to be heard. In the region of beats, however, the masking effect drops off a little. This general functional relation repeats itself at the region of each harmonic—at 1200, 2400, 3600 c.p.s. The perceptual picture is, moreover, complicated further by the entry of difference tones into the mixture.

Because these phenomena of masking imply an overlap in the physiological effects of adjacent frequencies, they have been employed, in connection with a place theory of pitch, for the relative

localization of different frequencies in the organ of Corti in the inner ear. Fletcher (1938), using thermal noise as a masking agent,

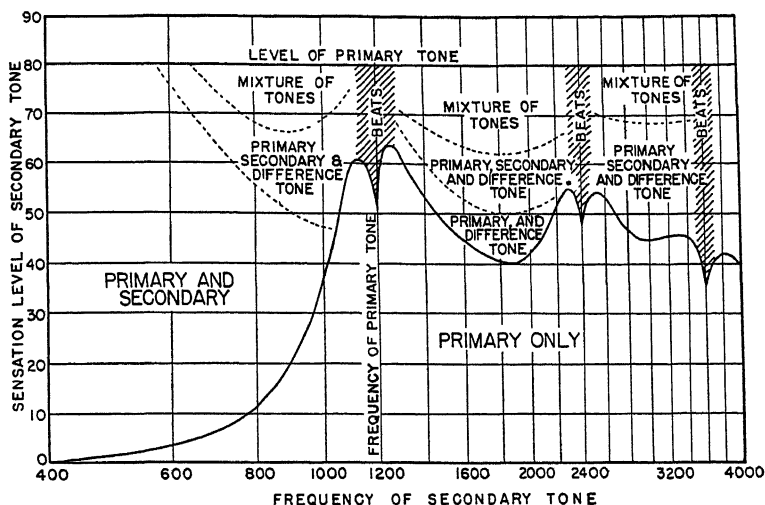


FIG. 56. TONAL MASKING: WEGEL AND LANE (1924)

The masking effect of a primary tone (1200 c.p.s. and 80 db above threshold) upon secondary tones whose intensity is given by the ordinate scale and frequency by the abscissa scale. The solid line is the locus of thresholds for the secondary tones in the presence of the primary. The threshold is lowered in the region of beats for all the harmonics of the primary. After Wegel and Lane, as modified by Fletcher.

was able in this way to assign loci in the organ of hearing to all the frequencies from 100 to 20,000 c.p.s.

Vowels

The nature of the sounds employed in speech forms in itself an interesting scientific problem, and there have been various attempts to build talking machines or automata, at least one of which included in its mechanism a dwarf concealed in a small compartment that looked like a bellows. After Helmholtz had made clear the nature of timbre the vowel sounds provided an especial puzzle, for the vowels, being sustained sounds, are thus presumably analyzable under Ohm's law, and they can in addition be sung at various pitches. Are the different vowels then simply different timbres?

Seemingly not, if voices of different timbres can sing the same vowel at different fundamental pitches. Before Helmholtz's time it was recognized that there is a pitch quality to vowels, such that the series U (as in trUth), O (sO), A (fAther), E (prEy), I (sEE) ascends steadily in pitch, even on a falling cadence of the fundamental. Hence after Helmholtz it became an accepted fact that vowel character is given in a sustained tonal complex by the intensification of certain components. This problem, which excited a great deal of interest and investigation during the latter half of the nineteenth century, recurs today with the new methods of analyzing sound. Of the several hundred papers on the subject, however, we can consider a few that are crucial.

There have been three theories of the pitch of vowels. (a) The theory of *relative pitch* makes the vowel equivalent to timbre, a pattern of the partials present. Although this theory was not held seriously by the important investigators, it was generally cited in contrast with the other two theories. (b) The theory of *fixed pitch*, as it was called, was attributed to Helmholtz; its assumption was that the mouth cavity is shaped for the different vowels so as to reinforce, by resonance, different partials of the total harmonic complex. The particular partials reinforced would depend, of course, on the pitch of the fundamental: for a bass voice the fifth partial might lie well within the region of resonance for a given vowel, whereas for a soprano voice, an octave and a half higher, the second harmonic would fall within the same region, though not at exactly the same pitch, and would be reinforced to give the same vowel. Thus, in this theory, the pitch is fixed only approximately: it is only the region of resonance that is fixed, for the exact pitch reinforced depends upon what partials happen to fall within the region. (c) According to the *formant* theory of Hermann, the region of resonance is so fixed that it responds with its own natural frequency independently of the place of the harmonics of the fundamental tone. The resultant pitch formant, which determines the vowel, is thus not necessarily a harmonic of the fundamental. In theory, then, the formant is more definitely fixed than the reinforced harmonic of the 'fixed pitch' theory, but actually those who followed Helmholtz generally managed to find some actual frequency which they stated as defining the position of the region of resonance, whereas Hermann's method was such as to lead him to specify general regions rather than exact optimal pitches,

There may be said to have been four principal methods of determining the pitches of the vowels (cf. Table IV). (a) The early method consisted in determining with tuning forks the resonance value of the mouth cavity when it was set to form the vowel. (b) Later the method of analyzing phonograms by Fourier's analysis came in. The early phonograms of spoken vowels were traced on smoked paper on a kymograph, but Edison's invention of the phonograph in 1877 provided so accurate a procedure that the results became highly complicated. (c) Along with these analytical methods there was the synthetic method of producing vowels. Helmholtz did it with tuning forks. Whereas the first two methods depend on the mouth for the specification of the vowels, the synthetic method depends on the ear. (d) Finally, there came in another auditory method in which pure tones were compared with vowels, or two tones were compared with each other with respect to their relative resemblance to some vowel. It is a reasonable question, for instance, to ask whether 950 c.p.s. is more or less like A than 900 c.p.s.

With this brief résumé of the research in mind, we are prepared to understand the history. The reader should keep his eye on Table IV as the discussion progresses.

Of the very early contributions one of the most important is that of Kempelen who, writing in 1791 on the mechanism of speech, described the construction of an elaborate talking machine. He seems to have proceeded by trial and error in the construction of his machine, arriving at no generalizations about the vowels or the other speech sounds. He gave, however, definite specifications for getting the different vowels by means of reeds and boxes of various shapes, and it is quite clear that he produced the series of vowels UOAEI by continuously reducing the concavity of the reenforcing box. In a sense, then, he may be said to have established this order of pitches for the vowels, although he did not make the generalization.

Ordinarily the history of experimentation in this field is regarded as starting with the Englishman Willis, who in 1829 published a study of the vowels. Vowel-like sounds, he found, are given off by tubes in which the column of air is activated by a vibrating reed; the particular vowel depends upon the length of the tube. The series UOAEI he could get from a tube with a piston in it, pushing in the piston to change the vowel. He was able to establish only

TABLE IV

PITCH CHARACTERISTICS OF THE PRINCIPAL VOWELS

Frequencies (c.p.s.) which characterize the principal vowels. The figures are rounded off, because the data for the 19th-century experiments were given in musical notation and are not exact, and because in no case is the specification of the vowel sound exact; but intended musical ratios have been kept in all cases. *Reson.* = analysis of resonance of mouth cavity when set to speak the vowel. *Phonogr.* = analysis of phonographic curve recorded for spoken vowel. *Synth.* = synthetic creation of vowel sound. *Comp.* = identification of tone with vowel character. Ten investigations and their dates.

	WILLIS	DONDEERS	HELMHOLTZ	KOENIG	HERMANN	MILLER	JAENSCH	KÖHLER				MODELL	
								THEOR.	Av.	RICH	RICH	RICH	RICH
	1829	1857	1863	1870	1895	1916	1913	1910	1910	1910	1915	1915	1919
	<i>Synth.</i>	<i>Reson.</i>	<i>Reson.</i>	<i>Reson.</i>	<i>Phonogr.</i>	<i>Phonogr.</i>	<i>Synth.</i>	<i>Comp.</i>	<i>Comp.</i>	<i>Comp.</i>	<i>Comp.</i>	<i>Comp.</i>	<i>Comp.</i>
U	trUth	Low	350	175	235	525-590	325	250	262	225	280	280	262
O	sO	525	295	470	470	590-660	470	500	525	470	600	600	525
A	fAther	590-1400	470	940	940	660-820	920-1240	1000	1050	1140	1190	1190	1065
E	prEy	2360-4200	1095	1880 + 175	1880	1970-2100	1950 + 690	2000	2100	2265	1900	1900	-
I	sEE	6300?	1400	2350 + 175	3760	2350-3525	3100 + 300	-	4200	3480	3750	3750	-

ranges for the frequencies for the different vowels; the range for U, moreover, was so long that he could not specify it, and the tube for I was so short that he was uncertain of it. Nevertheless, Table IV shows that his regions for A and E include most of the later specifications, and that his value for O is in the region fixed later. (The frequency for O has always been more reliably establishable than the frequencies for the other vowels.) Willis also produced the series UOAEI by holding a watch spring against the teeth of a wheel and speeding up the wheel (cf. p. 335). The uncertainty of his results he explained by noting that sustained vowels are never readily recognized. In speech, he said, the vowels are easily identified because they come rapidly and contrast with one another. He denied that the vowel sounds depend on a pattern of harmonics present, and, in thus speaking, he originated the theory of fixed pitch as against the theory of relative pitch.

In 1837 Wheatstone, who reviewed the work of Kempelen and Willis, went on to develop the physical theory of Willis' findings, thus establishing the fixed-pitch theory on firmer ground. After that nothing happened to this problem until Donders, Helmholtz and Koenig undertook the investigation of the resonance values of the mouth cavity in 1857-1870.

Donders, the Dutch physiologist, was the first to publish. He determined the resonance values of the mouth cavity for the various vowels (Table IV). Helmholtz, later in 1857, showed what happens when vowels are sung at the same fundamental pitch into a clavier with the damper removed from the strings. For U the fundamental alone responded; for O, the third harmonic; for A, the third and fifth. Two years later he published a paper which laid down the fixed-pitch theory, that vowel qualities are due to a reinforcement of partials in a fixed region of pitch. Against the view that relative pitch (timbre) determines the vowel he argued definitely. Then, in the *Tonempfindungen* of 1863, he went into the matter thoroughly, thus getting his name attached to a theory initiated by Willis. Next he determined the optimal resonance value of the mouth cavity for the different vowels, noting that resonance for the higher vowels may be double. In the region of E and I, for instance, the mouth is shaped like a bottle with a large neck; accordingly there is one period of resonance for the total cavity and another for a part. Hence both a high and a low frequency belong

to these two vowels. (Table IV.) The same result was found by Miller in 1916.

To validate his theory Helmholtz undertook a synthetic experiment. For the purpose he used the clang synthesizer, which we have already had occasion to mention (p. 331). (It consisted of a series of ten tuning-forks, the first ten harmonics of 128 c.p.s., arranged so that the relative intensities of the different harmonics could be controlled. Fig. 51.) With this apparatus he could set up any timbre for the fundamental 128 c.p.s., and he could also, by intensifying partials in the right regions, imitate the vowels. These synthetic vowels convinced Helmholtz, but, having in them too few components to match actual speech, they would probably not convince a modern psychologist. The clang synthesizer, elaborately standardized by Koenig, stands in many of the older psychological laboratories, a monument to the classical days of psychological brass instruments. It is usually known as the "Helmholtz vowel apparatus."

Rudolph Koenig himself, having determined the resonances of the mouth cavity, published values for the vowel pitches in 1870. The interesting thing about his data is that, placing U at $b = 235$ c.p.s., he placed the other vowels in the UOAEI-series exactly an octave apart—as did Köhler in 1910.

The study of the vowels by means of their phonograms was, for the most part, the achievement of Hermann. It is true that Donders had used the records of spoken vowels recorded on a kymograph (phonautograph) in 1864, but Hermann, making use of the Edison phonograph, was the principal contributor by this method. In 1889–1894 he published a series of six "phonautographische Untersuchungen." In the first of these he contented himself with a Fourier analysis of the extremely complex phonograms of the vowels. In the last he came to the view that the vowel-character, being determined by the natural frequency of the mouth cavity, is wholly independent of the fundamental pitch of the voice. Thus the determining frequencies for the vowels would ordinarily be inharmonic with the fundamental for the voice. Hermann named these pitches *formants*. It is a paradox that, coming to this conclusion by this method, he was forced by the analysis to find so many component elements, that—as in Table IV, based on his data of 1895—he could give only ranges for the formants. In theory the formants should be absolutely fixed, whereas it is the harmonics of the

'fixed-pitch' theory that would shift with the fundamental. In general, however, except for U, Hermann's results accorded tolerably well with Helmholtz's and Koenig's.

Hermann's method was employed by D. C. Miller in 1916 with greatly improved apparatus—he called the phonogram recorder a phonodeik—and the addition of a mechanical analyzer for the curves. He was able to fix the pitches much more exactly than Hermann, and in general his results were consistent with the others. It is interesting to note that, like Helmholtz, he found double regions of resonance for vowels in the EI half of the series.

We have already considered Jaensch's production of noises by aperiodic activation of his photoelectric siren (p. 364). Actually Jaensch was more concerned with speech sounds than with noise proper. The vowels he synthesized—so he claimed—by modulating the frequency irregularly. For instance, instead of cutting a series of sine waves on the edge of the siren disk—each wave subtending 24° with fifteen waves to the complete circumference—he made the lengths of the waves 22° , 24° , 23° , 26° , 25° , etc., letting each of these five wave-lengths occur three times in a fortuitous order in the circumference of the disk. The result was a frequency which fluctuated around the average frequency, corresponding to an average wave-length of 24° . Jaensch found that such fluctuation makes the tone noisy; nowadays we know that irregular modulation of the frequency of a tone in this manner would tend to scatter frequencies into regions adjacent to the average frequency. There would be a mass of tones with an approximate but not an exact pitch—a tonal noise, in fact. Jaensch regarded these noisy tones as good vowels, a conclusion which led him to suggest that vowel-character is a qualitative dimension of noise. His specifications were most general. He placed U at a frequency of 250 of his varying waves per second, and the other vowels at successive octaves above—O at 500, A at 1000, and E at 2000. I, which should have come at 4000, was too high for this method of synthesis.

It was Köhler, investigating the attributes of tones in 1910, who thought to compare simple tones with vowels. He used tuning-forks, and, in the upper regions of frequency, the Galton whistle. Actually he was reversing the conventional argument. Instead of finding the vowels tone-like, he used his results to indicate that the tones are vowel-like, that vowel-character or vocality is an attribute of tone. His results seemed to him to mean that the vowels UOAEI

lie separated by octaves, as Koenig had said. Fixing on *c*, instead of *b*, as the crucial point, he placed the vowels UOAEI at 262, 525, 1050, 2100 and 4200 c.p.s. respectively. This was his theory, for the actual results fell somewhat off from the exact octaves. (See the column for "Av." under Köhler in Table IV, the averages that Modell and Rich computed from Köhler's data for Köhler as observer.)

Most of Köhler's predecessors had investigated vowels intermediate between UOAEI, such series as *poo*h (U), *poe* (O), *pau*, *pa* (A), *pot*, *pat*, *pet* (E), *pate*, *peat* (I). The present exposition has omitted mention of these extra vowels. Köhler, having decided that UOAEI are principal vowels, like *RYGB* in the series of hues, proceeded to argue that intermediate vowels would thus be like the intermediate hues—*uo* between U and O like orange between R and Y. Such a system, accounting for octave similarity in phenomenological terms, also suggests that there should be still other principal vowel-qualities at octave intervals beyond the extremes of the UOAEI-series. The Galton whistle gives a hiss, a *sss* vowel, at a high frequency, perhaps at 8400 c.p.s. Still higher up it goes off into a *fff* and finally *ch* (like the soft *ch* German sound in *Chemie*). Köhler, writing in the period when the error in the calibration of the Galton whistle led to the belief that the upper limit of hearing may be as high as 50,000 instead of 20,000 c.p.s. (pp. 335 f.), tentatively placed *fff* at 16,800 and *ch* at 33,600 c.p.s. At the lower end of the scale he suggested that an octave below U there is a low *mmm* vowel near 125 c.p.s., and Titchener entered into the speculation by hearing in the rumble of the Appunn reed for 64 c.p.s. a continuative *vvv*. That made a series of ten principal vowels separated by nine octaves from 65 to 33,600 c.p.s.: *vvv-mmm-U-O-A-E-I-sss-fff-ch*. Phenomenology was not, however, destined to stretch quite so far. The system broke down when the exact octave relations could not be confirmed.

On the other hand, not only did Köhler's determination of the actual frequencies of UOAEI reenforce the earlier findings, but he himself was also confirmed, within the broad limits of tolerance which this type of investigation required, by Modell and Rich in 1915 and by Rich without Modell in 1919.

Recent investigations of speech sounds have been phonographic, making use of the oscillograph to obtain a record of the wave form. Analysis of such vowels shows a wide scattering in the auditory

'spectrum' of frequencies. Fletcher's account of them in 1929 is representative. Although the results do not contradict the generality that appears in Table IV, they show rather that total speech sounds are ever so much more complicated. If this seems an unsatisfactory result of so much experimentation, it must be remembered that a vowel is in itself no very specific object for investigation. The same person, trying to duplicate a vowel, may not succeed at all in reproducing the same sound spectrum.

Attributes of Tone

The phenomenological description of sensory experience, as we have seen in an earlier chapter (pp. 19-27), tended to become formalized by an enumeration of attributes and the determination of their laws of dependence upon the stimulus. All sensations, of course, 'had' quality, that is to say, they varied in respect of a qualitative dimension. Quality was thus the important attribute which usually gave the name to the sensation. Yet sensations also had intensity, for they could vary in degree. Since quality was regarded as the primary attribute, intensity was supposedly secondary, an intensity of a quality. That is approximately the relation found in Wundt's system. In 1893 Külpe formalized the doctrine, (a) arguing that the attribute is an inseparable part of the sensation (the sensation disappears if any attribute is abolished) and (b) implying that the attributes of sensation are distinguished from one another by being independently variable. Although he added duration to quality and intensity as attributes of tone, he did not admit extensity as a tonal attribute.

The phenomenological description of a tone is, however, not exhausted when its quality, intensity and duration have been stated. Tones have brightness, volume, octave similarity. Are these additional descriptive dimensions also attributes? On the ground that these aspects have to be included in a complete description, that they are, indeed, inseparable, the phenomenologists tended to say Yes. The psychophysicists tended to go farther and inquire whether, by quantitative experiment, all the alleged attributes can be shown to be independently variable. It was, in fact, the psychophysical experiments upon tonal volume that criticized and changed Külpe's too simple doctrine.

In the decade 1910-1920, when phenomenology was becoming

important in experimental psychology and Gestalt psychology was being born of it, there was quite a flurry in tonal attributes. At least nine important psychologists offered considered judgments about the number of tonal attributes and their names, to the end that a great deal of terminological and factual confusion resulted. If we ignore intensity (loudness), which everybody admitted, the following sentences give some idea of the situation in respect of the 'qualitative' attributes. Révész (1913) wanted three tonal attributes: *pitch*, *vocality* (vowel-quality) and *quality* (octave similarity). Jaensch (1913) said that tones have *pitch* and noises *vocality*. Max Meyer (1914), however, held that the dimensions of high-low, large-small, and vowel-quality are all the same attribute, which he called *vocality*, getting rid of *pitch* as a term but adding to vocality a second attribute, *tonality* (octave similarity). Watt in 1914 used *volume* to describe both the large-small and high-low dimensions of tone, and *pitch* for vowel-quality and octave similarity, achieving this extraordinary change in the meaning of *pitch* because the vowels, seeming to show octave similarity, appeared to him to be the basic quality of tone. Stumpf (1914) identified pitch with *brightness*, and allowed two other attributes, *volume* and *quality* (octave similarity). Köhler (1915) had four attributes: *brightness*, *volume*, *vocality* and *pitch*, the last of which he, like Watt, used for octave similarity. Titchener (1915) lent his authority to three: *pitch*, *volume* and *tonality* (octave similarity). Rich (1919) investigated the independent variability of all these claimants to attributehood by seeing whether they had, in terms of stimulus-frequency, different differential thresholds, so that they could vary independently when a single dimension of the stimulus is altered. He ended by agreeing with Stumpf that *pitch* is not to be distinguished from brightness and that *volume* and *tonality* (octave similarity) are independently variable with respect to each other and to pitch. It was much later (1934) that Stevens, on the basis of experimental work, added *density* to the list.

In the following paragraphs we shall consider successively the seven more important proposals for attributes of tone. For the sake of clarity, we shall ignore the authors' predilections for names, and employ the commonly accepted words. Thus the possible attributes are: (1) *Pitch*, which varies predominantly with stimulus-frequency and applies to the high-low dimension. (2) *Loudness*, the

new word for what psychologists called *intensity* before 1920. (3) *Brightness* for the bright-dull dimension. (4) *Volume* for change of extensity or size of a tone, since low tones tend to be judged larger than high, and loud tones larger than faint. (5) *Vocality* for vowel-similarity. (6) *Tonality* for octave similarity

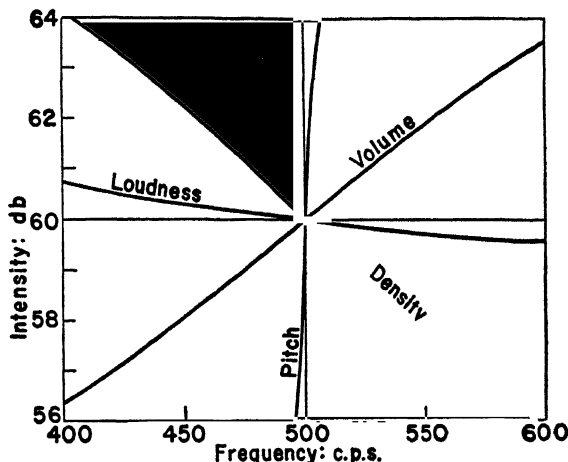


FIG. 57. ISOPHONIC CONTOURS FOR FOUR TONAL ATTRIBUTES: STEVENS (1934)

The standard tone (corresponding to the center of the diagram) has a frequency of 500 c.p.s. and an intensity of 60 db. The 'equal-loudness' contour shows the locus of those combinations of frequency and intensity which will give tones equal in loudness to the standard tone. The contours for pitch, volume and density are similar. The graphic relationships are considerably altered for standard tones of different frequency and intensity.

and thus presumptively for the varying disparity of the members of intervals within the octave. (7) *Density* for the dimension of compactness (hardness) vs. rareness (thinness).

So well was isomorphism worked into the thought of psychologists before 1920 that it was more or less taken for granted that every attribute of sensation would correspond to some dimension of variation of the stimulus—as pitch to frequency, loudness to energy. From that simple view phenomenology broke away; psychophysics after 1920 showed that the modes of correspondence, being complex, are to be established only by the experimental determination of the form of a functional relation. Thus it became apparent that the total number of attributes is limited only by the

number of relationships that can be established. This matter has been discussed in Chapter 2 (pp. 26 f.).

1. *Pitch*. All tones have pitch; on this point there never was dissent. Tones vary in quality from what is called *high* to what is called *low*. Galileo discovered that pitch varies with frequency, a simple isomorphic relation which held until Zurmühl in 1930 and Stevens in 1934 showed that the intensity of the stimulus also affects the pitch. (Fig. 57; also pp. 323 f.) It is true that others before 1930 thought that pitch changes or ought to change with intensity, but they lacked experimental evidence; neither did their guesses agree as to the nature of the change.

2. *Loudness*. Neither was there ever any question about tones' varying in loudness, but the correspondence with the stimulus was not so definite as it was for pitch. Sometimes amplitude of the wave and sometimes its energy—the energy varies with the square of the product of amplitude and frequency—was considered the 'true' stimulus to loudness, until, following the renaissance of auditory psychophysics after 1920, it was realized that there is no simple isomorphic relation and that the functional dependencies have to be worked out by experiment. Loudness was found to be a determinate function of energy and frequency. (Fig. 57; also pp. 336–338 and Fig. 54.)

3. *Brightness*. Low tones are dull; high tones are bright. Is brightness, then, just another term for pitch, or is it a new attribute? Mach had argued (1886) that *bright* and *dull*, not *high* and *low*, are the truly descriptive terms for the qualitative continuum of tones, suggesting that the pitch-series results from a combination in changing degree of *bright* with *dull*, just as the oranges result from a combination of red with yellow. After Stumpf (1890, 1914), Brentano (1907) and Köhler (1915) had all identified pitch with brightness, Rich (1919), failing to determine a significant difference between the differential limens for the two attributes, took this finding to mean that they do not vary independently. Much later Stevens and the present author (1936), noting that higher transients make a tonal complex bright, were inclined to equate brightness to density.

4. *Volume*. The first relationship, noticed by Stumpf (1890), was that low tones tend to be large and massive, whereas high tones tend to be small. Stumpf was not concerned at that time with the listing of a formal set of attributes. Titchener, however, pro-

nounced volume an attribute in 1910, followed by Rich (1916), who proved its independent variability by showing that the differential limens are larger than the limens for pitch and vary as a function of frequency in accordance with a different law. The volumic limens, in fact, were for him approximately proportional to the logarithm of the frequency, after the manner of musical intervals. Thus Ogden (1924) suggested that volume, not pitch, may be the basis for the musical scale.

Meanwhile the acousticians and musicians were using the word *volume* as practically synonymous with loudness. When Halverson (1924), employing observers accustomed to make judgments of volume as frequency is varied, found that they could also discriminate volumic differences with the variation of the intensity of the stimulus, he determined quantitatively the functional relation. This finding made tonal volume into a joint function of frequency and intensity, such that it ought to have been possible to equate in volume a loud high tone to a faint low tone. A heterophonic equation of this sort is, however, not easy to make and the first experiments failed. Stevens (1934), who finally hit upon the right method, worked out the functional relation that is shown in Fig. 57. This was the first study to make it plain that the criterion of the differentiation of attributes is not so much independent variability, as Külpe had said, but rather independent constancy. Though it is not possible to keep both pitch and loudness constant while volume varies, nevertheless one can keep volume constant while both pitch and loudness vary. This situation is realized when the frequency and intensity of the stimulus are varied concurrently along the contour of Fig. 57, which is the locus for constant volume.

The question has been raised as to whether tonal volume, as thus established by Stevens, is a 'true' attribute of tone or merely an association, as of low loud tones with big instruments and high soft tones with little instruments. Presumably the question is unanswerable; a learned attribute which shows constant functional relations to the stimulus, which is judged immediately, and which exhibits no large individual differences, is as 'true' as any other. Most observers make use of associated visual imagery in judging tonal volume as they do also in judging tactual localization, but Stevens found that a congenitally blind observer who used kinesthetic imagery, made the same kind of judgments of tonal volume as seeing persons.

5. *Vocality*. The work on the vowels left it quite indeterminate as to whether vocality is an attribute of tone or simply the fact that certain pitches characterize the vowels. Révész (1913) was the first person to label vocality an attribute. Köhler (1910), stressing the approximate octave relation of the principal vowels, regarded intermediate frequencies as giving mixed vowels; thus Watt (1914) identified vocality with tonality because of this apparent octave relation. Rich (1919), however, failed to get reliable limens for the discrimination of vocality, and the view today is that vocality is not something new in attributes, but merely the way the other attributes sound.

6. *Tonality*. It was perplexing for the psychologists to find that their measures of pitch did not correspond to the musical scale. Some octaves had in them many more difference limens than others. Pitch seems, moreover, to change continuously from low to high through the gamut, whereas octave similarity implies a circular variation of some other attribute—a something which goes around and around while pitch ascends, so that *c*-ness recurs once per octave and so that there is a *g* somewhere intermediate between one occurrence of *c* and the next. Thus, under different names, McDougall (1905), Brentano (1907), Révész (1913), Köhler (1915) and Titchener (1915) have subscribed to a separate attribute of tonality. It was Meyer (1914) who gave it that name. Even Rich (1919), finding a set of limens that were not for pitch or for volume, hoped that they might indicate the existence of tonality. Figures for tones, analogous to the color solid, have been constructed to include tonality. One such figure is a conical spiral line, in which pitch is thought of as ascending vertically, volume as measured radially from a central axis, and tonality as being indicated circumferentially, *c* being on one side, *g* on the other, and the other tonalities in between. Nowadays, with diminution of the phenomenological interest in tone, with the failure to get differential limens of tonality, and with the knowledge that the aural harmonics insistently apply the musical relations to tones, the need for this attribute has largely evaporated. The octave is, so it is thought, the tonal distance for which the harmonics—and there always are aural harmonics—interpenetrate maximally.

7. *Density*. Because both were similarly related to the frequency of the stimulus, density was originally identified with volume. Titchener (1910), in defining the volumic attribute, said that “deep

tones appear very large and diffuse, and high tones very small and concentrated." It was Stevens (1934) who noted that large-small and diffuse-concentrated are different dimensions, and, by plotting the isophonic contours of each against frequency and intensity, demonstrated their difference. (See Fig. 57.) If stimulus-frequency is increased, volume decreases and density increases, for the high tones tend to be small and concentrated. On the other hand, if stimulus-intensity is increased, both volume and density increase, for the loud tones are large and dense. Thus it comes about that a loud high tone may match a soft low tone in volume, as we have already noted, but that it takes a loud low tone to match a soft high tone in density. So far this discovery has helped more to clarify the definition and establish the nature of an attribute than to throw light upon other phenomena of hearing.

Auditory Localization

Sights and touches have extension, size and shape. The conventional view of psychology has been that tones and noises do not, although recently tonal volume has done much to establish its claim as a sensory attribute. Because they denied extension to tones, the nineteenth-century psychologists for the most part doubted the existence of any 'truly' auditory space perception, and sought for ways in which touch and kinesthesia could provide spatiality for tones. Here we have an excellent example of the way in which systematic belief influences experiment, for the experimentalists at the end of the century were mainly concerned with the genetic problem of how non-spatial tones get themselves placed in space. The problem was, they thought, to describe and understand a subjective space of which the conscious individual is immediately aware. Some geneticists, thinking in the tradition of Lotze, Helmholtz and Wundt, tried to decide how auditory space can be built up by the mediation of other sensations. Others, nativists like Stumpf and Münsterberg, argued that auditory space is itself immediate. Along in the present century, when the problem of the immediacy of perception had lost much of its scientific meaning, this difficulty disappeared; psychologists then asked merely, as Lord Rayleigh, the physicist, had always done: Can the organism discriminate the relative positions of tonal stimuli, and, if so, how?

Spatial discrimination in sight and touch is possible because the

visual and tactual receptors are spread out spatially in such wise that a pattern of stimulation can be impressed upon them. In the case of hearing, although, according to some theories, there is a pattern of pitch impressed spatially on the organ of Corti in the inner ear, there seemed—to introspection—to be no spatial pattern. Nevertheless, inasmuch as the organism possesses two ears in different places, physiologists recognized quite early that this displacement of one ear with respect to the other provides the basis for a crude and simple localization. The trouble was that psychologists were later distracted from this basic fact by their desire to solve the empiricistic-nativistic problem of auditory space.

Another way in which presuppositions hindered progress appeared in connection with the phase relations of tones. Helmholtz, who had studied phase, came to the conclusion that it makes no difference in the operations of Ohm's law or in the perception of timbre, vowels or any other qualitative pattern; and from this decision there arose the commonplace that the organism "cannot perceive phase." Thus, it was said, if the organism cannot perceive phase, then phase cannot be the basis of auditory space perception. Eventually, of course, it was shown that the difference of phase between the two ears is one of the bases of auditory localization, but the conclusion was prevented for many years by a faulty isomorphism: if tones have no attribute that corresponds with phase, then one cannot perceive phase, and, out of what he cannot perceive, he certainly cannot build up a perceptual space.

With this perspective of the way in which theory interfered with discovery, we can now turn to the actual history of the experiments on auditory localization, and in so doing we shall use Stumpf's convenient terminology. Stumpf (1916) called the simultaneous stimulation of the two ears by the same stimulation *diotic*, the simultaneous stimulation of the two ears by different stimulations *dichotic*, and the stimulation of one ear only *monotic*. In this connection it is important to note that a total stimulus can at the same instant be both diotic and dichotic; for instance, the binaural stimulus may be diotic as to frequency and dichotic as to intensity, as it is in the common situation for the intensive localizations of tones.

The empiricists—Berkeley, James Mill and Bain—all attributed the localization of sound to experience. In the application of this principle they could be most explicit about heard distance, for

there they argued that a sound whose intensity is known can have its distance inferred from the loudness with which it is perceived. On the means by which auditory direction is perceived they were more vague, but in general they held that other sensations must assist. Bain asserted, for instance, that, if one man speaks from among a row of persons, we cannot identify the speaker unless we recognize the quality of his voice or see his lips move. Johannes Müller, the physiologist, writing at this time (1838), noted that the perception of direction must depend upon the different position of the two ears; it must be, he said (hitting incompletely on the truth), the difference in intensity of the sound at the two ears or the change of intensity at one ear when the head is moved which provides the cue for direction. As showing how visual perception may determine the perceived direction of a sound, he also cited ventriloquism, an ancient art.

The experimental literature seems to begin with Ernst Heinrich Weber's observation in 1846 that when two watches are placed near an observer, one on each side of his head, they can both be heard simultaneously, and each can be referred correctly to its proper side. Here, then, is the initial recognition of the fact that the primary dimension of auditory localization is left-right, the direction of dichoticity of binaural stimulation.

At once there arose the question as to how there can be an auditory space when sound has no size. Eduard Friedrich Weber, one of Ernst Heinrich's younger brothers, decided that sound cannot be localized when the head is under water and the external canals of the ears are filled with water, except that right and left can be correctly distinguished. Thus, he argued, localization, except the gross distinction between left and right, is a tactual matter and depends upon tactual intensities, presumably the sensations in the external aural canals. He tried tying the pinnae of the two ears back against the head and having the observer cup his hands in front of his ears, so as to form artificial pinnae turned toward the rear. In that situation so many localizations were reversed from front to back, that Eduard Weber concluded the front-back localization to be dependent upon intensity—the intensive difference which comes from the ears' being aimed forward.

After the Webers nothing much of importance happened for twenty-five years. In 1876 Politzer reported the case of a man with a monaural defect who always localized sounds toward the side

of the more sensitive ear, and then in the same year Lord Rayleigh took up the problem. He stood with closed eyes on a lawn with a circle of assistants around him. When one assistant spoke, he tried to localize the voice, finding that he could be correct within a few degrees. With low pitched tuning-forks, however, he was not so successful, a fact which led him to propose that dichotic intensity is the basis for localization. Having figured out how the intensities of tones would vary at the two ears when the sound comes from the side, he showed that the intensive difference would be great for high tones, less for low, and negligible for tones whose wavelengths are as much as four times the circumference of the head (ca. 150 c.p.s. and less). Actually he got no difference in accuracy of localization for 128 and 256 c.p.s. The next year, as the result of more experimentation with forks, he was able to show that localization depends on the binaural ratio of the intensities at the two ears. It was from this work of Rayleigh's that the phrase *binaural ratio*—meaning always a ratio of intensities—came into the literature. Rayleigh pointed out the implications of his result: for every position in front of the observer there is a corresponding position behind where the binaural ratio is the same, a place which is confused with the region in front in the same way that front-back localizations in the median plane are confused. Thus he demonstrated altogether (a) that noises (voices) are better localized than tones, (b) that high tones are better localized than low, (c) that the binaural intensive ratio is crucial for localization, (d) that there is confusion between front localization and any symmetrical position in back, and (e) that there is no confusion between right and left. A considerable contribution, even though it neglected phase difference.

Other support came to the 'intensity theory' at this time. Von Kries and Auerbach argued in 1877—as they well might with Helmholtz's theory of *unbewusster Schluss* still in the air—that localization is an inference as to position based on auditory intensities. The observer must, they said, (a) estimate the intensity at each ear, (b) judge the relations of the two intensities, and then (c) conclude as to the location. Inference takes time, and they found that the time of localization, longest for the median plane, diminished as the sound (the crackle of a spark) was moved toward one side, becoming minimal at the side. Silvanus P. Thompson, who inaugurated the 'phase theory' in 1878, also found inten-

sitive difference determinative of localization in some cases. In the same year Tarchanow, having set up two telephone receivers, one at each side of the observer, showed that with equal intensities the bilateral stimulation "fused" into a single sound in the median plane. His report was brief, but Urbantschitsch in 1881 reported more thoroughly on the method. Equal intensities give, as he put it, "a combination field" of sound, which, as the binaural ratio is changed, migrates toward the louder side. Then in 1879 Thompson built his "pseudophone," a frame to fit over the head and carry at each ear a small shield which could be rotated, acting as an artificial pinna. When these pinnae were reversed, back to front, the localization of sounds tended to be reversed, front to back. When the pinnae were arranged to favor the sound in one ear and shield the other ear, the localization moved toward the favored ear. This experiment, therefore, supported the intensity theory. Later, in 1882, Thompson showed, as had Rayleigh, that intensive difference is more effective at high frequencies than at low, but that low notes may nevertheless be localized by their higher harmonics.

The quantitative measures of localization as a function of dichotic intensity came later. Bloch's results in 1893 (correcting Münsterberg's; *vide infra*) are classical. He measured the angular differential threshold, finding that it was least in front at the median plane, larger in back, and largest at the sides. Although there is never actual confusion between the two sides, a small change in angle at the side is not noticeable; whereas in the median plane there is confusion between front and back, though a small deviation from the median plane is readily noticed.

Matsumoto, working with Scripture at Yale in 1897, was the first to use a sound cage (but cf. Preyer's helmet, *infra*), a spherical metal frame work centered about the observer's head, so as to mark various angular positions at a constant distance from the head (Fig. 59). With a telephone receiver at each side and the relative intensities of their buzzing noise controlled, he measured the accuracy of localization, thus duplicating Bloch's function. He also used the cage with a single telephone receiver placed at various positions. Later more thorough studies confirming the general relation of the binaural intensive ratio to localization were made by A. H. Pierce in 1901 and Ferree and Collins in 1911. Recently Steinberg and Snow (1934) have shown exactly how the sound-shadow of the head affects the binaural ratio at various frequencies of tone.

vice that fitted over the head and carried 26 wire pointers, determining 26 equidistant positions from the head. Preyer's pupil, Arnheim, in the same year put out a dissertation relying on the same method and ending in the same theory. Münsterberg was a little more explicit in 1889. He determined the accuracy of localization as greatest in front of the observer, less at the sides and least behind—a functional relation that Bloch, as we have seen, presently corrected. Since the movements of the head are less accurate the farther the head is turned from the front position, he saw in these (incorrect) data a reason to believe that head movements, and thus the semicircular canals, contribute to auditory localization.

Against this theory of the semicircular canals Breuer argued (1891) that it would be "completely impossible" for the canals to mediate direction of sound, since the stimulus, after passing through the external aural canal, would have no way in which it could excite the three internal canals differentially. And von Kries (1891), who presented an experiment to show that two sounds, a noise and a whistle, can be localized simultaneously (as E. H. Weber had noted for two watches long before), remarked that the head could hardly move in two directions at once. (One wonders too what these motor theorists would have said to the intracranial localizations that were stressed so much later in connection with the phase theory. How does the head move to get inside itself?)

Also against the canal-theory was one touch-theorist, Gellé, who in 1886 reported on a patient of Charcot's. The patient was tactually anesthetic all over his face and at both ears. Since he could not, so it was reported, localize sound at all, Gellé was inclined to revert to what had been Eduard Weber's theory, that auditory localization depends upon touch.

This account brings the research pretty well up to the end of the nineteenth century. In the twentieth century the interest in the genesis of a subjective auditory spatial system disappeared, and in its place the questions of the function of dichotic phase and time arose. For a while there was conflict between what were called the 'intensity theory,' the 'phase theory' and the 'time theory.' Only after 1920 was it realized that the investigations indicate, not three theories of localizations, but different physiological means by which the same localization is effected.

The 'phase theory,' as we have seen, began with Silvanus Thompson. In 1877 he observed binaural beats. He had two tuning-forks,

one of 128 c.p.s. and one slightly mistuned from 128, and led the sound by rubber tubes from one fork to the left ear and from the other to the right ear. He heard beats. This observation has always proved difficult, for each sound seems continuous and yet the beats are heard. Although Thompson did not then note any movement of the fused sound, the next year he reported an experiment in which the sound from two forks, 256 and 512 c.p.s., was conducted through a single tube which branched and led to both ears. When he rotated one fork about its axis so as to change its phase relation to the other, the sound of that fork migrated from the front to the rear while the sound of the other fork stayed put. He also arranged to take these two frequencies into two telephone receivers and then to lead the sound as before through tubes to the ears. When he reversed one telephone electrically so as to change its phase with respect to the other, the localization changed as before. At that time these results were not accepted because they seemed to contradict Helmholtz's dictum that phase cannot be perceived.

Though Lord Rayleigh had considered the possibility that phase may determine the localization of sound, it was not until 1907 that he proposed the 'phase theory.' Taking his cue from Silvanus Thompson, he put two forks of 128 c.p.s. in isolation, mistuned one slightly, brought each sound separately by a tube through a wall, and adjusted the terminal tubes so that the observer could just fit his head between their ends, one ear to each opening. He heard binaural beats as Thompson had; and, although the observation was difficult, both he and Lady Rayleigh concluded that the beating sound moved back and forth from right to left. Now, as two tones beat, their phase relation passes through all possible values; thus Rayleigh concluded, since 128 c.p.s. is too low for dichotic intensity to be effective, that this movement, and hence the localization of low tones in general, may be due to the phase relation.

As soon as the great Rayleigh had made this pronouncement, More and Fry produced results that they had obtained back in 1902 and had presumably suppressed because of the improbability that phase is 'perceived.' They had arranged to bring a frequency through a tube which divided and led it to the two ears. In one side of the divided tube there was a slide extension, fixed so that the path to one ear could be made longer than the path to the other, and the dichotic phase relation of the tones changed accordingly. Working with both 512 and 320 c. p.s., they were able to make the

perceived tone shift from one side to the other simply by altering the relative lengths of the two paths. This experiment was repeated much more thoroughly in 1908 by Wilson and Myers, whose apparatus is shown in Fig. 60. There could be no doubt that low tones, thus administered, are localized toward the side of the lead-

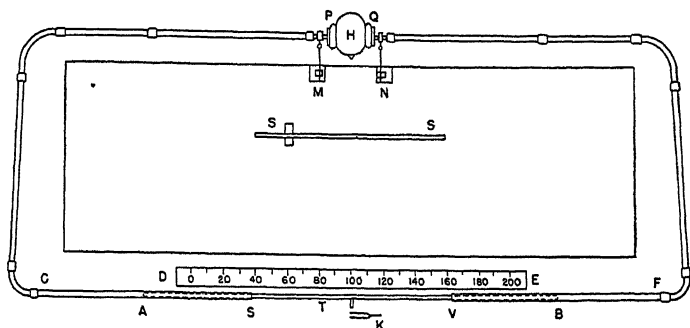


FIG. 60. APPARATUS FOR DICHOTIC PHASE: WILSON AND MYERS (1908)

The sound from the tuning-fork at *K* is divided in the tube *T*, which slides into *S* and out of *V* (or conversely), altering the relative lengths of the paths from *T* to the head *H*. *P* and *Q* are soft pads at the observer's ears; *M* and *N* are standards supporting the pads. *SS* is a screen to keep the observer from seeing how *T* is shifted, and the scale *DE* measures the shift of *T*. If the path *TSACP* exceeds in length the path *TVBFQ* by some amount less than half a wave-length of the frequency of *K*, the observer localizes the tone toward his left (*Q*).

ing phase. Within variations of half a wave-length, the localization moves toward the earlier stimulus. So obsessed, however, were Wilson and Myers by Helmholtz's rule that they were at some unnecessary pains to argue that a phase difference of this sort might be changed into an intensive difference before it became effective for localization.

In 1920 the 'time theory' was introduced, first by von Hornbostel and Wertheimer and then by Klemm, who had worked independently. Von Hornbostel and Wertheimer sought to work out with discrete sounds the time differences that would give differences of localization, and to show that these times are the same as those implied by the experiments on dichotic phase. Stimuli diotic as to time give a median localization. When the time-interval between the two stimuli is increased, the perceived sound starts to migrate from the median plane at about 30 microsec. (.03 millisec.), a

very short time indeed. As its dichoticity in time is increased, the sound migrates toward the earlier stimulus and becomes anchored at the side at about 630 microsec. Klemm, however, found great variability in his results from observer to observer and from observation to observation. The very least time difference for deviation from the median plane he determined as 2 microsec., which is an unbelievably short interval for a physiological differential. The single sound, anchored at one side, breaks up, he discovered, into two successive sounds at about 2000 microsec. (2 millisecc.). Klemm was inclined to reduce the 'time theory' to an 'intensity theory,' for he showed that dichotic intensity can be equilibrated against dichotic phase. A louder later sound in one ear may balance a fainter earlier sound in the other, so that localization becomes medial. Von Hornbostel and Wertheimer, on the other hand, sought to reduce the 'phase theory' to the 'time theory.' In 1920 they had every right to do so, for in 1912 Adrian had established the all-or-none principle of nervous transmission. A simple harmonic excitation, it was now probable, must be resolved in the nervous system into a series of discrete discharges, so that dichotic phase must become in the organism a series of pairs of dichotic times. Von Hornbostel and Wertheimer did not, however, make this explicit appeal to recent physiology.

Although there has been a great deal of research in this field since 1920, the effect has been mostly to bring the different 'theories' together. The present author in 1926 showed how phase and time are identified by the all-or-none principle and how time could be the physiological equivalent of intensity. Trimble in 1928 also brought the various researches together, making Rayleigh's original distinction explicit by attributing dichotic phase to the localization of low tones and dichotic intensity to the localization of high tones. The upper limit for the effectiveness of dichotic phase he placed in the region of 512 to 1700 c.p.s., but all the indications have been that the refractory period of the auditory nerve-fiber limits the effective frequencies to 1000 or less.

In 1934 Stevens and Newman investigated the localization of pure tones on a raised platform out in the open air where reflected sounds are minimal. Under these conditions they showed that the localization of low tones from 60 to 1000 c.p.s. is accurate, thus finally laying the ghost of Rayleigh's old suspicion that low tones are not localizable at all. Above 1000 c.p.s. localization gets poor

and is worst at some point between 2000 and 4000 c.p.s. Then it improves again, until at 10,000 c.p.s. it is quite as precise as it was at 1000. They analyzed this function into two, indicating that dichotic phase is effective for low tones and dichotic intensity for high tones, that one of these principles begins to work after the other leaves off, and that the poor localization near 3000 c.p.s. is due to the fact that there is just not enough overlap of these two

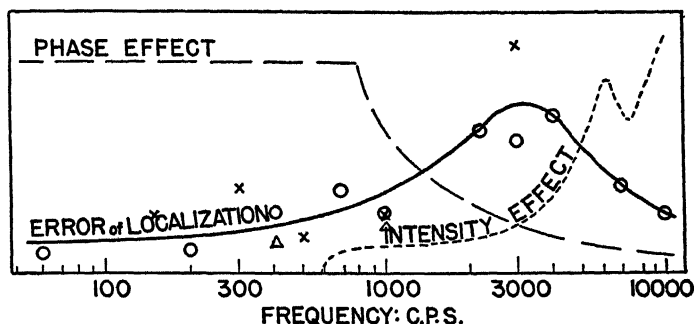


FIG. 61. ERRORS OF LOCALIZATION OF TONES AS A FUNCTION OF FREQUENCY: STEVENS AND NEWMAN (1934)

The solid curve shows how the errors increase up to about 3000 c.p.s. and then decrease. The dashed curve shows the theoretical (and very nearly the observed) effectiveness of dichotic phase for localization. The dotted curve shows the observed effectiveness of dichotic intensity for localization. Adapted from Stevens and Newman.

basic functions to make the organism as adequate in this critical region as it is at both extremes of frequency. (See Fig. 61.)

All this work shows that dichoticity is the primary factor in determining the localization of sound; yet dichoticity alone is effective only as a cue for the right-median-left dimension, whereas sounds are actually well localized in tridimensional space, in front or in back, above or below. Recently Wallach (1939) has shown that the kinesthetic system of the head, be it muscular or vestibular or both, is integrated with the auditory system when the head is allowed to move in such a way as to provide further specification of the locus of the sound. If the head is moved, the subject can 'decide' (by "unconscious inference," Helmholtz would have said!) whether the sound is in front or back. If rotation of the head about a vertical axis does not change dichoticity, then the localization is made for the position at which the dichoticity of a sound would not

be changed by such movement, i.e., above the head. Wallach has made it quite clear that the localization is not purely auditory, but the product of an integration of auditory, kinesthetic and, when the eyes are open, visual factors.

This history of auditory localization illustrates altogether the effect of systematic premises upon experimental research. In the last century the problem centered on the way in which a subjective spatial system can be built up out of auditory experience. In the present century perception has become the organism's capacity to discriminate the differentiation of the world of stimulus. We want to know the functional relations, and we want to understand them in terms of the physiology and physics of the perceptual situation. Finally we are beginning to discover that we must not be content with too simple a physiology: many different capacities of the organism may be used to determine a single localization. Not much escapes the organism that could be of use to it.

Notes

Beats and Combination Tones

For the discovery of beats and their use in the determination of relative frequencies and in tuning, see J. Sauveur [as reported], *Sur la détermination d'un son fixe*, *Hist. Acad. Sci. Paris*, 1700, 131-140, esp. 134-140.

For G. Tartini's discussion of difference tones, see his *Trattato di musica secondo la vera scienza dell'armonia*, 1754, esp. 13-19. G. A. Sorge's earlier mention of difference tones is in his *Anweisung zur Stimmung und Temperatur sowohl der Orgelwerke, als auch andere Instrumente, sonderlich aber des Claviers*, 1744, [n.v.], and *Vorgemach der musikalischen Composition*, Pt. 1, 1745, 13, [n.v.]. J. B. Romieu's independent discovery and mention of a beat-tone theory is in the rare *Assemblée publique de la Société royale des Sciences tenue dans la grande salle de l'Hôtel de Ville de Montpellier, le 16 décembre*, 1751,

77, [n.v.]. The usual references to *Mém. Soc. roy. Sci. Montpellier* are incorrect. This historical matter has been worked out by A. T. Jones, The discovery of difference tones, *Amer. Phys. Teacher*, 3, 1935, 49-51, which gives the references and quotations and shows that the usual attribution of the discovery of difference tones to Sorge is incorrect.

On the beat-tone theory of the first difference tone, see Romieu, *loc. cit.*; J. L. Lagrange, *Recherches sur la nature et la propagation du son*, *Miscellanea Taurinensia*, 1, 1759, [n.v.], reprinted in his *Oeuvres*, I, 1867, 39-148, where see esp. 142-144; T. Young, *Outlines of experiments respecting sound and light*, *Phil. Trans.*, Pt. I, 90, 1800, 106-150, esp. 130-133, or same title, *Phil. Trans.* abridged, 18, 1809, 604-626, esp. 616-618; Koenig, *loc. cit. infra*.

For G. U. A. Vieth on combination tones, see his *Ueber Combinationstöne*, *Ann. Phys. Chem. Halle*, 21, 1805, 265-314. For G. G. Hall-

ström's discovery and theory of difference tones of higher orders, see his *Von den Combinationstone*, *ibid.*, 100, 1832, 438-466.

H. Helmholtz's important paper is *Ueber Combinationstone*, *Ann. Phys. Chem.*, 175, 1856, 497-540, reprinted in his *Wissenschaftliche Abhandlungen*, I, 263-302. He also published two other minor papers on this topic in the same year, but his other principal contributions to the topic are in *Die Lehre von den Tonempfindungen*, 1863, Chap. 7 (combination tones), Chap. 8 (beats), Chap. 11 (beats of combination tones), or same chapters in later eds. or Eng. trans. It is interesting to note that Helmholtz departed so far from the beat theory of difference tones that he dealt with combination tones first and with beats second.

The view that combination tones are subjective was supported by R. Koenig, *Ueber den Zusammenklang zweier Töne*, *Ann. Phys. Chem.*, 233, 1876, 177-237; French trans. in his *Quelques expériences d'acoustique*, 1882, 87-148; W. Preyer, *Akustische Untersuchungen*, 1879, 11-43.

E. B. Newman, S. S. Stevens and H. Davis show the complexity of this problem by the modern technique of registering the electrical potential from the cat's ear, *Factors in the production of aural harmonics and combination tones*, *J. acoust. Soc. Amer.*, 9, 1937, 107-118; see also Stevens and Davis, *Hearing*, 1938, 197-201.

In general on these topics, see Helmholtz, *op. cit.*, Chaps. 7-11; Stevens and Davis, *op. cit.*, 89-94, 140 f., 184-208, 225-247. Stevens and Davis discuss fully the relation of beats and vibrato to amplitude modulation and frequency modulation, and of combination tones and aural harmonics to the distortion which is introduced by the transmission through the middle ear.

Timbre and Aural Harmonics

On timbre, see H. Helmholtz, *Ueber die Klangfarbe der Vocale*, *Ann. Phys. Chem.*, 184, 1859, 280-290, reprinted in *Wissenschaftliche Abhandlungen*, I, 397-407; *Die Lehre von den Tonempfindungen*, 1863, Chap. 5 and 6.

On aural harmonics, see Helmholtz, *Tonempfindungen* (*op. cit.*), 3 ed., 1870, 618-620, where he shows that they occur in connection with combination tones, and 2 ed., 1865, 595-598, which is before he realized this fact. For his discussion of pure tones, as if they could be heard, see any ed., Chap. 5, sect. 1. Lord Rayleigh, *Theory of Sound*, 2 ed., 1896, II, 453-469, esp. 458-466, repeated Helmholtz's argument and insisted that the ear is likely to transmit sound asymmetrically. The other citations on aural harmonics are R. L. Wegel and C. E. Lane, *The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear*, *Phys. Rev.*, 23, 1924, 266-285, esp. 272-274; H. Fletcher, *Speech and Hearing*, 1929, 175-180; A space-time pattern theory of hearing, *J. acoust. Soc. Amer.*, 1, 1930, 311-343, esp. 313-316; S. S. Stevens and E. B. Newman, *On the nature of aural harmonics*, *Proc. nat. Acad. Sci.*, 22, 1936, 668-672. In general, see S. S. Stevens and H. Davis, *Hearing*, 1938, 184-207, esp. 184-196.

Fusion and Consonance

For Helmholtz on consonance in relation to coincident harmonics, see his *Tonempfindungen*, any ed., Chap. 10 and 11. For C. Stumpf's theory of fusion, see his *Tonpsychologie*, II, 1890, *passim*, esp. 127-218. For other details and the general importance of this topic at that time, see O. Külpe, *Grundriss der*

Psychologie, 1893, or Eng. trans., 1895, sects. 43–48; E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 329–337. On the qualitative differences among the perceptions of the musical intervals, see W. Kemp, *Methodes und Experimentelles zur Lehre von der Tonverschmelzung*, *Arch. ges. Psychol.*, 29, 1913, 139–257; C. C. Pratt, Some qualitative aspects of bitonal complexes, *Amer. J. Psychol.*, 32, 1921, 490–515; E. M. Edmonds and M. E. Smith, The phenomenological description of musical intervals, *ibid.*, 34, 1923, 287–291. A. M. Brues determined degrees of fusion for the 24 intervals within the octave that are given by a quarter-tone scale: The fusion of non-musical intervals, *ibid.*, 38, 1927, 624–638. Pratt's *coup de grâce* to Stumpf's theory of fusion is his Tonal fusion, *Psychol. Rev.*, 41, 1934, 86–97.

Noise

On noise, see Helmholtz, *Tonempfindungen*, Chap. 1 (first part) and Chap. 6 (last part), in respect of which compare any of the first three eds. (1863 *et seq.*) with the 4 ed. (1877) or the Eng. trans., in order to see the change of view. The other references cited are S. Exner, *Experimentelle Untersuchungen der einfachsten psychischen Prozesse*, *Arch. ges. Physiol.*, 11, 1875, 403–432, esp. 416–418; V. Hensen, *Untersuchungen über Wahrnehmung der Geräusche*, *Arch. Ohrenheilk.*, 23, 1886, 69–90; C. Stumpf, *Tonpsychologie*, II, 1890, 497–514; O. Külpe, *Grundriss der Psychologie*, 1893, sects. 14, 15, or Eng. trans.; E. R. Jaensch, *Die Natur der menschlichen Sprachlaute*, 2, *Sinnesphysiol.*, 47, 1913, 219–290, esp. 256–269. H. J. Watt, *Psychology of Sound*, 1917, 34–41, presented a similar view and discussed the literature.

On the indefiniteness of pitch of tonal masses and their relation to

tonal hearing in general, see S. Balley, *Ueber die Zusammenklang einer grösseren Zahl wenig verschiedener Töne*, *Z. Psychol.*, 67, 1913, 261–276, A. G. Ek Dahl and E. G. Boring, *The pitch of tonal masses*, *Amer. J. Psychol.*, 46, 1934, 452–455; G. v. Békésy, *Zur Theorie des Hörens*, *Phys. Z.*, 30, 1929, 721–745, esp. 731 f.; S. S. Stevens and H. Davis, *Hearing*, 1938, 98 f. (on tonal masses), 102 f. (on Békésy).

Thermal noise was first discovered as a random variation in electrical potential in a thermionic system due to the thermal agitation of the electrons by W. Schottky, *Ueber spontane Stromschwankungen in verschiedenen Elektrizitätsleitern*, *Ann. Phys.*, 57, 1918, 541–567. He called it the “shot effect”, others later called it the “Schottky effect.” See E. L. Chaffee, *Theory of Thermionic Vacuum Tubes*, 1933, 134–136. Later when the acoustical significance of the effect was realized, it was called by Fletcher “electrical thermal noise,” and, when turned into sound by a loud speaker, “acoustical thermal noise.” See Stevens and Davis, *op. cit.*, 57 f., 98 f.; H. Fletcher, *The mechanism of hearing as revealed through experiment on the masking effect of thermal noise*, *Proc. nat. Acad. Sci.*, 24, 1938, 265–274.

Masking

The three papers cited on masking are A. M. Mayer, *Researches in acoustics*, *Phil. Mag.*, 2, 1876, 500–507, esp. 503–505; R. L. Wegel and C. E. Lane, *Auditory masking and the dynamics of the inner ear*, *Phys. Rev.*, 23, 1924, 266–285; H. Fletcher, *The mechanism of hearing as revealed through experiment on the masking effect of thermal noise*, *Proc. nat. Acad. Sci.*, 24, 1938, 265–274. See also Fletcher, *Speech and Hearing*, 1929, 167–187; Stevens and Davis, *Hearing*, 1938, 208–217.

Because Mayer could hear a faint tone of 1024 c.p.s. through a loud tone of 1280 c.p.s., he thought that the higher tone could not "obliterate" the lower, but consultation of Wegel and Lane's chart in Fig. 56 shows that the threshold of the 1024-tone must nevertheless have been raised considerably.

Vowels

The early work cited is W. de Kempelen, *Le mécanisme de la parole suivi de la description d'une machine parlante*, 1791, esp. 394-464 (De la machine parlante), where 447 f. gives the specifications for producing the vowels; R. Willis, On vowel sounds and on reed organ-pipes, *Trans. Cambridge phil. Soc.*, 3, 1829, 231-268, esp. 231-250, with the vowel frequencies on p. 243; C. Wheatstone, a review of Willis, Kempelen and Kratzenstein, [*London and*] *Westminster Review*, 28, 1837, 27-41, reprinted in *Scientific Papers of Sir Charles Wheatstone*, 1879, 348-367.

F. C. Donders' chief contributions are Ueber die Natur der Vocale, *Arch. holland. Beitr. Nat. Heilk.*, 1, 1857, 157-162; Zur Klangfarbe der Vocale, *Ann. Phys. Chem.*, 199, 1864, 527 f., on the phonographic method; *Die physiologie der spraak-lanken*, 1870, [n.v.]. Helmholtz's contemporaneous contributions are Ueber die Vocale, *Arch. holland. Beitr. Nat. Heilk.*, 1, 1857, 354 f., reprinted in his *Wissenschaftliche Abhandlungen*, I, 1882, 395 f.; Ueber die Klangfarbe der Vocale, *Ann. Phys. Chem.*, 108, 1859, 280-290, reprinted in *Wissenschaftliche Abhandlungen*, I, 397-407; *Die Lehre von den Tonempfindungen*, 1863 or later ed. or Eng. trans., Chap. 5, sect. 7 (in general and on analysis), and Chap. 6, first half (on synthesis). For R. Koenig, see his Sur les notes fixes caractéristiques des diverse voyelles, *C. R. Acad. Sci. Paris*,

70, 1870, 931-933, reprinted in his *Quelques expériences d'acoustique*, 1882, 42-46.

The first of L. Hermann's Phonographische Untersuchungen is in *Arch. ges. Physiol.*, 45, 1889, 582-620, and five more follow in the same *Archiv* up to 1894. Important are Untersuchungen mittels des neuen Edison'schen Phonographes, *ibid.*, 53, 1892, 1-51, because it takes account of Edison's discovery, and Nachtrag zur Untersuchung der Vocalcurven, *ibid.*, 58, 1894, 264-279, because it introduces the term *formant*. For actual values of the inharmonic formants, see his Weitere Untersuchungen über das Wesen der Vocale, *ibid.*, 61, 1895, 169-204. The work of D. C. Miller, the physicist, on the analysis of vowels, is described in his *Science of Musical Sounds*, 1916, 215-251.

E. R. Jaensch's synthesis of vowels with his noisy siren is in his Die Natur der menschlichen Sprachlaute, *Z. Sinnesphysiol.*, 47, 1913, 219-290. As to how frequency-modulation can result in a set of adjacent frequencies, see S. S. Stevens and H. Davis, *Hearing*, 1938, 234-241.

The important paper of W. Köhler's in this connection is his Akustische Untersuchungen II, *Z. Psychol.*, 58, 1910, 59-140, but see also his other papers in this series in *ibid.*, 54, 64, and 72, 1909-1915. The confirmation of his findings is by J. D. Modell and G. J. Rich, A preliminary study of vowel qualities, *Amer. J. Psychol.*, 26, 1915, 453-456; G. J. Rich, A study of tonal attributes, *ibid.*, 30, 1919, 121-164, esp. 131-143.

On sound spectra for vowels, besides Miller, *loc. cit.*, see H. Fletcher, *Speech and Hearing*, 1929, 28-63.

Good general discussions of this literature are to be found in Lord Rayleigh (J. W. Strutt), *Theory of Sound*, 2 ed., 1896, II, 470-478;

F. Auerbach's *Akustik*, in A. Winkelman, *Handbuch der Physik*, 2 ed., II, 1909, 681-705, esp. 691-701, which has a bibliography. Less satisfactory are Miller and Fletcher, *loc. cit.*

Attributes

For the history of research and opinion on the tonal attributes and for many more references than these notes give, see G. J. Rich, A study of tonal attributes, *Amer. J. Psychol.*, 30, 1919, 121-164, esp. 122-127.

On the theory of attributes, see, besides Chap. 2, pp. 19-27, O. Külpe, *Grundriss der Psychologie*, 1893, or Eng. trans., sect. 4; S. S. Stevens, The attributes of tones, *Proc. nat. Acad. Sci.*, 20, 1934, 457-459; E. G. Boring, The relation of the attributes of sensation to the dimensions of the stimulus, *Phil. Sci.*, 2, 1935, 236-245.

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For the English empiricists on localization, see Pierce, *op. cit.*, 7-27, where is also a discussion of nineteenth-century space theory in this connection. Pierce himself was arguing for an auditory space unsupported by other senses. Johannes Müller's discussion of these matters is in his *Handbuch der Physiologie des Menschen*, II, 1838, Bk. 4, sect.

3, Chap. 3, Pt. C (ventriloquism; imagination); Bk. 5, sect. 2, Chap. 3, Pt. 3 (intensive difference between ears and when head is moved); or later ed., or Eng. trans.

The contributions of the two Webers are E. H. Weber, Ueber die Umstände durch welche man geleitet wird manche Empfindungen auf aussere Objecte zu beziehen, *Ber. sachs. Ges. Wiss.*, 2, 1848, 226-237; E. F. Weber, Ueber den Mechanismus des Gehörsorgans, *ibid.*, 1851, 29-31. There were three brothers: E. H. Weber, 1795-1878 (Weber's law), W. E. Weber, 1804-1861 (famous physicist), and E. F. Weber, 1806-1871.

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ity, magnetism and the theory of the dynamo), are his Phenomena of binaural audition, *Phil. Mag.*, 5 ser., 4, 1877, 274-276 (binaural beats); *ibid.*, 6, 1878, 383-391 (phase theory); The pseudophone, *ibid.*, 8, 1879, 385-390; *ibid.*, 13, 1882, 406-415, esp. 415 (localization determined by higher harmonics).

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Ueber das Erkennen der Schallrichtung, *Z. Psychol.*, 1, 1890, 236-251. Against the canal theory, see J. Breuer, an expert on the canals, Ueber die Function der Otolithen-Apparate, *Arch. ges. Physiol.*, 48, 1891, 195-306, esp. 295-302.

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Chapter 11

AUDITORY THEORY

NINETEENTH-CENTURY dualism separated phenomenal fact from physiological theory. Having established the principal laws of vision or of hearing or of association, the psychologist then asked how they could be 'explained' in terms of the physiology of the retina or the ear or the brain. In a simple psychophysical parallelism conscious process had always to be correlated with physiological process. In general, the investigators paid more attention to the qualitative than to the intensive dimension of sensation, and to the sense-organ than to the brain. That was natural. Quality, being the primary attribute, should be considered first.

Johannes Müller's doctrine of specific nerve energies formed the basic theory of quality: a change in quality means a change in excited nerve fiber and thus in the place of origin of excitation in the sense-organ. Until the all-or-none law of neural transmission upset that simple isomorphism in 1912, sensory intensity seemed as if it must be simply the degree of this excitation. Thus it was Müller's doctrine which in part fixed attention so exclusively on the sense-organ, where the spatial differentiation of specific energies would have to originate; yet it is also true that knowledge of sensory physiology has always migrated inward from the stimulus toward the brain. In the seventeenth century it was an achievement to get visual stimulation as far in as the optical image on the retina; in the nineteenth man began to ask how such a colored image gets itself translated into a neural pattern in the optic nerve; in the twentieth we are actually considering the brain. Similarly in hearing, at the beginning of the nineteenth century, investigators were still demonstrating how sound gets from outside the body into the aural labyrinth in the skull; whereas in the latter part of the century they were, stimulated by Helmholtz's theory of hearing, concerned with the processes in the cochlea. Although that change was partly due to physiological discovery in the middle of the cen-

ture, there was also a further shift of interest that ran ahead of physiological knowledge. Men made suggestions about how the ear works; they advocated them on insufficient evidence. A theory of hearing—not Helmholtz's, but many others—thus became an uncertain mixture of physiological fact and physiologist's speculation, an 'as-if' physiology. Any theory of sensation—from 1870 to 1910, say—was adequate if it could describe imaginary physiological relations which 'explained' a majority of the psychological facts without contradicting many physiological facts. In the present century speculation has become less reputable, and the theories of hearing are being reduced to the factual physiology of hearing.

Before Helmholtz

The gross anatomy of the ear has long been known. Empedocles (ca. 490–ca. 435 B.C.) discovered the inner ear, and Galen (129–199 A.D.), naming it the labyrinth, regarded it as the essential nervous organ of hearing. Galen described the pinna and its muscles, the external meatus, the drumskin, and the distribution of the nerve to the labyrinth. About the middle ear he had, however, little to say, and certainly he knew nothing of the essential structure of the labyrinth.

After Galen there was a long period of anatomical observation, concerned often with the structure of the ear, as the copious footnotes of Haller's *Elementa physiologiae* (1763) show. It is enough for us here to realize that the details of aural anatomy began to reveal themselves in the sixteenth and seventeenth centuries. Bartolomeo Eustachi wrote *De auditu organis* in 1562, describing there and in a set of essays dated a year later, the ossicles, the tensor tympani and the Eustachian tube. He did not discover this tube; Alcmaeon (sixth century B.C.) knew that there was an opening between the mouth and the ear. Later G. Casserio in 1600 gave a more extended description of the tympanic membrane, the ossicles, their muscles and the gross structure of the cochlea, and C. Folio in 1654 described the semicircular canals. The whole subject was brought into excellent systematic shape by G. J. Duverney's thorough treatise on the ear in 1683.

In this way it came about that Albrecht von Haller, considering the ear in 1763, knew as much about its gross anatomy as the modern psychologist needs to know. He described the external auricle,

the auditory meatus, the elastic tympanic membrane (conical inward and vibrating for sound), the tympanum, the Eustachian tube for adjusting air pressure, the three ossicles (malleus, incus, and stapes), the tensor tympani and two (though actually there is only one) other muscles, the oval window and the round window opening into the labyrinth with the stapes closing the former, the vestibule with the "nervous pulp" in it, the conical cochlea with its two and a half turns, its spiral plate and its vestibular and tympanic canals, and the semicircular canals. About the ampullae of the canals he did not know, nor about the microstructure of the cochlea. Assuming that the canals are organs of hearing, he said that the functions of the cochlea are obscure; but he made, nevertheless, a conjecture about the cochlea that anticipated Helmholtz's resonance theory by one hundred years. Actually he noted that the "transverse nervous filaments" between the spiral plate and the wall of the cochlea become successively shorter as the tip of the cochlea is approached from the base, and he suggested that these "nervous cords, continually shortening their lengths, [are] by that means adapted to an harmonical unison or consonance according to the variety of acute or grave sounds, so as to tremble together in the same time with most of them, namely, the longest cords in the basis of the cochlea, with the grave sounds, and the shortest cords nearer the tip or apex, with the sharper sounds." He was, of course, wrong in detail. The transverse fibers in the cochlea are not nervous; and the long ones are situated, paradoxically, at the narrow tip of the cochlea, and the short ones at its broad base. Nevertheless Haller, in noting the existence of fibers and of the tapering space for them, actually did suggest resonance as the mechanism whereby acute and grave sounds may be separated in the ear.

Because the necessary knowledge about the microscopic anatomy of the inner ear waited upon the improvement of the compound microscope about 1830, there was not much important progress made in the first half of the nineteenth century. Cooper in 1801 described cases in which the tympanic membrane and the bones of the middle ear had been lost and in which hearing, after an initial deafness, recovered surprisingly well, although the original sensitivity was not entirely regained. Wollaston, who claimed to be able to increase or diminish the air pressure in his middle ear by blowing or sucking through his Eustachian tubes with his mouth

and nose closed, described in 1820 the clicking noises made by changing pressure on the tympanic membrane and the way in which acuity is reduced by such unequal pressure. That the function of the Eustachian tube is to equalize air pressure on both sides of the tympanic membrane became pretty clear, although Johannes Müller (1838), for one, was not wholly convinced of this function. Savart in 1824 managed to show that the tympanic membrane vibrates when a sound impinges upon it. He put sand on the membrane, as Chladni had done with metal plates, and observed its movement when the ear heard a sound.

Sir Charles Bell in his *Anatomy of the Human Body* in 1809 discussed the topic of hearing at length, repeating the standard anatomical facts that Haller had cited. Supposing the entire labyrinth to be the organ of hearing, he suggested that the cochlea, so much more highly developed in quadrupeds and man, may function for the finer auditory discriminations and for musical perception. Since there are, he supposed, from 400 to 500 distinguishable pitches, and also as many loudnesses, the cochlea would have to respond differentially to as many as 20,000 different tones.

Bell's discussion is especially remarkable, however, for his correct apprehension of the function of the round window. At that time it was coming to be supposed—and Johannes Müller still held this view in 1838—that the ossicles conduct sound directly to the oval window and that the air of the tympanum conducts it to the round window, affording thus alternative modes of transmission. The fact that hearing recovers after the loss of the ossicles supported this view. Bell pointed out that the stapes, up against a closed vessel filled with liquid, which is practically incompressible, would be unable to move at all except for the fact that the round window provides relief. He was of the opinion that the direct action of the sound upon the membrane of the round window could not be very great, partly because air in the tympanum would be a less effective conductor than the bones and partly because the action at the round window, if considerable, would oppose the action at the oval window so that the two effects would tend to nullify each other. He even suggested (correctly) that the position of the round window would, therefore, in part determine the course of the movement through the cochlea from the oval window. He did not, however, apply this logic to the semicircular canals,

which have no relieving membrane, and ought therefore not to function with the activation of the oval window.

In 1824 Flourens published his first findings indicating that the semicircular canals operate in the reflex orientation of the organism. His chief papers on this subject appeared in 1830. Eventually this discovery was to be established, making it clear that the canals do not function in hearing. (See pp. 536-543.) New fact is, however, slow to be accepted; Johannes Müller in shaping his theory of hearing considered neither Bell's argument about the round window nor Flourens' evidence about the canals.

Müller, then, in his authoritative account of the ear in 1838, reverted to the theory that sound is conducted in two ways through the tympanum—by the ossicles to the oval window, by the air to the round window. After he had undertaken research on the transmission of sound from one kind of medium to another, he concluded that conduction would be more effective from the solid bones to the liquid in the labyrinth than from the air to the liquid. He thought of the sound as being transmitted through immobile ossicles in the way that sound passes through any solid body, for he assumed that the incompressible liquid in the labyrinth would prevent these bones from moving. Supposing too that the sound is transmitted from the stapes to the canals and from the round window to the cochlea, he suggested that the lesser effectiveness of transmission through the round window might be compensated for by the much greater amount of nerve spread out within the cochlear spiral. The problem that Ohm's law was to solve five years later Müller recognized, but, not realizing how Fourier's theorem would apply, he hazarded the solution that primary and secondary maxima and minima of a compound wave might excite the nerve with the different component frequencies. That was not an adequate analysis of the problem.

It must be remembered (pp. 70-72) that at this same time formal promulgation was given by Müller to the law of specific nerve energies, a law which held that difference in quality depends upon difference in the nerve excited. Müller himself applied the principle only to the differentiation of the five senses, but Natanson in 1844 extended the principle to separate qualities within the single sense—to red, yellow, blue, temperature, touch, sweet, sour, bitter and the simple smells. When, however, Natanson lacked the

boldness to suggest that every pitch must have its own specific energy, that extension of the principle was left for Helmholtz to make. That Helmholtz had such courage of conviction as to suppose the existence of more than 5000 specific energies for hearing (with a separate fiber, of course, for every energy) was one reason why both Müller's doctrine and Helmholtz's theory became so important. The logic was sound, but it contradicted habitual modes of thought.

Just at the middle of the century the problem became the beneficiary of the new wave of research in microscopic anatomy, made possible by the improvement of the microscope. The new techniques were applied to the study of the inner ear. Corti's study of mammalian ears came out in 1851, Reissner's in 1851 and 1854, and Kölliker's in 1854. The *organ of Corti*, *Corti's rods* and *Reissner's membrane* are terms that perpetuate these contributions. This was the sort of information available to Helmholtz in his first formulation of his theory, though later Hensen's studies (1863) caused him to revise his theory. It was not until 1884, however, that Retzius published his classical description and charts of the human ear, the drawings that have for so long been copied in textbooks of physiology and psychology. Thus it was in this period of about thirty-five years that the most important work was done on the microscopic anatomy of the inner ear: the form and dimensions of the organ of Corti and certain inferences as to its probable kinematics were all worked out. The psychologists who needed physiology for speculative theories now had it.

Resonance Theory of Hearing

The resonance theory of hearing, a scientific achievement of first importance and one which has dominated work in this field during the eighty years since its inception, was formulated by Helmholtz in his *Tonempfindungen* in 1863. It is definitely a product of Helmholtz's clear thinking, of his ruthless logic and scientific objectivity; nor are we belittling these attributes of Helmholtz's if we also note that 1863 was just about the right time for such a theory. The necessary facts and premises were at hand for his use.

To make the theory Helmholtz drew upon four bodies of information until then largely unrelated. (1) In the first place, he knew the physiological acoustics of the ear—that sound is con-

ducted from the tympanic membrane through the movement of the ossicles to the oval window and confined to the cochlea by the position of the round window, which in turn allows the stapes to move and its displacement to be transmitted through the cochlea but not through the semicircular canals. Thus his theory depended upon Charles Bell's observation about the function of the round window (1809) and Flourens' establishment of the non-auditory function of the semicircular canals (1830). Johannes Müller did not use these data (1838); Helmholtz did. (2) In the second place, Helmholtz had—as Haller and Bell and Müller had not—some knowledge of the microscopic anatomy of the inner ear; this was due to the work of Corti (1851) and others who had taken advantage of the improvement of the microscope to examine the receptor mechanism of the cochlea. In this respect his theory was somewhat premature in 1863 because, choosing then the wrong microscopic resonators (Corti's rods), he had later to make a change when Hensen's histological research became available. (3) Then he had for use Müller's doctrine of specific nerve energies, a rule that difference in quality always means difference of conducting sensory fiber. It was his 'ruthless logic' which made him extend to all the pitches the principle that Müller had proved only for the differentiation of the five senses. Helmholtz, regarding Müller's theory as established fact, compared its importance for psychology to the importance of the theory of the conservation of energy for physics; yet, since its application to hearing has never been entirely accepted, we must regard his extension of the law more as a premise than a fact. Helmholtz, moreover, was handicapped by having no good psychophysical data on the number of discriminably different tones. He took Seebeck's value of 1846, which showed the number of pitches to be of the order of thousands, not hundreds or tens of thousands. (4) Finally, Helmholtz had Ohm's law of 1843, which depended in turn upon Fourier's theorem of 1822 and which had been given great importance by Helmholtz's own studies of vowels and timbre in the 1850's. Because it was clear that the ear makes the same analysis as would be made by a large set of resonators, it followed that the ear must in reception be a resonating organ. It remained only for him to identify in the microscopic anatomy of the cochlea the appropriate resonators. Put in this way it seems almost as if Helmholtz's elaborate argument for the theory amounted to little more than a statement of the ob-

vious, but it was Helmholtz's achievement to bring unrelated data together and in so doing to make their mutual significance obvious. After all, the best logic is obvious.

Helmholtz chose in 1863 the rods of Corti as the probable resonators. Corti had described them clearly. The rods along with the whole organ of Corti are large at the tip of the cochlea and small at the base. The facts that we have reviewed in the preceding paragraph asserted the existence of a graded series of resonators; and here actually was a series of objects, graded in size and so placed as to stimulate, if they vibrated, the hair cells in which the nerve fibers originate. Naturally enough, Helmholtz took them to be the resonators. Later in 1863, however, Hensen published two studies. One showed how the particular hairs in the hearing sacks of crustacea respond by resonance to particular tones, thus making the resonance of organic structures plausible and eventually persuading Hensen to adopt a theory of human hearing in which the hairs of the hair-cells are themselves the resonators. The other paper was a study of the structure of the cochleas of men and mammals which showed that the basilar membrane is a 30-mm. trapezoidal strip, consisting of a series of transverse fibers of graded lengths, varying from about .495 mm. at the tip of the cochlea to .041 mm. at its base—an ideal series of resonators, resembling a harp or a set of piano strings. In 1869 Helmholtz in an article changed his opinion and argued for the fibers of this membrane as the essential resonators. In those theories of hearing that appeal to resonance to account for Ohm's law, the basilar membrane has ever since, with few exceptions, been regarded as the resonating system.

There were five chief difficulties with Helmholtz's theory, and most of the other theories that it provoked were attempts to meet these difficulties. Let us enumerate them so that we can have them in mind.

(1) It was necessary to find an adequate set of resonators in the inner ear. Hensen's measurements showed the ratio of the longest basilar fiber to the shortest to be about 12, and later figures showed even a smaller ratio. A ratio of 12 accounts only for a range of three and a half octaves, whereas from 20 to 20,000 c.p.s. there are ten octaves. Some persons rejected the theory on this account. Only much later did Gray (1900) note that the shorter fibers might also be under greater tension and Roaf (1922) that the longer fibers are

the most heavily loaded by the columns of lymph that move with them.

(2) The small size of the organ of Corti created skepticism. How could such tiny structures resonate to such large waves? A tone of 100 c.p.s. has a wave-length of about 330 cm., which is eleven times the length of the entire basilar membrane and about 700 times the length of the fiber that is supposed to resonate to it. This argument was not physically sound: the whole wave does not have to be within the cochlea at once for its frequency to be effective at one point. Various theorists, nevertheless, tried to meet the objection by constructing tiny *camerae acusticae* to show that they would resonate. None of them was, of course, nearly so small as the inner ear itself.

(3) The most serious objection arose in connection with the width of the strip of the basilar membrane that must be excited. Most critics noted that the transverse fibers, connected as they were with each other, could not vibrate independently. Helmholtz himself, recognizing that their tuning would not be sharp, calculated the degree to which adjacent fibers would be forced to respond. Implicitly this objection was met by the assumption that pitch would be determined by the point of maximal stimulation—a premise elevated by Gray (1900) into a formal principle.

(4) Others argued that the resonance of the basilar membrane would consist in a pattern of activation, as is shown by the sand on one of Chladni's bowed metal plates. Although Ewald's (1899) and Lehmann's (1910) *camerae acusticae* seemed to show this result, there was, as we shall see, other evidence favoring a simple serial arrangement of resonances in the ear itself.

(5) Finally there was the quite natural premise of parsimony: five thousand or ten thousand specific nerve energies were too many when the eye was getting along with three and the skin was soon to settle down to four. Of course, this analogy was not quite fair, for the eye and skin, having to mediate spatial as well as qualitative patterns, thus have their myriads of 'local signs.' Nevertheless there were efforts, as we shall see, to reduce the necessary number of specificities, like Mach's reduction (1886) of them to two.

In 1870, when Helmholtz had got his theory adjusted to the new data about the basilar membrane, there were four important tonal

phenomena of which a theory ought to take account. (a) First, there was Ohm's law. Helmholtz's theory was built to take account of it. Because it was basic to an understanding of all complex sounds and because it had only recently been recognized, it was at that time the most important fact about tone. (b) Then there was intensity, which nobody thought about, because everyone took it for granted that the nerve fibers could respond with different degrees of activation. After the discovery of the all-or-none law (1912) this phenomenon became a problem. (c) There were beats. Helmholtz explained beats by an appeal to the width of the strip of the basilar membrane which responds to a tone. The strips overlap when the tones are close together, and the beats arise in the common portion. Thus one of the objections to the theory—the lack of sharpness of tuning in the basilar membrane—became the means for explaining beats. (d) And finally there were the combination tones. Helmholtz, who had shown that they can occur outside the organism, suggested that the ossicles of the middle ear may be their place of origin when they are subjective. That, then, was the Helmholtz theory in 1870.

For the next twenty years there was not so very much criticism of the theory. Then objections, modifications and new evidence began coming in. We may mention a few of the more important events.

Mach, in his *Analyse der Empfindungen* in 1886, made an effort toward parsimony of specific nerve energies. Just as the circular series of hues could arise from mixtures of three specific components, so, he suggested, might the open linear series of tones come from two specific energies: *D* (*dumpf*, dull) and *H* (*hell*, bright). Any tone would be given by $pD + qH$, where $p + q = 1$. Unfortunately that analogy to colors breaks down in the case of mixtures. On Mach's theory *c* and *c'* should mix in the right proportions to give *g*. He had to complicate his theory by suggesting a third component, *Z*, that would account for the richness of timbre; and with such gratuitous fabrication the theory failed. Köhler in 1913, in the temporary belief that there are tones of pure vocality an octave apart with mixed vocalities in between, applied Mach's theory of vocality within the octave—making it much more plausible but still not avoiding the same difficulty.

In 1900 A. A. Gray contributed the principle of maximal amplitudes as the solution of the difficulty raised by the lack of sharp

tuning in the basilar membrane. He made the comparison with touch. An extended cutaneous region can be localized with an error less than the diameter of the region because the area is considered to be where its center is. Similarly pitch could depend physiologically on where the center of the basilar excitation lies, that is to say, at the point of maximal amplitude. A noise, Gray thought, would be like a tactually perceived line, having approximate but not exact punctual location. This principle was, to be sure, implicit in Helmholtz, but Gray by using the tactual analogy made it more plausible.

Gray also at this time was the first person to note that the tension on the short basilar fibers might well be greater than the tension on the long ones, thus increasing the range of tuning from three and a half octaves (due to length alone) to something more like the total audible range. From the base to the tip, the cochlear tube gets smaller, the basilar fibers longer, and the spiral ligament—which occupies the space between the outer end of the fibers and the wall of the tube and which presumably keeps the fibers under tension—smaller. The short fibers in the large end of the tube leave room for a huge ligament, but for the long fibers there is only a small ligament. To this fact Roaf in 1922 added the consideration that the long fibers at the far end of the cochlea are weighted by the inertia and friction of long columns of lymph in the cochlear canals, columns that have to move between the oval and round windows when the long fibers are activated; whereas the short fibers, near the base of the cochlea, are weighted only by short columns. Wilkinson, writing in 1924 a defense of the resonance theory in association with Gray and without knowledge of Roaf's paper, made the same observation. Thus length, tension and weight could all be regarded as entering in to account for the tuning of the basilar membrane through ten octaves of audibility. The surprising thing is that, with all the talk about the resonance theory, it took thirty years for someone to think about the differences in tension and twenty more for someone else to think about the difference in weighting, two principles that should be obvious to anyone as sophisticated about tuning as was Galileo's pupil, Mersenne, in 1636. The next twenty years were to show that even this view was much too simple.

In general, the theorists who believed in resonance stuck to the basilar fibers as the resonators. An exception is Shambaugh. He, in

1907 and later, argued that the resonators must be the fibers of the tectorial membrane. The basilar membrane is too stiff, he said, its fibers are too rigidly connected to one another, its tension is too much dependent upon blood pressure from the blood vessel under it, for it to act as a series of resonators. The tectorial membrane is really, so his histological preparations showed, attached at both ends, not at one only; floating in the endolymph, it would easily respond to the impulses transmitted through the cochlear canal. This theory never gained acceptance, perhaps because the tectorial membrane does not show a graded series of resonators.

The theory of basilar resonance was supported by the construction of *camerae acusticae*. M'Kendrick built one which he described in 1900. It consisted of a little box with glass sides (cochlea), a horizontal partition extending from one end almost to the other (spiral plate), two windows, one above and one below the partition at the end and covered with membranes (oval and round windows), two membranes covering two openings in the horizontal shelf (basilar membrane) with watch springs of different natural periods suspended from them. A piston actuated the membrane corresponding to the oval window. At 12 c.p.s. the larger watch spring vibrated; at 24 c.p.s., the smaller. With these two frequencies compounded in the motion of the piston, both springs vibrated. Thus Helmholtz's principles were shown to hold for this system.

In 1910 Lehmann arranged to transmit different rates of vibration to a strip of rubber membrane, wide at one end and narrow at the other with transverse tension applied to it. When, in order to observe the vibration, he put sand on the rubber, he found that the membrane was activated most strongly in the strip most distant from the base of activation when the vibrations were slower. Although this result supported Helmholtz's theory as to place of resonance, Lehmann was also concerned to point out that the vibrating band was too wide to give the sharp tuning required in the ear.

Other evidence came from the tonal gaps. Stumpf in 1883 examined the hearing of certain persons for whom there were gaps in the tonal series. Certain frequencies gave no pitches. Bezold too in 1897, describing such tonal gaps and the consequent tonal islands which he found in a number of persons with defective hearing, noted that a defect of this sort is evidence for the separate and

serial localization of pitches in the ear. An injury would hardly filter out a band of frequencies unless it did so by destroying a part of a series of resonators.

Somewhat later there were several experiments in which animals were exposed for a long time to certain intense frequencies, one frequency for each animal, and their organs of hearing afterward examined histologically. The first experiment of this sort by Wittmaack in 1907 was inconclusive, except that it showed the organ of Corti to be affected and the organs in the vestibule not. Later Yoshii in 1909 found that the exposure-frequency determined in general the part of the cochlea where destruction occurred—the lower tones toward the tip and the higher toward the base, as the Helmholtz theory requires. (Fig. 62.) The experiment, however, also told against the theory; since a single frequency caused the virtual destruction of a large region of the organ of Corti, localization of pitch was indicated by this method to be ever so much less exact than it really is.

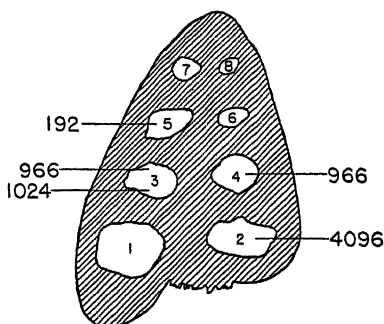


FIG 62. DEGENERATION OF ORGAN OF CORTI DUE TO LONG EXPOSURE TO TONE: YOSHII (1909)

Cross-section of 8 half-turns (as numbered) of the cochlea of a guinea pig, showing region in which degeneration occurred after long exposure to loud impure notes with fundamental frequencies of about 192, 966, 1024 and 4096 c.p.s. The region of injury is large, but the high tones are near the base of the cochlea and the low tones nearer the apex, as required by Helmholtz's theory.

When the electrophysiological methods came into use about 1930, the evidence for the resonance theory, though not conclusive, was plentiful. The case for the theory was put excellently and forcibly by Waetzmman in 1912, by Hartridge in 1921–1922, and by Wilkinson and Gray in 1924. In a later section we shall see what the new physical technique did for the problem.

Other Theories of Hearing

The constructive criticism of Helmholtz's theory took the form of the creation of other theories. These were all theories of pitch,

since it was taken for granted that intensity depends upon the degree of excitation of the single fiber. The theories differed in two respects. (1) There was the question as to the physiological correlate of pitch, whether it is (a) the place excited in the organ of Corti, as Helmholtz's theory had it, or (b) the pattern of pressures in the organ, or (c) the frequency of excitation (functioning independently of the particular fiber conducting the particular frequency). (2) Then there was the further question as to how the frequencies are separated in accordance with Ohm's law, whether (a) by resonance or (b) by some other mechanical or acoustical principle in the inner ear. Let us consider the five principal kinds of theory that were evolved in respect of these principles.

1. *Place-resonance theories*. These are the theories discussed in the preceding section. In them the component frequencies are regarded as separated out by resonance, each at its own place, and the particular fiber stimulated determines the pitch heard. Thus this type of theory is always one which employs the principle of the specific energy of nerves; that is the essence of a place theory—it makes particularity of fiber the determinant of quality.

2. *Pattern-resonance (pressure-pattern) theories*. It was the assumption underlying these theories that the basilar membrane responds to stimulation with a pattern of activation, much as Chladni's sound-plates form a pattern in the sand strewn upon them when they are bowed to give a particular tone. Though such theories are really resonance theories in that the properties of the responding membrane are partial determinants of the pattern, they do not lend themselves readily to an interpretation in terms of specific nerve energies. In them a pitch would depend upon a pattern of specific energies, not on a single energy or a simple relation of a few. It is true, of course, that the Young-Helmholtz theory of vision assumed a pattern of specific energies as the determinant of visual quality, but that analogy was not urged.

The first pressure-pattern theory was put forward briefly by Waller in 1891. He argued simply that the tympanic membrane would respond to different simple and compound tones with different patterns, and that it was reasonable to assume that these patterns are similarly impressed upon the basilar membrane. Such a view was reasonable only for those who believed the objections to the Helmholtz theory to be insurmountable.

Ewald in 1899 and 1903 developed the theory fully. He performed experiments upon a membrane in which standing waves were set up by an acoustic stimulus. The resultant patterns he photographed by reflecting light from the membrane, thus avoiding any alteration of the period of the membrane by putting sand upon it. He found, in general, that the distance between bands of the pattern increased when the wave-length of the exciting tone increased. Since his *camera acustica* did not exactly duplicate the ear, he had no proof that his theory was right; yet the effect of his experiments was to weaken the Helmholtz theory.

3. *Frequency-non-resonance* (telephone and leather chair-seat) theories. In 1886 Rutherford suggested that the ear works like a telephone, merely transmitting the simple and compound frequencies along the auditory nerve to the brain. He had noticed that a fairly high frequency can be put on a motor nerve. Ten electric shocks per second may result in ten twitches of a frog's nerve-muscle preparation, but forty shocks per second hold the muscle contracted in tetanus yielding what can actually be heard as a low-pitched tone. Rutherford reported that he could hear tones from such nerve-muscle preparations all the way up to 352 c.p.s. but only noises at higher frequencies. The recency of the invention of the telephone (1877) and his surprise at finding nerves capable of conducting such rapid frequencies, both predisposed him to believe that the analysis of Ohm's law must take place, not in the ear, but in the brain.

Perhaps the best publicized theory of hearing, next to Helmholtz's, was Max Meyer's, for no other theorist has equaled him in persistence and enthusiasm. Because it denies resonance and depends ultimately on frequency as the essential condition of pitch, his theory is really a frequency-non-resonance theory. He formulated it in 1896, extended its application greatly in 1898, and gave it his most explicit exposition in 1907. Dubbed the "leather-chair-seat theory," it denies the elasticity of the basilar membrane, likening it to a leather chair-seat, which, when pressed up, stays bulged up, and, when pressed down, stays bulged down. Meyer argued that the stapes, in moving in at the oval window, starts to depress the basilar membrane, and that this depression travels along from the base of the cochlea toward the tip until the inward movement of the stapes is completed. Then, as the stapes begins to move outward, a contrary action begins at the cochlear

base: the basilar membrane is pulled up continuously, until the stapes on its outward pull has undone the effect of its inward thrust. The result would be that a single harmonic frequency would cause a stretch of the basilar membrane to move up and down at the rate of the stimulus, and that the amount of the basilar membrane involved would depend upon the amplitude of the stimulus.

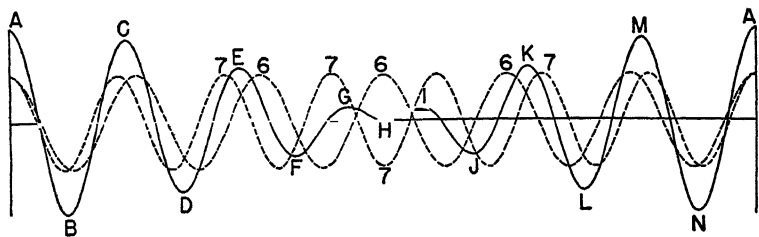


FIG. 63. COMPOUND WAVE OF COMPONENTS IN RATIO 6:7

The figure represents one period of the compound wave whose two components are in the ratio 6:7; i.e., if this period is .01 sec., the two components would be 600 and 700 c.p.s. This is the wave for which Max Meyer's diagram in Fig. 64 is drawn.

Thus Meyer was contributing a new theory of loudness as well as of pitch. Pitch depends in his theory upon the frequency of excitation of any part of the basilar membrane, and loudness upon the amount of the membrane affected.

Meyer did not, like Rutherford, leave the analysis of complex waves to some mystical property of the brain. He had a mechanical explanation which is diagrammed in Figs. 63 and 64. Let us take the case of the simultaneous presentation of 600 and 700 c.p.s., which in musical terms is approximately a minor third. Fig. 63 shows the compound wave for $\frac{1}{100}$ sec. Fig. 64 shows what, on Meyer's theory, happens to the basilar membrane in that $\frac{1}{100}$ sec. The instants A, B, C, . . . correspond in the two figures. We suppose that the basilar membrane is up at A as the stapes starts in. At B the stapes has completed its maximal excursion, and the membrane is depressed through sections I to VII of its length. Next the stapes starts to move out, and the membrane is pulled up until at C the stapes is about to reverse its direction of motion. Since the stapes does not move out as far as it moves in (Fig. 63), section VII is left depressed. The remainder of Fig. 64 is derived from Fig. 63: the stapes moves less and less in each direction until it

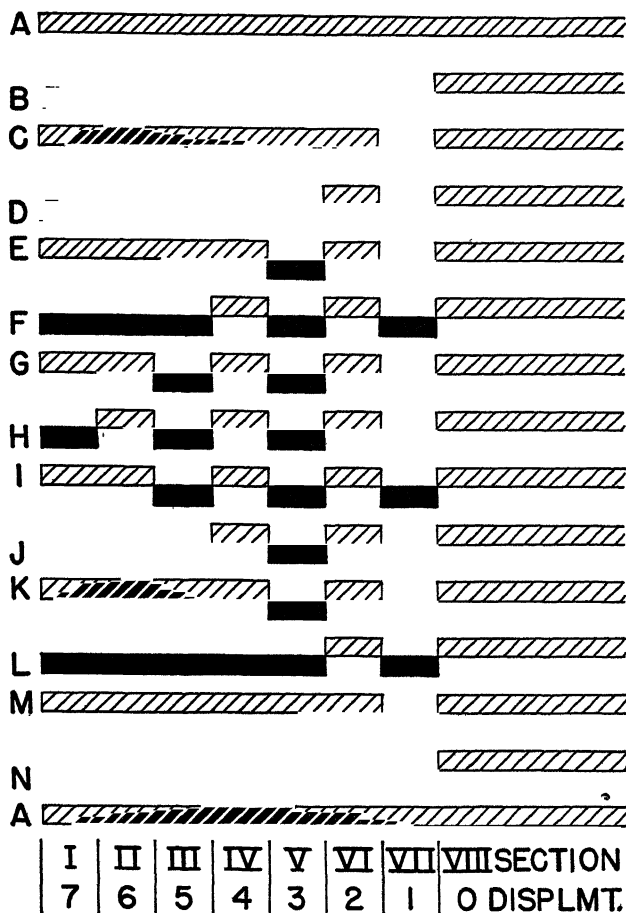


FIG. 64. MAX MEYER'S THEORY OF HEARING (1896-1907)

The figure shows the supposed position of the basilar membrane in 14 successive instants in the complete cycle, when the compound wave in the ratio 6:7 (Fig. 63) is perceived and analyzed by the ear. A, B, C, D, N, A are successive instants and correspond to the points with the same letters in Fig. 63. I, II, III, are successive sections of the cochlea numbered away from the base. When the basilar membrane is up, it is cross-hatched; when it is down, it is solid black. The numbers of displacements (up, down, and up again) in a complete cycle are totaled at the bottom. Section I had 7 such displacements in a cycle; section II, 6; section VII, only 1; section VIII and beyond, none. Thus these three sections furnish the frequencies for the two generating tones and for the first difference tone. See text for further explanation.

reaches its minimal excursion at *H*. Then the system of motion is reversed until after *N* the original state in *A* is again achieved. Counting the number of up-down movements of the membrane shows that section I has had 7 in $\frac{1}{100}$ sec. or 700 c.p.s., which is the frequency of one of the component tones. Section II has had 6 up-down movements in $\frac{1}{100}$ sec. or 600 c.p.s., which, although they are not uniformly timed (there is a long period from *G* to *I*) might give the other component tone. Section VII, reversing 100 times a second, might give the difference tone; and indeed it was the consideration of combination tones that first led Meyer to this theory. It is, to be sure, not clear why the frequencies of sections III to VI might not be heard; but in the days of theorizing about the ear, all theories had shortcomings and *tu quoque* was an acceptable defense.

In 1900 ter Kuile put out a theory of hearing quite similar to Meyer's: it assumed that the frequencies which determine pitch are the result of successive positive and negative bulges traveling up the cochlea from the base. For simple harmonic motion such a theory is adequate, but for compound waves it becomes involved. Ter Kuile, who worked without knowledge of Meyer's theory, later acknowledged the similarity between their basic principles.

In general, frequency theories have been favored by the discovery that dichotic time and phase may determine auditory localization. In 1926 the author of this book argued that a frequency theory of pitch provides physiological explanations for loudness, tonal volume and dichotic localization. It seems, however, that he did not go far enough; he should have kept to resonance too. Undoubtedly he was blinded by the commonly accepted opposition between the principles of resonance and of frequency, whereas these two principles are not opposed and may be combined, as already they had been by Wundt and Ebbinghaus (*vide infra*).

4. *Place-non-resonance theories*. There are at least two theories which, rejecting resonance, yet managed to find a way whereby frequencies might be localized in the cochlea and pitch regarded as dependent upon specificity of excited fiber. The classical theory of this sort was Hurst's in 1895. Because as few as two complete waves may give rise to a determinate pitch, he held the resonance theory to be impossible, as well as "all new resonance theories of hearing that may ever be propounded in the future." His theory

assumed that the sound wave passes up the vestibular canal and back down the tympanic canal to the round window, the ascending and descending waves interacting through the elastic basilar membrane to set up standing waves, the most prominent of which would fix the place that determines the pitch. This physical relation has never been established, and it is unnecessary to go into the complicated physical analysis on which it was based.

The other theory of this type was Watt's, formulated in 1914. Like ter Kuile's in its description of the mechanics of excitation of the ear, it insists, however, that it is the place reached by the traveling bulge which determines the pitch, not the frequency with which it is excited. Watt decided that pitch is really spatial, a localization in the cochlea, and that tonal volume is the spread of the excitation from the point of localization. Pitch is, nevertheless, not "cognized as spatial" although volume is—or so Watt thought. This analogy of pitch to tactual localization comes up every now and then; we have already seen how Waller made it in his pressure-pattern theory.

5. *Frequency-resonance theories.* There is no reason why a resonance theory should not also be a frequency theory; that is to say, frequency, rather than specificity of fiber, may determine pitch, and yet the frequencies may ordinarily be sorted out onto different nerve fibers by resonance. The first theory of this sort was Hensen's, contemporaneous with Helmholtz's in 1863. Although he was convinced of resonance because the hairs in the hearing-sacs of crustacea respond selectively to different frequencies,³ he was not prepared to accept the theory of specific nerve energies for the pitches.

Much later Wundt adopted this combination of principles. He accepted nearly all of Helmholtz's theory as it pertains to the action in the cochlea but rejected Helmholtz's belief that difference tones arise in the middle ear. Subjects can, he found, hear difference tones for two tuning forks held against the skull, and such "subjective difference tones" must be due, he believed, to the beat frequency in the auditory fibers themselves. This theory, which appears first in the 1893 edition of Wundt's *Physiologische Psychologie*, he amplified in 1902 and again in 1910.

Meanwhile, in order to explain difference tones, Ebbinghaus in 1902 had adopted a similar theory. It was his idea that a given

frequency activates not only its proper fiber, but also all the fibers for its "harmonic undertones," e.g., 600 c.p.s. activate the 600-fiber, also the 300-fiber vibrating at 600 c.p.s. in two segments, the 200-fiber vibrating at 600 c.p.s. in three segments, etc. Ebbinghaus wanted to know how 600 and 500 c.p.s. can give a difference tone of 100 c.p.s. Presumably the fibers for 600 and 500 c.p.s. would lie too far apart on the basilar membrane to interact. Their fifth harmonic undertones, however, would be nearer. If the 120-fiber is vibrating in five segments at 600 c.p.s. and the 100-fiber in five segments at 500 c.p.s., then there might be a common region of overlap where the beat-tone of 100 c.p.s. would show up. Such a theory is possible only if pitch is dependent upon frequency and independent of the specific fiber which, for the moment, vibrates with that frequency. If pitch is frequency, then fast beats might be a pitch.

Recently Troland (1929) adopted a frequency-resonance theory more consistent with good physics. By assuming that resonance in the basilar membrane analyzes a compound wave into activated regions corresponding to its component frequencies, and assuming too that the frequencies are preserved in the nerve, he was in a position to account for the role of dichotic time and phase in auditory localization, as well as for other related phenomena. In a way he anticipated the fact of the Wever-Bray effect to which we turn in the next section.

6. *Duplicity theory.* We have already seen (pp. 363 f.) how Jaensch² in 1913 formulated a duplicity theory of hearing, the notion that the cochlea functions for tone and the vestibule for noise.

We have now noticed, and reviewed as space permitted, twenty-one theories of hearing. The scientific progress made is disappointing. Helmholtz's original contribution, was, to be sure, a great advance, his clear thinking and sound physics worthy of admiration; but not nearly so favorable a verdict can be given of his critics. Sincere and industrious, they nonetheless dealt naively with a complicated problem of physical dynamics, and there can be little doubt that they were partially blinded to the deficiencies of their theories by their own ingenuities. In that respect they were being little more than consistent with the period in which they wrote, the period when theories were validated almost as much by the personalities of their authors as by their supporting facts. So im-

portant, indeed, were theories before 1920 that even an inadequate one seemed better than none at all. With the new techniques the atmosphere has completely changed.

The New Physiology of Hearing

Since 1930 there has been a new and effective period of research in psychophysiological acoustics. Primarily the electrical techniques for the control of the stimulus and for the amplification of physiological potentials have been responsible. It is also true that many of the contributions of the present have been made by men better trained in physics for their day than were the older theorists of hearing for theirs. The spirit and atmosphere of Helmholtz have been revived in a period when knowledge, techniques and the number of persons engaged in research are all greatly increased.

New facts about the physiology of the nervous system have also influenced the theory of hearing in this period, just as the new histological observations of the structure of the inner ear had determined Helmholtz's thought. We have already seen (pp. 60 f., 66 f.) how Bowditch in 1871 discovered the all-or-none principle for muscle, how Lucas in 1905 and 1909 established and named the law and indicated that it might apply to nerve, how Adrian in 1912 proved that the principle is a property of motor nerve, how Forbes and Gregg in 1915 argued that the law ought to apply to sensory nerve and, in consequence, invented the frequency theory to explain how sensory intensity can be perceived, how Adriaan in 1926 and immediately thereafter proved that Forbes and Gregg were right, and how part of the verification of the validity of the all-or-none law and the frequency theory for sensory nerve consisted in the research on theories of hearing, research that began with the experiment of Wever and Bray in 1930. It is to these investigations of hearing that we now turn. They depended upon the new discoveries about sensory conduction and also upon the new electrical techniques that had made those discoveries possible.

Although it is not practicable in this text to describe most of the scientific progress achieved in the physiology of hearing since 1930, we may, nevertheless, comprehend its nature. In general, we shall find that there were four principal topics in respect of which these recent researches have yielded positive advances. Let us see what happened.

1. *The Cochlear Response.* When a sound acts upon the auditory mechanism in the normal manner, there are two effects. (a) A neural impulse is set up in the auditory nerve, and its passage along the nerve, consisting in the release of the nerve's own energy, creates 'action potentials,' electrical voltage-differences that are measurable by an 'action current' through a galvanometer. (b) The energy of the sound is also transformed at the cochlea into other differences of potential which can also be measured; it is these potentials, not transmitted along the nerve nor apparently dependent upon the release of nervous energy, which have been called cochlear microphonics (since the cochlea acts like a microphone in transducing mechanical energy into electrical) and which constitute the cochlear response. We are concerned for the moment with the way in which these two phenomena came to be differentiated.

In 1910 Buitendijk measured with the string galvanometer what he supposed were the action potentials of the auditory nerve. With an electrode upon the nerve he noted a considerable deflection of the galvanometer for a percussion shot, and even for the ticking of a clock and the sounding of a flute. The abrupt sounds were synchronized with their stimuli, and the latent time was very short—of the order of .005 sec.

Much later the question of whether the frequency theory of hearing could be settled by physiological experiment led Forbes, Miller and O'Connor in 1927 to see whether rapid successions of sounds could be detected by a string galvanometer and thermionic amplifier, measuring action potentials in the auditory tract in the medulla. Their best results were obtained with a rapid series of clicks from a watchman's rattle. Here they could get synchronism between the stimulus and the action potentials up to rates of 200 per second, although the best records were obtained for frequencies less than 100. There was nothing in these results to make them favor a frequency theory of hearing, as there would have been had they obtained synchronism at very high frequencies (well above 1000, say), a result which would have indicated to them that the refractory period of auditory nerve fibers is much less than for other fibers investigated and that high auditory frequencies can thus pass through the auditory nerve.

Then in 1930 came the dramatic experiment of Wever and Bray. The amplified potentials from an electrode placed on the auditory

nerve of a cat they conducted to a telephone receiver in a sound-proof room. Thereupon they discovered that tonal frequencies from 100 to 5000 c.p.s. and human speech were reproduced in the receiver when they were presented to the cat's ear in the other room. That was a result to appeal to the imagination, for it seemed at the time as if one man could talk to another over the cat's auditory nerve. Wever and Bray, however, were conservative and cautious. By shielding the wires and avoiding tonal stimuli that were made of steel or iron (like tuning forks), they ruled out the possibility of microphonic action in the receiving apparatus. They proved that the phenomenon is biological by showing that it ceases shortly after death, is reduced when blood supply to the head is reduced, and is restored when blood supply is restored. Although they considered the possibility of microphonic action in the cochlea itself, they decided (incorrectly) against it.

Perhaps the most surprising thing about this discovery was the existence, as it seemed, of frequencies of several thousand cycles per second in the auditory nerve. There was no other evidence that the refractory period of any nerve is less than .001 sec. To explain their result, however, Wever and Bray presented what they called the "volley theory." They supposed, as had Troland before them in 1929, that different fibers would get out of step with each other, so that one set would fire a volley of impulses at the critical point in one wave of a tone, another set at the corresponding critical point in the next wave while the first set remained refractory, and so on. See Fig. 65. There, for intensity *A*, fibers *a*, *b*, *c* and *d* are discharged at every third wave of the stimulus, remaining refractory for the two intervening waves; but, since the fibers are out of step, the successive volleys come at the rate of the stimulus which, for this intensity, is three times the rate of the nerve. At intensity *B*, which is greater than *A*, the fibers would discharge earlier in the relative refractory period and thus oftener, perhaps at every other wave. The result is the same frequency with a greater summated total excitation. In this way, Wever and Bray, by appealing to both the multiple fiber and the frequency theories of intensity, as laid down by Adrian and Forbes, were able to explain not only how a nerve can conduct a frequency greater than the maximal rate of any single fiber, but also how an electrical detector can again integrate these scattered frequencies into the original frequency-amplitude pattern of the stimulus. The theory

was right, even though not all of the Wever-Bray effect was action potentials of the auditory nerve.

Immediately there arose a controversy about the nature and origin of the Wever-Bray effect. It was biological. Was it neural?

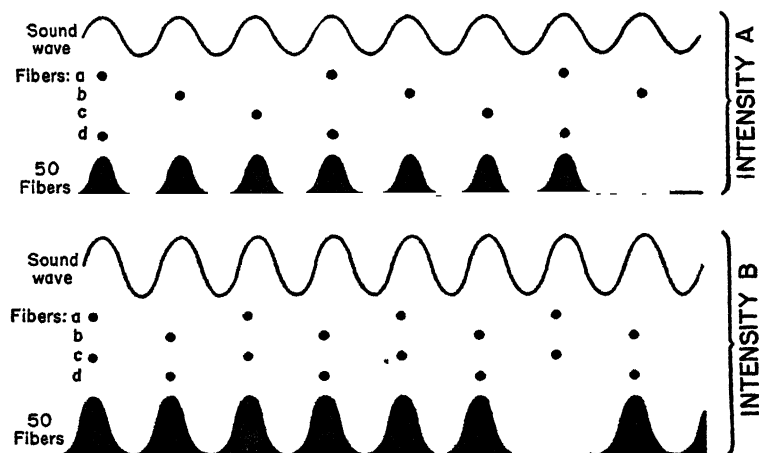


FIG. 65. VOLLEY THEORY OF AUDITORY NERVE-CONDUCTION: WEVER AND BRAY (1930)

Nerve-fibers *a*, *b*, *c*, *d* are discharged at frequencies less than the frequency of the sound wave, but in synchronism with the wave. At intensity *A*, the fibers discharge at every third wave, one-third of them at each wave. At the greater intensity *B*, half of them respond at every other wave. A large number of fibers gives the total response indicated by the black hillocks, a series which reproduces both the frequency and intensity of the original sound. When the intensity is increased from *A* to *B*, the hillocks are higher, since the total number of nerve impulses is greater, but the synchronism with the sound wave is unaltered. From Wever, in Boring, Langfeld and Weld, *Introduction to Psychology*, 1939, 596.

Or might it be microphonic in the cochlea? Adrian early in 1931 argued that it was microphonic because he found that it persisted when the nerve was damaged or narcotized, but later in association with Bronk and Phillips he changed his view, largely because he found the effect abolished by cocaine—a result seeming to imply a neural origin. The first clear differentiation between the two phenomena was made by Hallowell Davis and his associates in 1931 and immediately thereafter. They used as an electrode a fine silver wire passed through a hypodermic needle and insulated from it (a “coaxial electrode”), in such a way that the needle-

casing, which was connected with the ground, formed a shield from the inductive effects of neighboring potentials. Enabled in this way to get action potentials from the nerve with the cochlear microphonics eliminated, they found that the total potentials were not so accurately duplicative of the stimulus as they were in the Wever-Bray effect.

Further research in a very active period of investigation established many of the properties of the cochlear response, as this microphonic action came to be called. The response also provided a general method of investigation, for it taps the chain of causal events at the cochlea and before the nerve. If one puts a pure tone into the external ear and gets harmonics in the cochlear response, then one knows that he has aural harmonics. If high tones give a stronger cochlear response at the base of the cochlea than at the tip, then one makes an inference about the localization of pitches in the cochlea. The Wever-Bray effect opened up this new technique, and Davis's isolation of the cochlear response within the effect was a very important additional discovery.

2. *Action potentials in the auditory nerve.* The new technique with the shielded electrode made it possible to measure the action potentials in the auditory nerve with the cochlear response eliminated. To pick up action potentials in the auditory tracts in the brain also became possible. The main discovery was that the potentials of the auditory nerve reproduce the stimulus, including intelligible speech sounds, with considerable accuracy, that its fibers are nevertheless limited to 1000 discharges per second as a maximal frequency, and that accuracy of reproduction is diminished in the tract in the medulla. These findings are due mostly to the work of Davis and his associates in 1931 and thereafter.

The most important question about the auditory nerve—because its answer bears on the problem of frequency theories and place theories—concerns the means whereby it transmits frequencies greater than the refractory period permits. We have just seen that Wever and Bray, making specific in their volley theory an earlier suggestion of Forbes and Gregg and of Troland, explained this phenomenon as occurring by the successive discharge of “volleys” (Wever and Bray) by different “ platoons ” (Forbes and Gregg) of fibers. Derbyshire and Davis proved that this relation holds for frequencies up to about 3000 c.p.s. As the intensity of a tonal stimulus is increased, the magnitude of the action potential in the

nerve also increases until it reaches a maximum—presumably at a point where all the available fibers are being excited. Derbyshire and Davis, having determined the size of this maximal response for various frequencies, found the function that is plotted in Fig. 66. Here the maximal response is constant for low frequencies since every available fiber is presumably discharged for every cycle of the tone. Then at about 850 c.p.s. there is a sudden drop in the response, indicating that the refractory period for auditory fibers

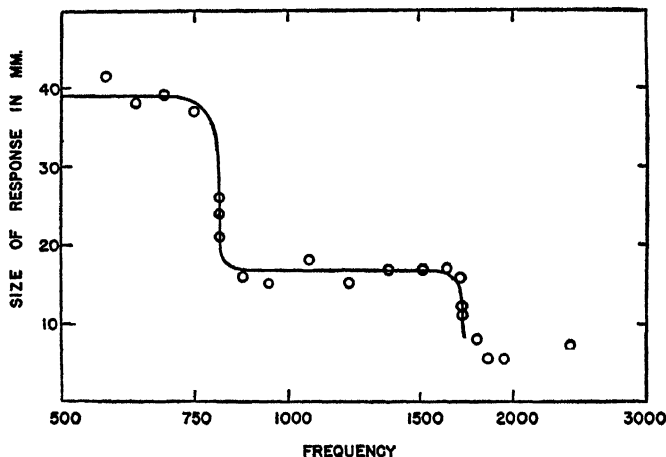


FIG. 66. ACTION POTENTIALS IN THE AUDITORY NERVE: DERBYSHIRE (1935)

The maximal action potential from the auditory nerve of the cat at different frequencies. The sudden drop near 850 c.p.s. implies that the refractory period of the fibers has been reached, and that they are discharging in two platoons at every other wave of the stimulus. The second drop at about 1700 c.p.s. indicates that the refractory period permits discharge at only every third wave. From Stevens and Davis, *J. acoust. Soc. Amer.*, 8, 1936, 9 (based on additional unpublished data of Derbyshire's).

has been reached and that the fibers are beginning to be discharged in the volleys of two alternating platoons. Near a frequency of $2 \times 850 = 1700$ c.p.s. there is another drop where the fibers would presumably be divided into three platoons. Above 3000 c.p.s., however, the evidence of platoon organization is lost, and the fibers presumably fire 'at will.'

3. *Localization of pitch in the cochlea.* The place theory of pitch raises the question as to whether it can be shown by physiological techniques that particular frequencies require particular parts of

the cochlea for their perception, and especially whether the base of the cochlea can be shown to function for high tones and the apex for low.

Direct evidence requires the observation of a correlation between tonal frequencies and places in the cochlea. Frequencies have been measured (a) by noting the frequency of a prolonged stimulus that is expected to damage the inner ear, (b) by determining the frequency at which the cochlear responses are markedly diminished or abolished, and (c) by finding the frequency at which hearing loss is greatest in an audiogram, using the method of the conditioned reflex when the subject is an animal. Place has been fixed (i) by post-mortem histological examination of the organ of Corti for a region of injury and (ii) by observing the place on the outside of the cochlea where the cochlear response is maximal. At least four of these correlations furnished the subject of investigation in 1934-1935.

The method of Wittmaack and Yoshii was revived. After an animal had been subjected to strong stimulation, the region of cochlear injury was determined by histological examination of the cochlea. Guild in 1919 found that animals exposed to loud detonations—he was concerned with 'war deafness'—showed widespread destruction of the organ of Corti in the second to the fourth half-turns of the cochlea. Davis and four of his associates in 1935 found only slight histological damage to the hair cells of guinea pigs subjected to 75 days of stimulation by 600 and 800 c.p.s. at 60-96 db, but great damage from 45 days of 2500 c.p.s. at about 100 db. In no case was the damage narrowly localized; indeed, with 2500 c.p.s. the maximal lowering of sensitivity, as determined by the cochlear response, occurred at 1200 c.p.s. instead of 2500. Kemp at this time reviewed the entire literature of 'stimulation deafness,' showing that the effects are widespread and tend to be maximal for all frequencies (except perhaps the very high tones) around the second whorl of the cochlea. This negative conclusion is, after all, not inconsistent with Yoshii's finding (Fig. 62, p. 411).

In 1934 Crowe, Guild and Polvogt published a report on cases of deafness to high pitches, cases for which they had both an ante-mortem audiogram showing the subject's sensitivity and a post-mortem examination of the cochlea. They found the degeneration for the high frequencies always in the basal turn of the cochlea, were able to localize approximately 8192, 4096 and 2048 c.p.s. and

for the changes are not abrupt. It was possible in this way to scale off the basilar membrane for frequencies. See also Fig. 70.

By the end of the 1930's there had come to be pretty general agreement on at least these points: that the high tones require for

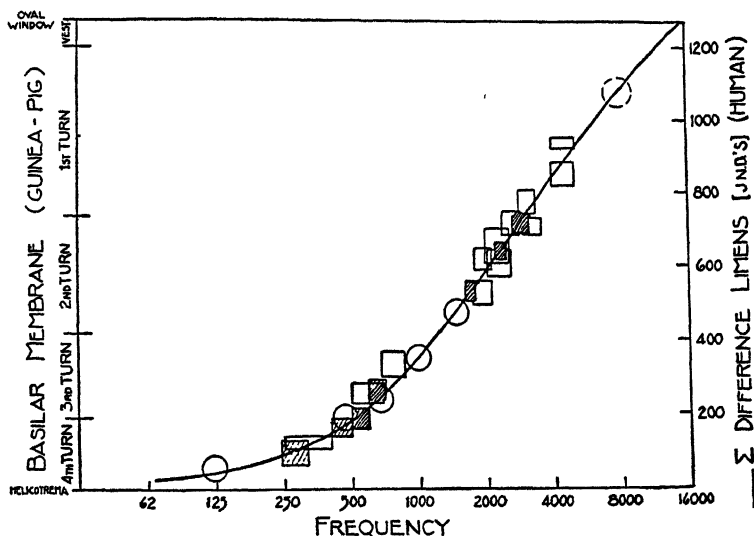


FIG. 69. LOCALIZATION OF PITCHES IN THE COCHLEA: STEVENS, DAVIS AND LURIE (1935)

Localization of frequencies in four turns of the guinea-pig's cochlea. The horizontal position of each rectangle represents the beginning or end of a region of destruction of the organ of Corti caused by boring through the cochlea with a drill and determined afterward by histological examination. The vertical position of each rectangle represents the beginning or end of a region of loss of sensitivity. The widths and heights of the rectangles represent the range of uncertainty of localization, since neither change is abrupt. The solid line is the distribution of frequencies determined by integrating j.n.d. from Knudsen's data, and the fact that the rectangles tend to lie on the solid line seems to mean that j.n.d. occupy equal spaces along the basilar membrane. From Stevens, Davis and Lurie, *J. general Psychol.*, 13, 1935, 312.

their perception the functioning of the basal part of the cochlea, that excitation spreads pretty far for all frequencies and further for greater intensities, and that the spread is so great for the low tones that their localization at the tip of the cochlea is at best only a gross approximation.

Besides all these methods there have been two others. Wegel and Lane, in their early paper in 1924, arranged the frequencies along

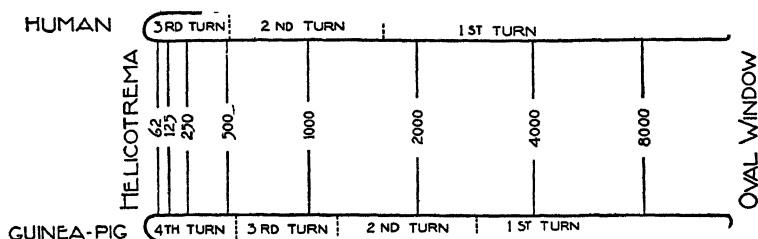


FIG. 70. LOCALIZATION OF PITCHES IN THE COCHLEA: STEVENS, DAVIS AND LURIE (1935)

The generalized graph for the guinea-pig and man, arrived at by scaling off the data of Fig. 69 for the two kinds of cochleas. From Stevens, Davis and Lurie, *J. general Psychol.*, 13, 1935, 312.

the basilar membrane in proportion to the numbers of j.n.d. observed between them, as in Fig. 71. Stevens, Davis and Lurie also counted up the j.n.d. that Knudsen got to construct the curve of Fig. 69, which agrees so well with the localizations they obtained from drilling the cochlea. It would seem that a j.n.d. of pitch is

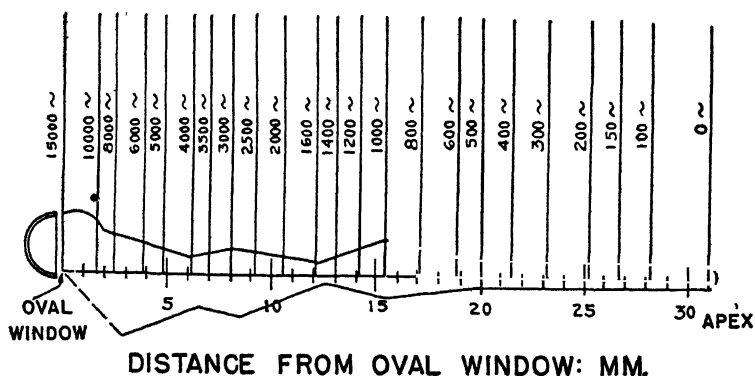


FIG. 71. LOCALIZATION OF PITCHES IN THE COCHLEA: WEGEL AND LANE (1924)

Distribution of frequencies proportional to number of j.n.d. between them. From Wegel and Lane, *Phys. Rev.*, 23, 1924, 279.

related to just so much of a millimeter along the basilar membrane. Fletcher in 1938 mapped the membrane for frequencies by noting the degree to which different frequencies are masked by a thermal noise, arriving at the localizations of Fig. 72, for masking is a

phenomenon that implies overlap of frequencies on the basilar membrane (pp. 365–367).

All in all it is astonishing how well these many methods agree (Figs. 67–72). As Davis showed in 1940, it is hard to plot the different investigators' results on the same coordinates because the various curves so nearly coincide.

4. *Dynamics of the cochlea.* Much more sophisticated physics has been applied to the problem of cochlear dynamics than was represented in the old resonance and pressure-pattern theories.

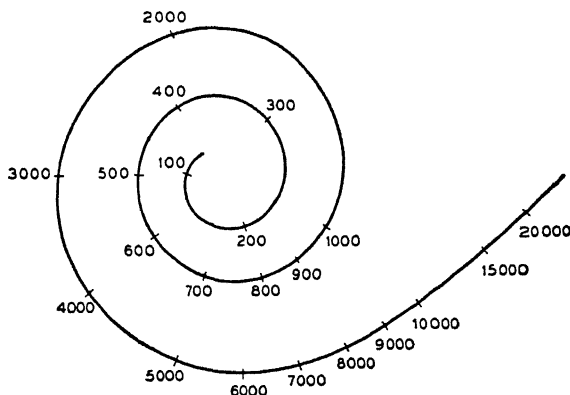


FIG. 72. LOCALIZATION OF PITCHES IN THE COCHLEA: FLETCHER (1938)

Chart based on the method of masking tones by a thermal noise. From Fletcher, *Proc. nat. Acad. Sci.*, 24, 1938, 273.

The first attempt of this sort was made in 1924 by Wegel and Lane, who set up an electrical model of the cochlea, a series of tuned resonating circuits connected in parallel to represent the series of resonators in the Helmholtzian conception of the ear. Because the circuits were separate, however, this model was wrong. The subsequent demonstration of the localization of pitches in the cochlea served to show that the spread of excitation is great, much greater than the simple overlap required by beats. Hence later investigators came to see the problem as one of the total hydrodynamics of the cochlea, of a tube with elastic walls, and not as a series of nearly independent resonators.

The most trenchant analysis of this sort was made by Békésy (1928–1933), who showed that the movement of the stapes in such a system—he had a model and observed the vibration of its mem-

brane by a stroboscope—would establish a wave with a maximal bulge in the basilar membrane near the middle of the cochlea, and that this wave would then travel on to the tip of the cochlea. (Fig. 73.) The speed of the wave would range from about 30 meters per second at the base of the cochlea to about 10 at the tip; the displacements of the basilar membrane would not be in phase with each other. There might be as much as 2 milliseconds required for



FIG. 73. TRAVELLING WAVE IN THE BASILAR MEMBRANE: BÉKÉSY (1933)

As the stapes moves out, the column of lymph toward the base acts as a piston and first elevates the basilar membrane as in the solid line; thereafter the wave travels on to the tip of the cochlea (helicotrema) as in the dotted line. From Békésy, as redrawn by Stevens and Davis, *Hearing*, 1938, 279.

the passage of the wave to the tip of the cochlea, an interval that is long with respect to the refractory period of nerve fiber and very long indeed with respect to the times that are effective in the localization of sound. Fletcher in 1930 made a similar analysis, and Reboul in 1938 developed the differential equations which show how such a system would operate for sounds of different periods. Stevens and Davis summarized and extended this analysis also in 1938. *

The mathematical physics shows that there should be in the cochlea a widespread displacement of the basilar membrane, a displacement which travels from the base to the tip and changes form as it goes. In the sense that the dynamic properties of the cochlea determine the form of the displacement, such a view constitutes a resonance theory; but it is not a resonance theory in Helmholtz's sense of there being a series of nearly independent resonating elements. The Helmholtzian properties of the ear—the fact that the longest fibers under the least tension and the greatest weight are at the tip of the cochlea and that the shortest fibers with the greatest tension and the least weight are at the base—enter into the hydrodynamic system by making the displacement greatest at the tip for low tones and greatest at the base for high.

Put thus simply, the problem would seem to be far advanced;

yet it is hard to see how these long flat waves can determine pitch discrimination, which amounts, as Stevens and Davis noted, to a differentiation of a distance of about .02 mm. on the basilar membrane. Gray's principle of maximal amplitude would hardly seem valid for a wave without a peak. Békésy first, and Stevens and Davis later, have argued, however, that the essential basis for excitation of the hair-cells may lie, not in the amount of displacement, but in the gradient of the displacement, just as a visual gradient of grays is marked off by contrasting rings at the point of greatest change or as a ring of pressure is felt at the maximal gradient when a finger is held in a liquid. In some such way, they think, the necessary spatial specificity required for the discrimination of a thousand or more pitches might be achieved.

While it is clear that the problem of the physics of the inner ear is not yet solved, it is also clear that modern physical analysis has brought it much nearer to solution than did the more personal speculations of the physiologists of the late nineteenth century. Always, however, we must except Helmholtz from such strictures, for he made the greatest contribution in the history of this problem, thinking clearly and impersonally, and combining the courage of speculation with an objective respect for fact.

Notes

Anatomy of the Ear

For Galen's knowledge of the structure of the ear, see H. Werner, *Die galenische Otologie*, 1925, (dissert.). Later anatomical contributions and summaries of previous work were by B. Eustachio, *De auditu organis*, 1562, and also his *Opuscula anatomicae*, 1563; G. Caserio (he was one of Harvey's teachers), *De vocis auditusque organis historia anatomica*, 1600, [n.v.]; C. Folio, *Nova internae auris delineatio*, 1654, [n.v.]; G. J. Duverney (a very thorough account), *Traité de l'organe de l'ouïe*, 1683; Eng. trans. of 2 ed., 1748.

After them came Albrecht von Haller (1708-1777), whose important work was *Elementa physiologiae corporis humani*, 8 vol., 1757-

1766, in which *De auditu* is Vol. V, 1763, Bk. 15, pp. 186-305, a very thorough discussion. A briefer account of Haller's is in his *Primae lineae physiologiae*, 1747; 2 ed., 1751; 3 ed., 1764; Eng. trans. of 3 ed., 1779. His anticipation of the resonance theory is in sects. 494 and 495 of the 3 ed.; I have not seen the earlier eds.

The incidental researches cited for the middle ear are A. Cooper, Further observations on the effects which take place from the destruction of the membrana tympani of the ear [and from the loss of the ossicles], *Phil. Trans.*, 1801, 435-450; W. H. Wollaston, On sounds inaudible by certain ears [including those where the air pressure in the middle ear is too great or too little], *ibid.*, 1820, 306-314; F. Savart, Recherches sur

les usages de la membrane du tympan et de l'oreille externe [sand method], *Ann. Chim. Phys.*, 2 ser., 26, 1824, 5-39.

Sir Charles Bell's account of the ear is in his *Anatomy of the Human Body*, III, 1803, [n.v.], but in the 1809 ed., 232-279, esp. 246-272 for the human anatomy and 272-277 for hearing. On the function of the round window, see p. 273.

For Johannes Muller on the ear and hearing, see his *Handbuch der Physiologie des Menschen*, II, 1838, Bk. v, sect. 2, or later ed., or Eng. trans. Muller's discussion of specific nerve energies is in the introductory portion of Bk. v, but in general on this topic see pp. 69-72.

For M. J. P. Flourens' establishment of the semicircular canals as organs of orientation and thus not of hearing, see pp. 014, 014.

The references on the microscopic anatomy of the inner ear are A. Corti, *Recherches sur l'organe de l'ouïe des mammifères*, *Z. wiss. Zool.*, 3, 1851, 109-169, esp. 131-127 and, for the dimensions of the organ of hearing, 139 (he examined 200 cochleas of beef, pigs, sheep, cats, rabbits, moles, mice and men); E. Reissner, *De auris internae formatione*, 1851, [n.v.]; V. Hensen, *Studien über das Gehörorgan der Decapoden*, *Z. wiss. Zool.*, 13, 1863, 319-412, esp. 397-399 for the resonance of hairs of crustaceans to tones; *Zur Morphologie der Schnecke des Menschen und der Säugethiere*, *ibid.*, 13, 1863, 480-512, esp. Fig. 20 for the dimensions and shape of the basilar membrane; G. Retzius, *Das Gehörorgan der Wirbelthiere*, 1881-1884, esp. for man, II, 328-357 and Plates 33-39 (two huge volumes with many plates of figures now classical); Sir Arthur Keith in T. Wrightson, *An Enquiry into the Analytical Mechanism of the Internal Ear*, 1918, 161-254 (a more recent careful anatomical and kinematic study of the inner ear).

Resonance Theory

This text is not the place to describe the anatomy and kinematics of the inner ear and the organ of Corti, known to every student of a good introductory course in psychology. The reader is supposed to know the positions and functional relations of the oval window, the round window, the vestibular canal, the tympanic canal, the cochlear canal, Reissner's membrane, the spiral plate, the spiral ligament, the organ of Corti, the tectorial membrane, the basilar membrane, the rods of Corti forming the arch of Corti, and the inner and outer hair cells in which the fibers of the auditory nerve originate. If he does not know them, there are innumerable accounts which also discuss critically the resonance theory: J. G. M'Kendrick in E. A. Schäfer, *Text-Book of Physiology*, 1900, II, 1164-1194; E. Waetzmann, *Die Resonanztheorie des Hörens*, 1912; T. Wrightson, *op. cit.*, 35-59 for the simple structure, and *loc. cit.* for Keith's anatomical appendix; G. Wilkinson and A. A. Gray, *The Mechanism of the Cochlea: a Restatement of the Resonance Theory of Hearing*, 1924; S. S. Stevens and H. Davis, *Hearing*, 1938, 249-287.

H. Helmholtz's account of his theory is in his *Die Lehre von den Tonempfindungen*, 1863; 2 ed., 1865; 3 ed., 1870; 4 ed., 1877; the end of Chap. 6 in any ed. Of course, one compares the 1870 ed. with either of the two preceding to note the change from the rods of Corti to the basilar membrane. The actual date of this change is fixed by Helmholtz's *Ueber die Schallschwingungen in der Schnecke des Ohres*, *Verhandl. naturhist. med. Verein Heidelberg*, 5, 1869, 33-38, [n.v., but reprinted in his *Wissenschaftliche Abhandlungen*, II, 582-588]. Helmholtz published no preliminary papers on the mechanism of hearing

before 1863, but it was he who afterwards worked out the exact mechanics of the ossicles: Ueber die Mechanik der Gehörknöchelchen, *ibid.*, 4, 1867, 153-161, [n.v., but in *Wiss. Abhandl.*, II, 503-514]; Die Mechanik der Gehörknöchelchen und des Trommelfelles, *Arch. ges. Physiol.*, 1869, Separatabdruck, [n.v., but in *Wiss. Abhandl.*, II, 515-581].

E. Mach's parsimony of specific energies occurs in his *Analyse der Empfindungen*, 1886 or any later edition, the chapter on Tonempfindungen (Chap. 13, sect. 17 in later editions, but the chaps. in the first ed. were fewer and unnumbered). For W. Köhler's analogous parsimony, see his *Akustische Untersuchungen*, *Z. Psychol.*, 64, 1913, 96-105, esp. 103-105.

A. A. Gray showed how the changing size of the spiral ligament along the cochlear tube might lead to a change of tension on the basilar fibers in On a modification of the Helmholtz theory of hearing, *J. Anat. Physiol.*, 34, 1900, 324-350. The notion that the long basilar fibers are most heavily weighted by the inertia and friction of the columns of lymph in the cochlea was first apparent to H. E. Roaf, The analysis of sound waves by the cochlea, *Phil. Mag.*, 43, 1922, 349-354, and then independently to G. Wilkinson in Wilkinson and Gray, *op. cit.*, 1924, 57-59.

Gray's principle of maximum amplitudes and his analogy between the determination of pitch and localization of touch is in his *op. cit.* (1900). Had Gray known that the tactual error of localization is smaller than the two-point limen for this very same reason, he could have pressed his analogy further. The error of localization measures the accuracy with which the center of the stimulated region is placed, but the two-point limen (three times as large) shows how far away two

regions have to be not to overlap. Thus the perceptual "dumb-bell" or "double paddle," that lies between perceiving one point and two points, corresponds to the case of tonal beats. Cf. E. G. Boring, The two-point limen and the error of localization, *Amer. J. Psychol.*, 42, 1930, 446-449. Also see pp. 484 f. *infra*.

G. E. Shambaugh's first argument for the fibers of the tectorial membrane as resonators is his A re-study of the minute anatomy of structures in the cochlea with conclusions bearing on the solution of the problem of tone perception, *Amer. J. Anat.*, 7, 1907, 245-257. His later articles are Die Membrana tectoria und die Theorie der Tonempfindung, *Z. Ohrenheilk.*, 59, 1909, 159-168; Das Verhältnis zwischen der Membrana tectoria und dem Cortischen Organ, *ibid.*, 62, 1910, 235-240 (which has the best plates of the new preparations of the organ of Corti).

On models of the inner ear and camerae acusticae, see M'Kendrick, *op. cit.*, 1900, 1182 f.; A. Lehmann, Ueber die Schwingungen der Basilar-membran und die Helmholtzsche Resonanztheorie, *Folia neuro-biol.*, 4, 1910, 116-132; Wilkinson and Gray, *op. cit.*, 1924, 82-105.

On tonal gaps and islands, see C. Stumpf, *Tonpsychologie*, I, 1883, 411-419; F. Bezold, Demonstration einer kontinuierlichen Tonreihe zum Nachweis von Gehördefekten, insbesondere bei Taubstummheit, und die Bedeutung ihres Nachweises für Helmholtzsche Theorie, *Z. Psychol.*, 13, 1897, 161-174, esp. 172 f.

The investigations of the effects upon the organ of Corti of long exposure to tones are K. Wittmaack, Ueber Schädigung des Gehörs durch Schalleinwirkung, *Z. Ohrenheilk.*, 54, 1907, 37-80; Zur Frage der Schädigung des Gehörsorgan durch Schalleinwirkung, *ibid.*, 59, 1909, 211-220; U. Yoshii, Experimentelle Untersuchungen über die Schädigung

g des Gehörsorgan durch Schallwirkung, *ibid.*, 58, 1909, 201-2. The final verdict has not, however, supported Yoshii, for stimulation of the cochlea to give the efficacy of loss desired for the resonance theory. On this point, see H. Kemp, A critical review of experiments on the problem of stimulation of deafness, *Psychol. Bull.*, 32, 1925, 325-342.

For the general summaries, see Waetzmann and Wilkinson and Ray, *op. cit.*, see H. Hartridge, vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 11, 1921, 277-288; 12, 1921, 142-146; 12, 1922, 362-382; 13, 1922, 48-51, 185-194.

Other Theories

The references for the place-resonance theory are given in the preceding section. Below are the citations indicated in the text for the other theories.

Pattern-resonance (pressure-pattern) theories: A. D. Waller, *Introduction to Human Physiology*, 1891, 458-462, esp. 461 f., or 2 ed., 1893, 464-469, or 3 ed., 1896, 470-475; J. R. Ewald, Zur Physiologie des Labyrinths: eine neue Hörtheorie, *Arch. ges. Physiol.*, 76, 1899, 147-188; *idem*: die Erzeugung von Schallbildern in der camera acustica, *ibid.*, 93, 1903, 485-500.

Frequency-non-resonance (telephone and leather chair-seat) theories: W. Rutherford, The sense of hearing, *J. Anat. Physiol.*, 21, 1886, 166-168; M. Meyer, Ueber Kombinationstöne und einige hierzu in Beziehung stehende akustische Erscheinungen, *Z. Psychol.*, 11, 1896, 177-229; then there are five articles in *ibid.*, 16, 1897-1898, 1-34, 352-372; 17, 1898, 1-14, 401-421; 18, 1898, 274-293; but the most complete and final exposition is An introduction to the mechanics of the

inner ear, *Univ. Missouri Studies, Sci. Ser.*, 2, no. 1, 1907; E. ter Kuile, Die Uebertragung der Energie von der Grundmembran auf die Haarzellen, *Arch. ges. Physiol.*, 79, 1900, 146-157; Die richtige Bewegungsform der Membrana basilaris, *ibid.*, 79, 1900, 484-509, esp. 499-509 (theory); E. G. Boring, Auditory theory with special reference to intensity, volume and localization, *Amer. J. Psychol.*, 37, 1926, 157-188, esp. 176-183.

Place-non-resonance theories: C. H. Hurst, A new theory of hearing, *Trans. Liverpool Biol. Soc.*, 9, 1895, 321-353; H. J. Watt, Psychological analysis and theory of hearing, *Brit. J. Psychol.*, 7, 1914, 1-43, esp. 27-43.

Frequency-resonance theories: Hensen, *op. cit.* in first section of these notes; W. Wundt, *Grundzüge der physiologischen Psychologie*, 4 ed., I, 1893, 478-480; 5 ed., II, 1902, 131-133, 137 f.; 6 ed., II, 1910, 136-138, 143 f.; H. Ebbinghaus, *Grundzüge der Psychologie*, 1902, 313-329, L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. general Psychol.*, 2, 1929, 28-58, esp. 36-47.

Duplicity theory: E. R. Jaensch, Die Natur der menschlichen Sprachlaute, *Z. Sinnesphysiol.*, 47, 1913, 219-290, esp. 256-269.

A fairly good secondary source for eleven of these theories (Helmholtz, Gray, Rutherford, Lipps, Hurst, Meyer, Ewald, ter Kuile, Lehmann, Göbel, Watt) is H. J. Watt, *The Psychology of Sound*, 1917, 139-162 for the others, 162-175 for Watt, somewhat altered since 1914.

Recent Physiology of Hearing

In general on the contents of this section, see E. G. Wever, The physiology of hearing: the nature of response in the cochlea, *Physiol.*

Rev., 13, 1933, 400-425 (bibliography of 33 titles); H. Davis, The physiological phenomena of audition, *Handbook of General Experimental Psychology*, 1934, 962-986 (bibliography of 39 titles); S. S. Stevens and H. Davis, Psychophysiological acoustics: pitch and loudness, *J. acoust. Soc. Amer.*, 8, 1936, 1-13; Stevens and Davis, *Hearing*, 1938, 268-436 (total bibliography of the book is 334 titles).

The first electrical measurement of the response of the auditory mechanism (were they neural action potentials or only cochlear microphonics?) is F. J. J. Buijten-dijk, On the negative variation of the nervus acusticus caused by a sound, *Akad. Wettensch. Amsterdam, Proc. Sect. Sci.*, 13, 1910, 649-652. The study of synchronism of clicks (watchman's rattle) with potentials in the medulla is A. Forbes, R. H. Miller and J. O'Connor, Electric responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 80, 1927, 363-380.

The initial papers of E. G. Wever and C. W. Bray are Action currents in the auditory nerve in response to acoustical stimulation, *Proc. nat. Acad. Sci.*, 16, 1930, 344-350 (preliminary); The nature of the acoustic response: the relation between sound frequency and frequency of impulses in the auditory nerve, *J. exp. Psychol.*, 13, 1930, 373-387 (main experiment); Present possibilities of auditory theory, *Psychol. Rev.*, 37, 1930, 365-380 (volley theory). The volley theory may be said to have been anticipated by Forbes and Gregg, *loc. cit.*, 1915 ("platoon fire") and by Troland, *op. cit.*, 1929, 24 f. The name *volley theory* emphasizes the simultaneous discharge of many fibers, whereas the term *platoon fire* asserts the discharges of successive volleys by different sets of fibers and would seem, for that reason, to be the more descriptive term. It seems

fair to remark, in the obscurity of these notes, that both the experiment of Forbes, Miller and O'Connor, *op. cit.*, 1927, and these original experiments of Wever and Bray were stimulated by Boring's paper, *op. cit.*, 1926.

E. D. Adrian's first suggestion of the cochlear microphonics is in his abstract of a paper, The microphonic action of the cochlea: an interpretation of Wever and Bray's experiments, *J. Physiol.*, 71, 1931, xxviii f. His change of mind is manifest in Adrian, D. W. Bronk and G. Phillips, The nervous origin of the Wever and Bray effect, *ibid.*, 73, 1931, 2P f.

The isolation of the cochlear response from the auditory action potentials by Davis and his associates appears in the following papers: H. Davis and L. J. Saul, Action currents in the auditory tracts of the midbrain of the cat, *Science*, 74, 1931, 205 f. (brief mention); Saul and Davis, Electrical phenomena of the auditory mechanism, *Trans. Amer. otol. Soc.*, 22, 1932, 137-145 (brief but crucial); Davis, A. J. Derbyshire, M. H. Lurie and Saul, The electric response of the cochlea, *Amer. J. Physiol.*, 107, 1934, 311-332 (full account of the properties of the cochlear response); Davis, The electrical phenomena of the cochlea and the auditory nerve, *J. acoust. Soc. Amer.*, 6, 1935, 205-215 (good summary). In general see S. S. Stevens and H. Davis, *Hearing*, 1938, 310-355.

On action potentials in the auditory nerve fibers, see Wever and Bray, *op. cit.*: Davis *et al.*, *op. cit.*; and esp. Saul and Davis, Action currents in the central nervous system: action currents of the auditory tracts, *Arch. Neurol. Psychiat. Chicago*, 28, 1932, 1104-1116 (gives seven characteristics of true action currents); Davis, Forbes and Derbyshire, The recovery period of auditory nerve and its significance

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Chapter 12

SMELL AND TASTE

THE experimental psychology of smell and taste has very little 'history.' A thoroughgoing modern handbook, like von Skramlik's, in giving all the facts gives also nearly all the history, for there has not been time and thought enough for many facts to go out of style or many theories to be proved wrong. The history of these 'chemical senses' must lie largely in the future. Why?

Why have the 'chemical senses' had no true scientific development? It is easy to say that not all fields can mature at once, that some must wait upon discovery in other fields and upon such chance insights as procreate successful research, that smell and taste are nowadays like sight and hearing in 1750, that there is a limited body of fact which interests a few men but no sure method for going further. Such an account, however, is a description, not an explanation, of the present state of these two fields.

One explanation is that these senses are too simple for elaborate description; lacking as they do the complex organization of sight and hearing with their analytical receptor systems, there is, therefore, little to find out about them. That theory, however, cannot stand. Smell, at least, shows such a great complexity of qualities that it has been likened to vision, and the olfactory membrane compares well in absolute sensitivity with the retina and the organ of Corti, even though its differential sensitivity is not so good. There is still plenty to find out.

Another theory is that smell and taste are so unimportant in human civilization as not to interest men. If human culture could have been founded on a dog's life, smell and not vision would be the great chapter of sensory psychology, and Helmholtz would have written three huge volumes of a *Handbuch des physiologischen Geruchs*, as well as a *Die Lehre von den Geschmacksempfindungen als physiologische Grundlage für die Theorie der Geschmackslehre*. To a certain extent this theory must be correct;

there can be no doubt that human interest excites intellectual activity, which in turn, by its very amount, begets the happy coincidences that are the insights which lead to scientific progress. Such an explanation, however, is not enough. No amount of strongly motivated intellection is going to provide the occasion for the correct insight, when the terms of the insight, to be drawn from seemingly unrelated fields, are not yet discovered. In any case this theory reduces to the next.

The real reason for the undeveloped state of the experimental psychology of smell and taste must be that their stimuli have not yet been discovered. Of their stimulus-objects we know, yes. Sugar tastes sweet, and violets smell—like violets. Yet what property is it that is common to sugar and all other sweet sapid substances? In respect of what physical or chemical attribute are all fragrant objects alike? And how does one fragrance vary from another? In other words, what are the *m.u.* of smell and the *c.p.s.* of taste? Until the stimulus-object has been analyzed so that its essential property is known, until the true stimulant is discovered, until, in short, we have a correct 'theory' of olfactory and gustatory quality, not until then shall we have the means to control and predict odors and tastes in new situations and thus to determine the psychophysical correlations which make the bulk of systematic knowledge in any department of sense. It has long been possible to write huge books about odors, but the material is casual and anecdotal without a generality to hold it together. Zwaardemaker spent a large part of his professional life looking for new physical principles that would give him the key to smell by an understanding of the true nature of its stimulus. He did not succeed, perhaps because that essential physical or chemical discovery—if physical or chemical it is to be—cannot be made until some other seemingly unrelated discovery has occurred or some apparently remote error of habitual thought has been corrected.

Smell

The amount that is written on a given subject is not an exact measure of how much there is to say. About smell a great deal has been written. Cloquet in 1821 and Zwaardemaker in 1895 contributed the important books for their periods, but it was the hand-

books that created a demand for systematic treatment of the subject. An editor of a general handbook of physiology could not very well leave out an entire sense. Let us, then, call the roll of chapters on olfaction in handbooks and special treatises, for, in so doing, we shall outline the history of this field and indicate where the knowledge within it can easily be found.

Albrecht von Haller in his *Elementa physiologiae* of 1763 devoted sixty-one pages to the chapter *Olfactus*. His treatment, casual, anecdotal, pathological, was not very systematic, although he made an attempt at classification. Charles Bell in 1803 had only four pages to write about smell, but Cloquet in 1821 achieved a book of 758 pages. He dealt, as systematically as one could in those days, with the classification of odors, the physiological seat of olfaction, its mechanism, its pathology, its practical uses and individual differences in sensitivity. For over seventy years Cloquet's book was the most frequently cited systematic source on this topic. Johannes Müller in 1838 could, nevertheless, find only seven pages of really solid fact on smell for his handbook, and Bidder contributed in 1844 only eleven pages to Wagner's *Handwörterbuch der Physiologie*. Von Vintschgau in Hermann's *Handbuch* in 1880, however, had sixty-two pages, because by that time there had been some experiments on smell and the receptor-cells had been described. It was shortly after, in 1887, that Zwaardemaker thought of the olfactometer and began his experiments, contributing so much fact and stimulating so many others that in 1895 he could write his classical *Physiologie des Geruchs* of 324 pages. In the same year Passy published his forty-eight-page review of the experiments on smell. For Schäfer's *Text-Book of Physiology* Haycraft wrote in 1900 a 113-page chapter, which included a report of some of his own researches. Briefer again, Nagel in his *Handbuch* in 1905 had only thirty-two pages, but in 1916 Henning brought his articles in the *Zeitschrift für Psychologie* together into a book of 533 pages, the largest since Cloquet. There were seventy-nine pages by G. H. Parker in 1922, and then in 1925, thirty years after the first treatise, Zwaardemaker published in French a revision and reaffirmation of his treatise, *L'odorat*. The next year von Skramlik issued his handbook with 345 pages on smell—now the standard source, since it recounts the opinions and experiments of others from the very beginning. The *Handbook of General Experimental*

Psychology in 1934 had sixteen pages by Crozier, and Woodworth's *Experimental Psychology* in 1938 nineteen pages. One hundred seventy-six years, seventeen treatments, 2717 pages.

'The organ of smell is the nose,' but at first it was not clear as to where in the nose the *olfactory receptors* lie. In 1862, during a period of very active histological research, Max Schultze, who had founded the *Archiv für mikroskopische Anatomie*, and who later distinguished between the cones and rods in the retina, localized the olfactory membrane high up in the nasal cavity. He found there in a small area the olfactory receptors, long cells with hair-like processes, a discovery which was confirmed and made more definite by von Brunn much later in 1892. Von Brunn described the olfactory cells and their supporting cells, fixing the area as little more than a centimeter across in one of the least accessible parts of the cavity.

Schultze's location of the organ so high in the cavity raised the question as to whether odoriferous air can reach it as readily and promptly as olfactory acuity requires. To answer this question, Paulsen in 1882 cut the head of a cadaver in half along the median plane, fixed little squares of litmus paper at different points in the nasal cavities, put the head together, and pumped ammonia through the nostrils into the trachea. When the blueness of the litmus paper showed that the main currents had passed well below the olfactory region, it became evident that smell must be initiated by small eddy currents or by diffusion. Franke, repeating this experiment in 1893 with half of a cadaver's head covered in the plane of section by a glass plate, drew smoke through the nose and confirmed Paulsen's conclusion. Zwaardemaker in 1895 got the same result with a horse's head and a plaster model of the nasal cavity. The effect of these researches was to emphasize the sensitivity of the olfactory organ.

Another of the early questions concerned the nature of the *stimulus*. Did it have to be a vapor or diffused particles, or might it be a liquid? E. H. Weber in 1847, studying the effect of cold and warmth upon nerve-conduction, had found not only that cold and warm odorous liquids cannot be smelled when poured into the nostrils of the inverted head, but that eau de Cologne and acetic acid are not sensed at body temperature. He concluded, therefore, that the temperature of these liquids is irrelevant, and that it is their liquidity which renders them ineffective as olfactory

stimuli. In 1886, however, Aronsohn, who had evidence to show that eau de Cologne in the nostrils irritates and thus perhaps anesthetizes the sensory cells, questioned this conclusion. He repeated Weber's experiment in a more tender manner, filling the nostrils first with normal saline solution, and then gradually introducing the olfactory stimulus. He got odor, but Haycraft in 1900 failed to confirm his result and was of the opinion that Aronsohn had trapped air in the inverted nostrils. Thus the decision went against liquids as direct stimuli.

Haycraft also decided that diffusion is not an adequate stimulus, that the air has to be in motion for the stimulating particles to penetrate to the olfactory membrane. If a person by continuous deep breathing renders himself apnoeic so that he does not have to breathe for two or three minutes, if a bottle of ammonia is then held for one or two minutes beneath his nose, and if then he pinches his nose, filled with ammonia vapor, and goes out of the room into the fresh air and takes a breath, then he smells the ammonia only when the air within the nostrils is set in motion by his inspiration.

It was Zwaardemaker, a cultivated and erudite Dutch physiologist, who really created an interest in the psychology of smell. His own concern about olfaction came out of his conviction that smell is like vision, and that the standard laws for vision—sensitivity, adaptation, mixture, compensation, analysis into specific energies—could be worked out for smell. As early as 1884, when he was acting as physician in a military hospital in Amsterdam, he “asked Nature which are our principal and most elementary smell sensations and . . . how do we become conscious of them? But Nature did not answer.” It seems that the reason for her silence at that time was that he lacked both an idea and a laboratory, but in 1887 at the College of Veterinary Medicine in Utrecht he had both. The idea was the principle of the simple *olfactometer* (Fig. 74). A glass tube, bent up at one end to enter the nostril; a piece of india-rubber tubing as stimulus to slip loosely over and along the other end; a small screen between the observer and the rubber; a scale on the tube to show how far the rubber projects beyond the glass and thus how much rubber is exposed for the air, sniffed by the nostril, to pass through. With such a device the threshold could be measured, and Zwaardemaker called the number of centimeters of exposed rubber necessary to produce a just noticeable odor an

olfactie, using it as a unit of intensive measurement. "That is beautiful," remarked Donders, Zwaardemaker's teacher, when he saw the olfactometer, "for it is simple." "It is also useful," Zwaarde-

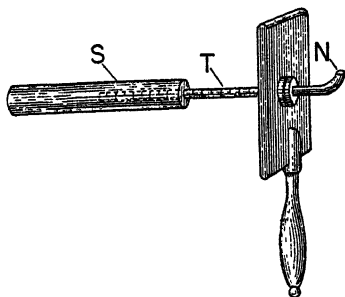


FIG. 74. SIMPLE OLFACTOMETER:
ZWAARDEMAKER (1888)

S is a tube of odoriferous substance, like india-rubber, which slides over the glass tube T. The scale on T measures the distance that S projects beyond the end of T; hence the distance that air, sniffed by a nostril at N, passes over the olfactory stimulus; hence the intensity of the stimulus. A double olfactometer consists of two simple olfactometers, one for each nostril.

maker replied, "for now we can mix odors, as you mix colors in your double-slit spectroscope, using odor equations instead of color equations." And mix odors Zwaardemaker did, determining the laws of compensation, in terms of olfacties, and the rates of adaptation.

The first determination of the absolute *threshold* for smell had been made, however, before Zwaardemaker's invention—in 1884 by Fischer and Penzoldt. By allowing, first, an odorous substance to diffuse in a standard chamber, weighing it carefully before and after diffusion and finding its loss in weight, and then determining the greatest dilution in air for which the odor could be sensed, they de-

termined the minimum perceptible concentration of the stimulus. It was in this research that the much quoted figure for mercaptan was found, a dilution of 1 in 50,000,000,000 parts being perceptible, or 1/400,000,000 mg. in 500 cc. of inspired air—a sensitivity which these investigators computed to be three times as great as the sensitivity of the spectroscope to sodium. Inasmuch as this method of dilution has been used by most of the investigators subsequent to Fischer and Penzoldt, the handbooks now give tables of thresholds for a considerable list of materials. With the true nature of olfactory stimulation still unknown, however, no general statement of the threshold in energetic terms has ever been possible as it has for vision and hearing.

The advantage of the olfactometer became obvious in Zwaardemaker's subsequent researches. The technique of his instrument he developed as *olfactometry*, and the measurement of intensity

as *odorimetry*, bringing the accumulating body of fact together in his book in 1895. Although he determined differential limens with the olfactometer, it was not until later that the question of the validity of Weber's law was definitely raised. Gamble in 1898 found the Weber fraction to vary between $\frac{1}{6}$ and $\frac{1}{2}$, with nearly two-thirds of the determinations lying between $\frac{1}{4}$ and $\frac{1}{3}$, an average ratio of about .32; and Hermanides, Zwaardemaker's pupil, in 1909 found equally great variability and an average ratio of about .38. Because neither of these investigators discovered great differences in the fraction at different intensities, they both concluded that Weber's law applies approximately to smells; but Zigler and Holway with an improved technique showed in 1935 that the fraction for india-rubber decreases steadily from the low intensities to the high, reaching a minimal ratio of about .10 at about 200 olfacties.

From the start it was clear that *adaptation* in smell is considerable and rapid; Zwaardemaker by use of the olfactometer was able to plot its curves. The most interesting thing about adaptation, however, turned out to be its mutuality. Aronsohn found in 1886 that adaptation to ammonium sulphide diminishes sensitivity to hydrogen sulphide and to chlorine and bromine vapors, and also prevents the perception of certain ethereal odors; that adaptation to camphor abolishes the perception of ether and eau de Cologne. In this discovery there seemed to lie the possibility of a classification of olfactory stimuli and an insight into the nature of the stimulus by a determination of the chemical community of equivalent stimuli, but nothing ever came of it.

Perhaps the most important result of Zwaardemaker's use of the olfactometer to find for smell the analogies of the laws of color mixture was his discovery of olfactory *compensation* in 1889. He was able then to designate various pairs of odors which act antagonistically—pairs which, in mixture, result in the perception of only the stronger component or even in mutual cancellation. Such antagonism he held, moreover, to be physiological: if one stimulus be presented to one nostril and the other to the other by use of a double olfactometer, there is still compensation. Ammonia and acetic acid tend thus to neutralize each other. They would, to be sure, neutralize each other chemically if allowed to react, but it was Zwaardemaker's claim that cancellation comes about likewise when each is presented to a different nostril. In this finding there

seemed to lie another hint for an understanding of the nature of the olfactory stimulus, but unfortunately the relationships are by no means simple or predictable. For instance, of the four substances, beeswax, paraffin, tolu-balsam and india-rubber, Zwaardemaker reported that every one of the six possible pairs except paraffin and tolu-balsam act antagonistically. When stimuli antagonistic to the same stimulus are still antagonistic to each other, all we learn about the nature of stimulation is that not so many different processes of smell can be excited at once. Hermanides, although verifying Zwaardemaker for both "unilateral" and "bilateral" stimulation, noted that cancellation is more often incomplete than complete, and that there can be alternating rivalry between the two odors.

In 1915-1916 Hans Henning adopted from Stumpf's terminology for tones (*monotic*, *diotic*, *dichotic*, p. 382) the terms *monorhinic* for smelling with a single nostril, *dirhinic* for smelling the same stimulus with both nostrils, and *dichorhinic* for smelling a different stimulus with each nostril. He contended that there is never any dichorhinic compensation, and that dirhinic or monorhinic compensation, when it occurs, is chemical. When Zwaardemaker, nevertheless, reaffirmed his position in 1925, the matter was left in the air, with the admission that complete compensation is rare and requires, at any rate, very careful adjustment of the stimuli.

The *classification* of odors on the basis of qualitative similarity is the most obvious and therefore the oldest form of their scientific consideration. The earliest classification which remains of importance today was made in 1752 by the great botanist, Linnaeus, who was forming a system for the description of the odors of plants. He made out seven classes: (1) aromatic, (2) fragrant, (3) ambrosiac, (4) alliaceous, (5) hircine, (6) foul, (7) nauseous. Haller, the physiologist, on the other hand, offered in 1763 but a three-fold classification: (1) sweet-smelling or ambrosiac odors, (2) intermediate odors, (3) stench. Besides these, there were also in the eighteenth century two classifications based on chemical characteristics of odoriferous substances, one by Lorry in 1784 and one by Fourcroy in 1798; and then there were at least five other little-known attempts in the nineteenth century before Zwaardemaker tackled the problem in 1895. Zwaardemaker ended by accepting the classification of Linnaeus and adding one class of Lorry's and another of Haller's to give nine:

1. Ethereal odors (Lorry): the fruity ethers of perfumes, beeswax, ether, etc.
2. Aromatic odors: camphor, spices, anise, citron, almond, etc.
3. Fragrant odors: flowers, vanilla, balsam, etc.
4. Ambrosiac odors: amber, musk, etc.
5. Alliaceous odors: onion, acetylene, iodine, etc.
6. Empyreumatic odors (Haller): roasted coffee, tobacco smoke, xylol, naphthalin, etc.
7. Hircine odors: goatly odors, cheese, sweat, chestnuts, etc.
8. Foul odors: narcotics, some bugs, coriander flower, etc.
9. Nauseous odors: carrion, carrion flower, feces, etc.

Actually there were in all these groups subclasses, for which Zwaardemaker gave long lists of examples. This classification, having the authority of the expert on smell, met with general acceptance up to the time when Henning invented the smell prism.

The smell prism was the result of an elaborate investigation of the qualitative similarity of odors by Henning in 1915. Having had his observers arrange great numbers of olfactory stimuli in many qualitative series, he sought to construct a superordinate continuum into which all the series would fit. The result was the prism of Fig. 75. In it there are six principal groups of odors. Intermediates between the groups are represented as lying on the edges or in the surfaces, but not in the inside. Though the principal groups are related to Zwaardemaker's, the instances differ considerably. These are Henning's principal classes:

1. *Ethereal* odors (*E*), corresponding to Zwaardemaker's ethereal group.
2. *Fragrant* odors (*F*), corresponding to Zwaardemaker's fragrant group.
3. *Spicy* odors (*S*), corresponding approximately to part of Zwaardemaker's aromatic group.

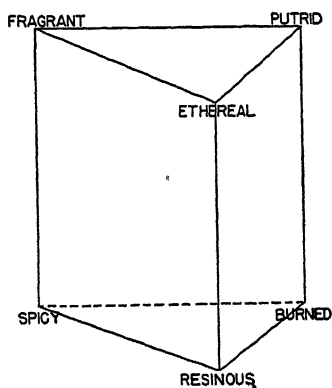


FIG. 75. SMELL PRISM: HENNING (1915)

The six principal qualitative classes of odors are shown. Intermediate qualities lie on the edges or in the surfaces.

4. *Resinous* odors (*R*), corresponding approximately to the other part of Zwaardemaker's aromatic group.
5. *Burned* odors (*B*), corresponding to Zwaardemaker's empyreumatic group.
6. *Putrid* odors (*P*), corresponding to Zwaardemaker's foul and nauseous groups.

Zwaardemaker's alliaceous odors fall for Henning in the middle of the *FPSB* face (Fig. 75). His hircine odors do not fit the prism. For instance, myrtle is hircine for Zwaardemaker, but *SR* for Henning. Balsam is resinous for Henning; it was fragrant for Zwaardemaker. The animal fetors lie on the *PB* line: Henning, missing examples for this part of the figure, found them in the

TABLE V. REPRESENTATIVE STIMULUS-OBJECTS FOR HENNING'S SMELL PRISM

<i>Simplex odors</i> (corners)	<i>Duplex odors</i> (edges)	<i>Quadruplex odors</i> (faces)
<i>F</i> = violet	<i>FE</i> = geranium	<i>FESR</i> = cedar
<i>E</i> = lemon	<i>FS</i> = thyme	<i>FPSB</i> = garlic
<i>S</i> = nutmeg	<i>ER</i> = pine	<i>EPRB</i> = grapefruit
<i>R</i> = balsam	<i>SR</i> = cinnamon	
<i>B</i> = tar	<i>FP</i> = carrion flowers	
<i>P</i> = hydrogen sulphide	<i>PB</i> = animal fetors	
	<i>SB</i> = roasted coffee	
	<i>EP</i> = almond	
	<i>RB</i> = incense	

Hamburg zoological gardens. *Arbor vitae* and its botanical relatives of the *thuya* genus lie in the middle of the *FESR* face. Shaddock (grapefruit?) is in the middle of the *EPRB* face. Common objects which represent the corners, edges and surfaces of the prism according to Henning are listed in Table V.

At least three experimentalists have undertaken to confirm the qualitative relationships implied by Henning's prism. All by different methods reached the same result: as a gross approximation the prism can be verified, but the verification fails as to detail. Apparently Henning never meant that it should be taken too exactly, for there are some difficulties inherent in the system itself. Must, for example, every *FR* odor be *ipso facto* an *ES* odor? Henning does not say so, but the prism implies it.

Although smell is always said to be one of the two chemical senses, there is no clear evidence that *chemistry* will eventually provide the knowledge of the essential nature of the olfactory

stimulus. The mere fact that different substances have different smells and also different chemical constitutions does not make of smell a chemical sense. Different substances have likewise different colors and different chemical constitutions, and yet color vision is not for this reason a chemical sense. The nature of the olfactory stimulus still escapes the psychologists; yet we may consider here three attempts to fix its chemical nature.

At the very beginning of this period of experimental interest in smell, Sir William Ramsay suggested (1882) that the adequacy of a stimulus for smell might depend on its molecular weight, that the olfactory threshold might lie at a molecular weight in the region of 30. He noted the marsh-gas series (C_nH_{2n+2}):

Methane, CH_4 , with molecular weight = 16, is odorless;
 Ethane, C_2H_6 , with molecular weight = 30, is odorless;
 Propane, C_3H_8 , with molecular weight = 44, has an odor.

Hydrocyanic acid (molecular weight = 27) is smelled by some and not by others; and Ramsay cited other instances as well to show that substances with light molecules tend to be odorless. His view received support in 1892 from Passy, who showed that the relative effectiveness of the various alcohols increases with their molecular weights, as indicated in Table VI. It was Ramsay's idea

TABLE VI. OLFATORY EFFECTIVENESS OF ALCOHOLS AS A FUNCTION OF MOLECULAR WEIGHT

<i>Substance</i>	<i>Formula</i>	<i>Molecular weight</i>	<i>Relative effectiveness</i>
Methyl alcohol	CH_3OH	32	1
Ethyl alcohol	C_2H_5OH	46	4
Propyl alcohol	C_3H_7OH	60	100
Butyl alcohol	C_4H_9OH	74	1000
Amyl alcohol	$C_5H_{11}OH$	88	10000

that smell, like vision and hearing, may be a vibratory sense, that the lighter molecules vibrate too rapidly—he set the rate for hydrogen at 44×10^{14} vibrations per second—to affect the receptor cells.

Haycraft, in 1887 and later, undertook to relate odor to Mendeléyev's Periodic Table of the chemical elements. That table was receiving considerable attention just then, for in 1886 the third of the unknown elements that Mendeléyev had predicted from his table in 1871 had just been discovered. Haycraft sought to use this new chemical system to order the smells. That chemicals related in the periodic series often have related qualities of odor he

was able to demonstrate. Thus sulphur, selenium and tellurium constitute a series of closely related elements, and their compounds tend to form an olfactory series of similar odors, with the odor of the selenium compound lying always between the odors for the sulphur and tellurium compounds. It is the same for the halogens (chlorine, bromine and iodine) and Haycraft was able to point to some similar organic series. The view, nevertheless, although it was suggestive, did not go far enough, partly because organic substances (compounds of hydrogen, carbon and oxygen) are the chief source of olfactory stimuli; hence the key to smell is not likely to be found in the Periodic Table.

For this reason Henning in 1916 undertook to fit an organic chemistry to his prism. He centered his theory upon the odors of the substitution-products of the benzene ring, which is a closed ring of six carbon atoms with one hydrogen atom attached on the outside to each carbon atom. Other groups of atoms can be substituted for a hydrogen atom; the character of the resultant compound depends, when there are two such substitutions, upon whether the substitutions occur at adjacent corners of the ring (ortho-substitution), corners next but one (meta-substitution), or opposite corners (para-substitution). Although such compounds may to a large extent account for *F*, *E*, *S* and *R*, Henning had to consider other elements for the *P* and *B* groups. Here is his theory for the six principal qualities:

Fragrant: ortho-disubstitution (at adjacent corners) on the benzene ring.

Spicy: para-disubstitution (at opposite corners) on the benzene ring.

Resinous: closed disubstitution on the ring, where a single substitution group is united with the ring at two corners.

Ethereal: monosubstitution at one corner on the ring of a forked group of atoms.

Burned: smooth heterocyclic ring which involves nitrogen at one corner instead of carbon.

Putrid: forked structure (no ring) involving elements in the Vth and VIth groups of the Periodic Table, like arsenic, bismuth, phosphorous, sulphur, selenium, tellurium.

About the *FESR*-face of the prism Henning was able to be logical. A trisubstitution product which is a combination of ortho-

substitution and para-substitution would lie, he said, on the *FS* edge; and similarly the other binary combinations on the other edges. As a test of the theory Henning called attention to the *FESR*-point near the middle of the *FESR*-face, a point where the thuya genus of evergreens (*arbor vitae*, cedar) belongs. The active principle of these odors is thuyol, and the structural formula for it is actually a combination of ortho-substitution and para-substitution at three corners of the ring, with a closed chain across two other corners and a fork to one of the substitution products. The theory is logically beautiful as it applies to the *FESR*-face of the prism; yet it has failed of general acceptance—presumably because there are many exceptions to it and many odorous substances that do not come under it. Psychology still lacks a chemistry of odor.

The handbooks used to carry discussions of the olfactory *specific nerve energies*. The complexities of color had been reduced to three elementary nervous processes by Helmholtz, or six by Her-
ing, just as, in like manner, the complexities of touch had in 1883–1884 come to be understood as combinations of four specific energies (pressure, warmth, cold, pain). If there are as many odors as colors—and some said there were—must not there be a similar reduction in smell? The classifications might have provided this analysis, but they did not. A chemical theory might have provided it, but there has never been a thorough-going chemical theory. The failure to make the analysis is simply a phase of the failure to make the crucial discovery about smell, to find the essential nature of its stimulus.

Taste

The experimental psychology of taste has even less history than the experimental psychology of smell. We can deal with it expeditiously.

If a physiologist discussed the senses, inevitably he had to discuss all of Aristotle's five; and of these taste is one. So Haller—with whom we may start in 1763—had his book on *Gustus*, in which he described the various forms of papillae. Inasmuch as anatomy was far ahead of physiology, his account was pretty good. That these papillae might be the organs of taste seemed reasonable enough, for the tongue can taste and they were the only special structures in sight. Furthermore, he made out a classification of tastes under

six heads with a discussion of the sapid objects characteristic of each class, concluding with the observation that substances, if they are to be tasted, must be soluble in saliva.

Johannes Müller, on the other hand, in 1838, was primarily interested in showing that taste corresponds to one of the five specific energies of sensory nerves, in other words, that mechanical and electrical stimulation, as well as sapid, can give rise to the taste quality. Because he knew about Horn's research in 1825 (*vide infra*), an experiment showing the different papillae to be differently sensitive, he could, with more assurance than Haller, assert that the papillae are the gustatory organs—or even with more assurance than Charles Bell, who had made the same statement in 1803. Müller also, although he noted that sapid substances must be soluble, hazarded the incorrect guess that some gases may be directly tasted. Remarking on the fusion of tastes and smells in flavors, he pointed out that the smells can be eliminated and the tastes left simply by stopping the nostrils. Repetition of gustatory stimulation, he suggested, renders the perception less distinct; nevertheless it is only recently that the long skepticism about gustatory adaptation has been finally dismissed by the discovery that it occurs quite rapidly.

Bidder's contribution on taste to Wagner's *Handwörterbuch* in 1846 did little to advance the knowledge of the subject, but von Vintschgau's to Hermann's *Handbuch* in 1880, introducing, as it did, a period of active research, marks the beginning of the belief that sweet, sour, saline and bitter are the four elementary qualities—a belief that was not fully accepted, however, for twenty years. Haycraft's contribution to Schäfer's *Text-Book* in 1900 was important, partly because of his chemical theory of taste; but Zwaardemaker's chapter in 1903 had in it nothing very new. The best modern discussions are those of Parker in 1922, von Skramlik in 1926, and Crozier in 1934. The field has not, however, been active in these last two decades. The classical experimental investigations were made by Oehrwall in 1891 and Kiesow in 1894–1896.

As we have seen, the papillae on the tongue were early selected as the *organs* of taste. That was Haller's opinion (1763), as it was Charles Bell's (1803) and Johannes Müller's (1838). The taste-buds were discovered in 1867 independently by Schwalbe and Lovén. Schwalbe was a pupil of the Max Schultze who described

the olfactory cells and the retinal rods and cones. He called the organs taste-beakers, presumably having in mind some kind of glass that curves in at the top like a modern brandy glass. Lovén called them taste onions or buds, for the spindle-shaped gustatory cells bulge out from the root and come together at the taste-pore, very much like the petals of a bud. These 'buds' they both found in the 'ditch' around the papilla, many in the wall of the papilla itself, some in the opposite wall. Though Schwalbe was uncertain how many buds there might be in a single papilla, his sketches show as many as ten at different depths in the side of a papilla big enough to allow for 42 buds around its circumference. That would make over 400 buds in the walls of the papilla with still more on the outside of the 'ditch.' Von Wyss in 1870 estimated the number of buds at about 400 per papilla, and Krause in 1876 thought there might be as many as 2500. An accurate count by Heiderich in 1906 showed 508 as a maximum, 33 as a minimum, with the average lying somewhere near 250. So there were plenty of receptors to go around among the qualities, which no one seems to have thought would be more than a dozen and which eventually turned out to be four.

Charles Bell in 1803 had strengthened belief in the papillae as the organs of taste; he demonstrated that, not only is the tongue insensitive to taste where there are no papillae, but gustatory sensibility is generally confined to the tip and edges of the tongue and is absent in the middle of its surface. Horn in 1825 took a further step in showing that different papillae are differently sensitive to various sapid substances, thus indicating the possibility of a physiological analysis of the many tastes into a few. Later, when the four elementary qualities were coming to be accepted, there were more careful investigations of the differences in sensitivity among papillae. Oehrwall in 1891, having discarded his saline solution as unsatisfactory, published the following inventory of the sensitivities of 125 papillae to his particular stimuli (sugar, tartaric acid and quinine at predetermined concentrations):

Sweet, sour and bitter	60	Sweet only	3	
Sweet and sour	12	Sour only	12	Total =
Sweet and bitter	4	Bitter only	0	125 papillae
Sour and bitter	7	None at all	27	,

He might, if he had used the base of the tongue, have found papillae sensitive to bitter alone. As it was, he left little doubt of the physiological distinctness of these three tastes.

That sensitivity to the four elementary tastes varies with the region of the tongue was Shore's contribution in 1892. Using glycer-

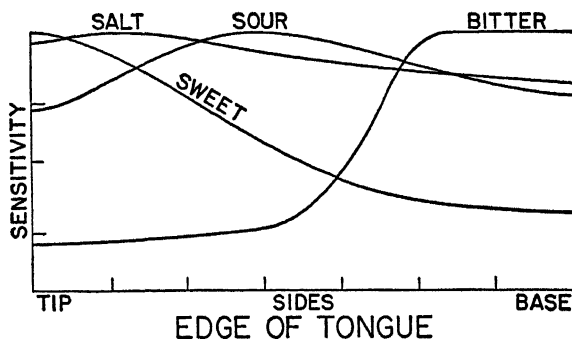


FIG. 76. DISTRIBUTION OF TASTE SENSITIVITY ALONG THE EDGE OF THE TONGUE: HÄNIG (1901)

Sensitivity is the reciprocal of the threshold value and is plotted as a ratio of maximal sensitivity = 1. Threshold data from Hänig. *Saline* and *sour* are least clearly differentiated. *Sweet* and *bitter* are antithetical, and clearly differentiated from each other and from *saline* and *sour*.

ine for sweet, sulphuric acid for sour, quinine for bitter and sodium chloride for saline, and determining thresholds for all, he reported relative sensitivity according to the differences shown in Table VII. Kiesow in 1894 verified Shore's generalization, and Hänig in

TABLE VII. RELATIVE GUSTATORY SENSITIVITY OF THE TONGUE:
SHORE (1892)

Region	Sweet	Sour	Bitter	Saline
Tip of tongue	high	low	low	medium
Edges of tongue	low	high	low	medium
Base of tongue	medium	low	high	medium
Dorsum of tongue	no sensitivity, even for strong solutions			

1901 worked out the quantitative functions which have been plotted in Fig. 76. These researches, showing the existence of four different patterns of sensitivity, indicated clearly that there must be at least four different physiological processes of taste. They did not, however, show that there are *only* four.

The question of the number of gustatory *qualities* first arose in

the classifications of the tastes. Bravo listed nine classes in 1592, Linnaeus eleven in 1751, and Haller, also in 1751, twelve, which he reduced to six in 1763. English equivalents of all their Latin adjectives are given in Table VIII. Of the sixteen qualities in that table, only five appear in all four classifications: *sweet*, *acid*, *bitter*, *saline* (the four that eventually survived) and *sharp*. All three men gave *insipid* as basic, but Haller dropped the class in 1763. It is hard to make some of the distinctions real, as between *insipid* and *aqueous*, *sharp* and *pungent*, *spirituous* and *aromatic*.

TABLE VIII. EARLY CLASSIFICATIONS OF TASTES

Bravo: 1592	Linnaeus: 1751	Haller: 1751
sweet	sweet	*sweet
—	—	*spirituous
—	—	aromatic
acid	acid	*acid
—	—	—
sharp	—	*sharp
pungent	—	pungent
harsh	—	harsh
—	—	—
fatty	viscous	—
bitter	fatty	—
insipid	bitter	*bitter
—	insipid	insipid
—	—	—
saline	aqueous	*saline
—	saline	urinous
—	—	putrid
—	nauseous	

* Haller's six classes in 1763

From multiplicity in the eighteenth century, opinion changed to parsimony in the nineteenth. Zenneck in 1839 and Valentin in 1848 held that there are only two qualities of taste: *sweet* and *bitter*. Physiologists were beginning to be aware that they must abstract the tastes from the tactual qualities. Thus Valentin referred *sour*, *saline*, *sharp*, *burning* and *cool* to touch. Stich then in 1857 was obliged to argue that *sour* is after all really a quality of taste because it cannot be got from all parts of the mouth cavity, which are nevertheless tactually sensitive. Similarly Schiff in 1867 held that *sour* cannot be tactual because acid too weak to be perceived as sour can nevertheless be felt. Still Duval in 1872 was arguing that only *sweet* and *bitter* are incontestable as qualities, that *acid*, *saline* and *alkaline* are dubious and probably due to touch.

Meanwhile the problem had become important because the tale

of the qualities was seen to be also the tale of the *specific nerve energies*. Von Vintschgau in 1880 decided definitely in favor of four specific nerve energies: *sweet, sour, bitter* and *saline*. This decision he made after his experimental determinations of gustatory thresholds had shown that sensitivity varies independently for these four tastes. He naturally chose the smallest number of elementary qualities possible, because the pressure of the theory of specific energies was toward parsimony. In spite of Helmholtz's extravagance with specific energies in his theory of hearing, the chief use of this principle was to show how the organism can get multiplicity of qualitative discriminations out of paucity of physiological processes. Wundt in 1880 and 1887 held to six qualities: *sweet, sour, bitter, saline, alkaline* and *metallic* (the last a new word for what had been called *astringent*). Oehrwall in 1891, however, supported the basic four, with the result that Wundt in 1893 and thereafter expressed doubt about the alkaline and metallic tastes. Oehrwall believed that he had definitely analyzed the alkaline, metallic and astringent tastes into fusions of taste and touch. *Inspidity* he concluded to be also a matter of touch, due to the absence of carbon dioxide. Water, he found, always tastes insipid when all the carbon dioxide is removed from it; and, conversely, insipidity is always removed from a solution by the addition of carbon dioxide. (Thus sodawater and carbonated drinks.) Kiesow, who also in 1894 held to these four basic qualities, nevertheless, in 1896, designated *insipidity* as a quality due to the mixture of sweet and salt—a *Mischempfindung* like orange from red and yellow. Although this 'discovery' of a fifth quality by Kiesow was never finally accepted, the researches of Oehrwall, Shore, Kiesow and Hänig in 1891–1901 on the differences in distribution of the four accepted basic qualities (*vide supra*) were enough to establish them—even though Sternberg did argue as late as 1898 for only *sweet* and *bitter* as basic. Wundt kept on in the editions of his *Physiologische Psychologie* saying that *alkaline* and *metallic* are doubtful, until Herlitzka in 1908 administered the *coup de grâce* to *metallic* by a special study in which he reduced it to a fusion of tastes, smells and touches.

Along with this problem of specific nerve energies, which had been under consideration since 1880, there had been coming up the question of *modalities*. Helmholtz had said that different qualities belong to different modalities when they have no intermediates.

Red and yellow are of the same modality because they are connected by the oranges, but color and tone are of different modalities. Does taste then consist of four modalities? is it four separate senses? Oehrwall was inclined to say *Yes*, but his view was never popular. Finally in 1916 Henning constructed the taste tetrahedron (Fig. 77) settling the problem to everyone's satisfaction. *Sweet*, *sour*, *bitter* and *saline* are, according to him, principal qualities that lie at the four corners of this figure. In between them on the edges lie intermediate qualities like the salty-bitter of potassium iodide, which is just as simple as an orange or a purple. These intermediates arise, he thought, not from mixtures of the stimuli for the principal tastes, but from simple substances whose chemical properties are intermediate between the chemical properties of the substances that lie at the corners of the figure (*vide infra*).

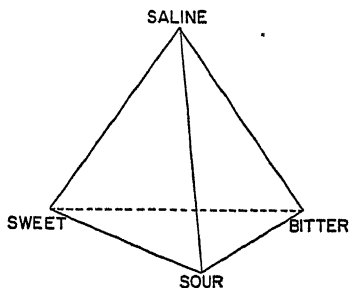


FIG. 77. TASTE TETRAHEDRON:
HENNING (1916)

The four principal qualities of taste are shown. Intermediate qualities lie on the edges or in the surfaces.

The *chemistry* of the sapid stimulus has never been successfully worked out for all the tastes. Haycraft in 1886 studied the subject in relation to Mendeléyev's table, as he did later for smell. His chief conclusion was that *saline*, for which NaCl (sodium chloride) is the representative stimulus, depends upon the combination of a metal (like Na) from group I of the periodic series with a halogen (like Cl) or a similar substance from group VII of the series. Fluorine and chlorine combined with any metal of group I taste saline, but the heavier halogens (bromine and iodine) combined with the same metals taste salty-bitter. So do the sulphates of these metals, compounds in which the SO_4 -radical acts like an element of group VI. It is also true that the chlorides of the metals of group II are salty-bitter. Haycraft's generalization was that any considerable chemical deviation from sodium chloride (to group II from I, or to VI from VII, or to heavier elements) introduces bitterness with the saline taste.

In 1884 Arrhenius published his important paper on the electrolytic theory of solutions, a crucial paper although slow of ac-

ceptance. By 1898, however, there were two men who saw that this new theory might contribute to an understanding of the gustatory stimulus, that attention must be paid to the ionization of solutions, since it is only solutions that stimulate taste. Richards pointed out that, since all sour acids have free hydrogen ions, the stimulus to *sour* must be the hydrogen ion. For inorganic acids (hydrochloric, sulphuric and nitric) he was able to show that sourness varies with the degree of ionization; but he was baffled by the fact that some organic acids (tartaric, citric and especially acetic), though much less ionized than the inorganic acids, yet taste more sour. Kahlenberg came to the same conclusion at the same time. His decision was that acetic acid is four times too sour for its degree of ionization. That it must be the hydrogen ion which is effective for sourness both these men were convinced, for the other component of the acid may occur in a salt solution without giving rise to the acid taste. Hydrochloric acid, for instance, provides H and Cl ions; sodium chloride provides Na and Cl ions. The former tastes sour, the latter saline. Hence it must be the H ions that taste sour.

This same logic would indicate that it is the Na in sodium chloride which tastes saline, since Cl is also in hydrochloric acid. Kahlenberg thought not, however, because sodium acetate, although ionized, is not salty. He believed that the H ions in HCl must be more effective than the Cl ions, which may still be the true stimulus to the saline taste.

Sternberg, who published in 1898, took up the problem so far avoided by the others: the stimulus for *sweet*. It was his conclusion that the lighter elements from the groups in the middle of the periodic table enter into sweet substances. Thus beryllium, bismuth, carbon, nitrogen, and oxygen—all light elements ranging from group II to group VI—are apt to make stimuli sweet, as does also lead, which is heavy but lies in the very middle of the series of groups. The stimuli for *bitter* Sternberg placed outside this zone at the extremes of the table, thus recognizing an antithetical relation between *sweet* and *bitter*.

Although Cohn in 1914, in an elaborate study of sapid substances, attempted especially to get at the nature of the stimuli for *sweet* and *bitter*, he met with little success. It was plain to him that the answer to this question would be found in organic chemistry and not in the periodic table. The alcohols and sugars are the natural

stimuli for *sweet*; the hydroxyl ion is sweet; but beyond that there is little generalization possible for the "glucogenes," as Cohn called the essential stimuli for *sweet*. *Bitter* comes from the alkaloids, he said, like quinine and especially strychnine. The NO_2 -radical is bitter if present in the molecule in sufficient amount: NO_2 alone is never bitter, $(\text{NO}_2)_2$ is sometimes bitter, $(\text{NO}_2)_3$ is always bitter.

Finally Henning in 1916, reviewing this literature and testing out chemical series for himself, was able to list certain elements and radicals that characterize the four corners of his taste tetrahedron, and to find some chemical intermediates which represent the intermediate qualities along the six edges. Here are some of his stimulating elements:

Salt: NaCl is the best.

Sour: H ions.

Sweet: Al , Fe , Be , Pb ; the radical OH (hydroxyl).

Bitter: two or more units per molecule of S or NO_2 .

Salt-bitter: K is more bitter than Na ; the series Cl , Br , I moves toward bitter; hence the series NaCl , KCl , NaBr , KBr , NaI , KI progresses from pure saline well on toward the bitter corner.

Salt-sour: sodium carbonate, ammonium chloride, the reasons for which are not clear.

Salt-sweet: potassium hydroxide, in which the K furnishes the salt and the OH the sweet.

Sour-sweet: CO.OH , in which the OH furnishes the sweet; lead acetate, in which the Pb furnishes the sweet.

Sour-bitter: calcium sulphate, in which the SO_4 is bitter.

Sweet-bitter: NO_2 or S , occurring only once in the molecule; CO (carbonyl).

It is plain from this account that the stimulus to taste is still unknown, except for such gross generalizations as that hydrogen ions taste sour, salts taste saline and may deviate toward bitter or sour, alcohols and sugars are sweet, alkalies are salty-sweet, alkaloids are bitter.

Although there have been in addition other investigations in the sense of taste, they do not add materially to the historical account. The experiments on mixture, compensation and contrast seem to have been inconclusive. *Adaptation*, suggested by Johannes Müller, was doubted, or was believed to take a very long time, until B. Mayer in 1927 showed that it is rapid and that recovery too is rapid. Since Mayer there have been several other studies. Von Vintschgau

and Hönigschmied in 1875–1876 published the classical results on gustatory *reaction times*, and these differences of times were used to differentiate the taste qualities. The times may, however, indicate, not the latent time of the receptor, but its degree of inaccessibility in the papillary ‘ditch.’ There were many determinations of the threshold in terms of the concentration of solution necessary to excite the taste, but no satisfactory determination of the *Weber function* until it was made by Holway and Hurvich in 1937. If only the nature of the gustatory stimulus were to become known, undoubtedly all these problems would receive a new lease of life.

Notes

Smell

The important treatises and handbook chapters cited in the text for smell are as follows: A. v. Haller, *Elementa physiologiae*, 1763, Bk. XIV, which is Vol. V, 125–185 of the Lausanne ed.; H. Cloquet, *Dissertation sur les odeurs, sur le sens et les organes de l'olfaction*, 1815, [n.v.], of which the very much enlarged 2 ed. (practically a new book) had a new title, *Osphrésiologie, ou traité des odeurs, du sens et des organes de l'olfaction*, 1821; F. Bidder, in R. Wagner's *Handwörterbuch der Physiologie*, II, 1844, 916–926; M. v. Vintschgau, in L. Hermann's *Handbuch der Physiologie*, III, ii, 1880, 225–286; J. Passy, *Revue générale sur les sensations olfactives*, *Année psychol.*, 2, 1895, 363–410; H. Zwaardemaker, *Die Physiologie des Geruchs*, 1895 (the classical experimental treatise); J. B. Haycraft, in E. A. Schäfer's *Text-Book of Physiology*, II, 1900, 1146–1258; W. Nagel, in his own *Handbuch der Physiologie*, III, 1905, 589–620; H. Henning, *Der Geruch*, 1916 (reprints four papers from *Z. Psychol.*, 74–76, 1915–16); G. H. Parker, *Smell, Taste and Allied Senses in the Vertebrates*, 1922, esp. 23–91 (bibliography of 80 titles); H. Zwaardemaker, *L'odorat*,

1925; E. von Skramlik, *Handbuch der Physiologie der niederen Sinne*, I, *Die Physiologie des Geruchs- und Geschmackssinnes*, 1926, 1–345 (the standard and fullest handbook source); W. J. Crozier, *Chemoreception, Handbook of General Experimental Psychology*, 1934, 987–1036 (bibliography of 139 titles); R. S. Woodworth, *Experimental Psychology*, 1938, 477–495.

On the location of the *olfactory membrane*, see M. Schultze, *Untersuchungen über den Bau der Nasenschleimhaut*, *Abhandl. naturforsch. Gesell. Halle*, 7, 1863, 1–100, esp. 70–78; A. von Brunn, *Beiträge zur mikroskopischen Anatomie der menschlichen Nasenhöhle*, *Arch. mikr. Anat.*, 39, 1892, 632–651.

On the *movement of the air* through the nasal cavities, see E. Paulsen, *Experimentelle Untersuchungen über die Strömung der Luft in der Nasenhöhle*, *Sitzungsber. Akad. Wiss. Wien*, 85 (8), 1882, 352–373; G. Franke, *Experimentelle Untersuchungen über Luftdruck, Luftbewegung und Luftwechsel in der Nase und ihren Nasenhöhle*, *Arch. Laryngol. Rhinol.*, 1, 1893, 230–249; Zwaardemaker, *Die Physiologie des Geruchs*, 1895, 49–51; Haycraft, *op. cit.*, 1900, 1256 f.

On the adequacy of *liquids* as olfactory stimuli, see E. H. Weber,

Ueber den Einfluss der Erwärmung und Erkältung der Nerven auf ihr Leistungsvermögen, *Arch. Anat. Physiol. Berlin*, 1847, 342-356, esp. 351-354; E. Aronsohn, Experimentelle Untersuchungen zur Physiologie des Geruchs, *Arch. Physiol. Leipzig*, 1886, 320-357, esp. 322-330; Haycraft, *loc. cit.*

For the history of Zwaardemaker's thought about smell and his invention of the olfactometer, see his An intellectual history of a physiologist with psychological aspirations, in *A History of Psychology in Autobiography*, I, 1930, 491-516, esp. 491-504. Altogether between 1887 and 1932 he published fifty papers on smell out of a total bibliography of 186 titles. All the early papers deal with the olfactometer, but the paper that marks its invention is *Die Bestimmung der Geruchsschärfe*, *Berliner klin. Wochenschr.*, 25, 1888, 950 f. On the olfactometer, see *Die Physiologie des Geruchs*, 78-136.

On sensitivity, see, for the method of dilution, E. Fischer and F. Penzoldt, Ueber die Empfindlichkeit des Geruchssinnes, *Sitzungsber. phys.-med. Soz. Erlangen*, 18, 1886, 7-10 (the paper was read in 1884). On differential sensitivity and Weber's law, see Zwaardemaker, *Die Physiologie des Geruchs*, 188-194; E. A. McC. Gamble, The applicability of Weber's law to smell, *Amer. J. Psychol.*, 10, 1898, 82-142; E. Toulouse and N. Vachide, Note sur un nouveau moyen de vérifier la loi de Weber-Fechner sur le rapport de la sensation à l'excitation et sur la vérification de cette loi par la mesure de l'odorat au moyen des solutions décimales, *C. R. Soc. Biol. Paris*, 51, 1899, 640-642; J. Hermanides, *Over de constanten der in de olfactometrie gebruikelijke negen standaardgeuren*, 1909, 56-59; M. J. Zigler and A. H. Holway, Differential sensitivity as determined by amount of olfactory substance, *J.*

general Psychol., 12, 1935, 372-382.

On adaptation and fatigue, see E. Aronsohn, *op. cit.*, 1886, 342-349; Zwaardemaker, *op. cit.*, 1895, 203-207; Hermanides, *op. cit.*, 1909, 14-21.

On compensation, see Zwaardemaker, Compensation von Gerüchen mittelst des Doppelreichtmessers, *Fortschr. Med. Berlin*, 7, 1889, 721-731, which is the 'discovery paper'; *Die Physiologie des Geruchs*, 165-174; *L'odorat*, 1925, 124-137; Hermanides, *op. cit.*, 60-100; H. Henning, *Der Geruch*, 1916, 98-158.

On classification of odors, see in general von Skramlik, *op. cit.*, 198-213, who gives 14 classifications from Linnaeus to Henning quite fully, with lists of substances that fall in the classes. For the classifications mentioned in the text, see K. von Linné (Linnaeus), *Amoenitates academicae*, III, 1756, the chapter *Odores medicamentorum*, [n.v., but I have seen what is apparently a 1764 reprint of the volume, and this chapter is pp. 183-201, and is itself dated 1752, which becomes therefore the date of the classification]; A. v. Haller, *Elementa physiologiae*, 1763, Vol. V, Bk. XIV, sect. ii, 5, *Classes odorum*, pp. 162-168; D. Lorry, *Observations sur les parties volatiles et odorantes*, *Hist. Soc. roy. Méd. Paris*, année 1784-1785 (published 1788), 306-318; A. F. de Fourcroy, *Mémoire sur l'esprit recteur de Boerrhaave, l'arome des chimistes français, ou le principe de l'odeur des végétaux*, *Ann. chim.*, 26, 1798, 232-250; Zwaardemaker, *op. cit.*, 1895, 207-238; Henning, *Der Geruch*, 1916, 51-98. The *odores tetri* of Linnaeus and Zwaardemaker are variously translated as *foul*, *putrid*, *repulsive* and *virulent*. Haller was not so rigid a classifier as Linnaeus: he named the pleasant group *odores ambrosiaci* and then shifted to calling them *odores suaveolentes*, and most

persons cite this second more appropriate term. His third group was simply *foetores*. The three experimentalists who verified Henning's prism in its gross relations, though not in detail, are M. K. Macdonald, Experimental study of Henning's system of olfactory qualities, *Amer. J. Psychol.*, 33, 1922, 535-553; A. E. Findley, Further studies of Henning's system of olfactory qualities, *ibid.*, 25, 1924, 436-445; F. L. Dimmick, Note on Henning's smell series, *ibid.*, 33, 1922, 423-425; The investigation of olfactory qualities, *Psychol. Rev.*, 34, 1927, 321-335.

On the *chemistry* of smell, see, in connection with molecular weights, W. Ramsay, On smell, *Nature*, 26, 1882, 187-189; J. Passy, L'odeur dans la série des alcools, *C. R. Soc. Biol. Paris*, 44, 1892, 447-449; in connection with the periodic table, J. B. Haycraft, The sense of smell, *Proc. roy. Soc. Edinb.*, 14, 1887, 207-219 (reprinted in *Brain*, 11, 1888, 166-178); in connection with organic compounds, Henning, *Der Geruch*, 1916, 281-305.

The handbooks say so much about smell as a *degenerate sense*—thus accounting for the simplicity of the organ and our lack of knowledge about its laws—that these notes should point out how E. B. Titchener took the opposite view, championing the sense in lectures and in textbook statements, as in his *A Beginner's Psychology*, 1915, 49-51, where he noted that smell "has more sensations, probably, than all the rest of our senses put together," and where he told how the Malays track game by scent, how man can regain an appreciation of this ground sense prone on the dining-room floor, and how Galton learned to do arithmetic by smell. In this connection it must be recalled that the threshold for smell is astonishingly low. Modern culture, on the other hand, puts a taboo upon the free use of smell in social relations. The

two parents at Cornell who tried to make their little son's olfactory life as rich as his visual and auditory were nevertheless embarrassed when he began to sniff at their guests.

Taste

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Chapter 13

TACTUAL SENSIBILITY

TO THE older physiologists touch, the fifth sense, presented a baffling problem. Even Aristotle recognized its difference from the other senses, for he assigned to it several sense-qualities—hard and soft, hot and cold, smooth and rough—making touch a complex sense or, as some have interpreted him, several senses. When E. H. Weber began the experimental investigation of touch as reported in his *De tactu* of 1834 and his *Der Tastsinn und das Gemeingefühl* of 1846, he left part of the field quite vague, for, although he was pretty specific about the *Tastsinn*, he left the *Gemeingefühl* as a classificatory catch-all for everything that did not fit the *Tastsinn*—for pain, tickle, shudder, shiver, itch, the muscular sensations, the vasomotor sensations, nausea, thirst and hunger. Nevertheless, even the *Tastsinn* lacked a convincing analysis until it yielded to the search for specific nerve energies by Blix and Goldscheider in 1882 to 1885, an advance which enabled von Frey at the end of the century to establish the modern doctrine of four qualities (specific energies): pressure, warmth, cold and pain. For a while it looked as if Henry Head with his theory of epicritic and protopathic sensations (1905 *et seq.*) was going to substitute a new picture of the 'truth,' but eventually von Frey prevailed.

Meanwhile the *Gemeingefühl* itself was becoming definitized by the analysis of some of its parts. For instance, Mach, Breuer and Crum Brown in the 1870's put the sense of rotation, the "ampullar sense" of the semicircular canals, on a satisfactory foundation; and Goldscheider in 1889 analyzed the kinesthetic sensations into the conventional triad of muscular, tendinous and articular. Von Frey in 1894 was most instrumental in getting pain out of the *Gemeingefühl*. The other organic sensations, like hunger, thirst and nausea, simply turned into perceptual patterns of pressures and pains in accordance with the conventions of introspective psychology after 1910, or else persisted as functional terms. Cannon's discovery that the stimulus for hunger pangs is the rhythmic contractions of the

stomach (1912) did much to remove the mystery from this visceral sense-datum. The *Gemeingefühl* was, however, never abolished; it merely disappeared.

Underlying all the history of research on somesthetic sensibility before 1920 lay the felt need for analysis. Investigators wanted to know how many senses, or sensations, or specific energies, or qualities there are in the body. Because different criteria of differentiation were used without being distinguished one from the other, the literature is greatly confused; but we ourselves, with the perspective of the present, can easily be clear: there were four ways in which one sense (or sensation) was distinguished from another, and they are as follows:

(1) *Physiological*, as when *articular sensation* was given its special name because the joints were found to be sensitive, or the *ampullar sense* because the semicircular canals are sensitive, or *epicritic* and *protopathic sensibility* because they are differently distributed afferent systems, or *pressure* and *cold* because they are aroused by different spots on the skin.

(2) *Functional*, as when *temperature* was called a sense because the organism can appreciate differences of heat, or *hunger* because the organism knows when it needs food, or *kinesthesia* because the organism is aware of its own movement and position.

(3) *Qualitative*, as when *temperature* was divided into *warmth* and *cold* because those sensations are introspectively different, or *prick* was distinguished from *ache*, or *heat* from *warmth*, on introspective grounds.

(4) *Perceptual*, as when *wet* is distinguished from *dry*, or *hunger* from *nausea*, because they constitute different patterns of simpler qualities.

These criteria constantly cut across one another. According to the doctrine of specific nerve energies, a qualitative difference always means a physiological difference, a difference in the specific fibers excited. Later the theory of evolution made men look for a functional specificity whenever they found a separate neural system, since a system to have evolved must have a use, and—conversely but less logically—to look for a new system when they found a new function. In the present century it has become clear that different functions may be carried by different patterns of the same qualities; nevertheless Gestalt psychology has in general refused to sanction such analyses, tending thus to identify quality

with function. All these differences we must keep in mind as we trace the history.

Tactual Differentiation

Since Aristotle had left the question of the complexity of touch unsettled, the ancients and their successors tended to make lists of tactual sense-qualities. All told, there was considerable agreement among these lists. That touch is adequate to the perception of light and heavy, hot and cold, wet and dry, nearly all the classifiers agreed. Some added tickle or thrill; others pain; still others the antithetical pair, pleasure-pain. In addition, they mentioned the deep-seated qualities that Weber put in the *Gemeingefühl*—hunger and thirst, which almost no one forgot, and other sense-qualities like shudder, suffocation, and sexual and muscular sensibility. These early classifications are not important, however, except as they gave notice of the existence of a problem. We may pass at once to the much more recent Weber.

In 1834 Ernst Heinrich Weber (1795–1878) published a Latin monograph on the pulse, absorption, hearing and touch, which was for the most part a discussion of experiments upon touch: *De subtilitate tactus*. He divided his exposition into *De loco*, *De pondere* and *De calore*—place, weight and heat. This work exerted little influence, but in 1846 he contributed to Wagner's *Handwörterbuch der Physiologie* his soon-to-be-famous *Der Tastsinn und das Gemeingefühl*, which echoed the original work in subdividing the *Tastsinn* into an *Ortsinn*, a *Drucksinn* and a *Temperatursinn*. In this analysis Weber was not using the word *Sinn* in a technical physiological manner; he was merely listing the functional capacities of the skin for sensory discrimination, saying *Sinn* as we might speak of a sense of proportion or a sense of decorum. By the *Ortsinn* he meant the capacity of the skin to distinguish the localizations of the two points in the compass test; for he intended in this analysis to show only that touches on the skin can be distinguished as to their pressures, their temperatures and their positions. Since such a statement is nothing more than saying that colors can be discriminated in respect of saturation, brightness and extension, it is plain that Weber was distinguishing three attributes, rather than three separate senses, although the formal problem of attributes did not exist in those days. Actually Weber was at pains

not to separate these functions, but to show that they go together and that they are interdependent. He appealed to their concomitances, pointing out that all three are missing together in the tissue of wounds and in the alimentary canal. By his often-cited experiment of the silver *Thaler*, he demonstrated that temperature and pressure are interrelated. (A *Thaler* is a German coin intermediate in size between the American dollar and half-dollar.) A cold coin, placed upon the forehead of a recumbent subject, feels heavier than a warm coin; in fact, it feels heavier than two warm coins, when one warm coin is stacked upon the other. If temperature affects perceived pressure, then the 'senses,' Weber argued, cannot be separate. Nor can the 'place sense' be regarded as independent, since there cannot be a localization unless there is something to localize.

In the next thirty-five years only slowly did opinion shift toward the belief that temperature and pressure are independent senses. Wunderli in 1860 argued for identity, noting that a tuft of cotton wool brought in contact with the skin and a warm object brought near the skin cannot be consistently distinguished. Szabadföldi too in 1865 showed that a warm object, although it seems lighter than a cold, also appears heavier than an object of neutral temperature. There were, on the other hand, at this time some findings that pressure and temperature do not always vary together in pathological conditions, and the histologists, using their new techniques to hunt for end-organs in the skin, began to find enough organs to make it seem that the skin might have more than one sense resident in it. Thus, after Funke and Hering in 1880 agreed that the pressure sense and the temperature sense are really separate, Hering undertook to build a theory of the temperature sense analogous to his theory of color vision. Phenomenologist that he was, he noted that cold and warmth are qualitatively different (like blue and yellow), that, since adaptation to one means sensitization to the other, they seem to represent antagonistic processes, that they are separated by a physiological zero (*Nullpunkttemperatur*) which varies with the adaptation, and that an object of medium temperature feels warm after cold stimulation, cold after warm. This last fact John Locke had remarked, but Hering's theory complicated the tactual sense further by making warmth and cold dependent upon different, though interrelated, physiological processes.

Meanwhile pain had, for the most part, been left in the *Gemeingefühl*. Such a classification was consistent with the view of pain as a form of common sensibility and with the belief that all intense stimuli are painful. Certainly very bright lights, very loud sounds, very strong pressures, great heat and great cold are all painful. Weber had even worked out the thermal thresholds for pain, for it was obvious to him that the *Gemeingefühl* is not excluded from the skin. Johannes Müller's theory of specific nerve energies was, however, opposed to this intensive theory of pain, since on that theory every nerve must have its own and only its own quality. He had been able to cite evidence to show that direct surgical stimulation of the optic nerve yields a flash of light but no pain. In 1858, moreover, Schiff came out against the intensive theory on the ground of experiments and the study of many abnormal cases, which led him to localize pain and touch separately in the spinal cord. Incisions in the gray matter of the cord sometimes interfered, he found, with the conduction of pain but not of touch, whereas division of some of the tracts in the white matter interfered with touch but not pain. Nevertheless, Erb in 1874 declared for the intensive theory of pain, although Funke in 1880 made the opposite decision. Herzen in 1885 noted that, when a cutaneous nerve is anesthetized by pressure block, sensibility to cold and touch is lost first, then pain, then heat, and that the return is in the reverse order after the block is removed. Altogether there was, when Blix began his experiments on cutaneous specific nerve energies in 1882, no certainty about pain as an independent sense; the separation of pressure and temperature, however, though not of warmth and cold, was generally accepted.

The independent discovery of sensory spots in the 1880's, first by Blix in Sweden (1882), then by Goldscheider in Germany (1884), then by Donaldson in America (1885), came about because this next step was so obvious. Helmholtz had made the doctrine of specific nerve energies basic to sensory psychology. That meant that the number of cutaneous qualities must be the number of kinds of nerves to be found in the skin. In investigation small stimuli were called for. They had not been used before except in Weber's experiments on localization. Blix, trying faradic stimulation of the skin (a single electrode from an induction coil), found separate spots that responded independently with the qualities of pressure, cold, warmth and pain. That seemed to indicate four

specific energies. Then Blix rigged up an apparatus with a hollow conical metal point through which warm or cold water could be passed, and used this stimulus to map the cold spots and the warm spots. When he got them in different places, the cold more numerous than the warm, he knew that he had definitely separated the temperature sense into two.

Goldscheider, who read the German translation of Blix's Swedish paper when he was about to publish his own, used cork points and needles for pressure, needles for pain, drops of ether from a brush or capillary tube for cold, and warmed conical brass cylinders for warmth. He mapped the skin for spots. What he found was separate warm spots, cold spots and pressure spots—all very much smaller and more numerous than those charted by any other investigators who followed him. He also, in his early work, found pain spots, very much more numerous and closer together than the other spots; but that discovery he took back later. He described the action of the pressure spots precisely. A light pressure with a cork point on a pressure spot gives a quality of "lively contact"; a heavier pressure gives "granular pressure" as if a grain were forced into the skin; a strong pressure, a "bruising, pressing pain." A needle on a pressure spot gives a "lively contact" for light pressure and an intense "neuralgic pain" for strong pressure. On a pain spot a needle gives a "fine, sticking pain" or, at greater pressure, a "lancing pain." In between the spots the cork points give dull contacts and the needles dull pricks. Goldscheider did not, therefore, claim that the skin is completely anesthetic between the spots, nor that pressure spots are insensitive to pain when the stimulus is strong.

Blix and Goldscheider may thus be said to have established pressure, cold and warmth as three separate qualities, or specific energies, as they were called then. The separation of the spots implied the existence of independent nerve endings beneath them. Both men also presented good evidence for pain as a fourth sensory system—evidence which, however, was less convincing because the 'spots' were so numerous and close together that it was practically impossible to find pressure, cold or warm spots which were analgesic. Both eventually, reversing their original positions, held that pain is common sensibility. Later, in the 1890's, when von Frey was arguing that pain is a fourth cutaneous modality, Goldscheider vigorously supported the opposing intensive theory. He showed,

among other things, that stimuli, separately adequate only to pressure, will, when given at the right frequency, summate to give pain. Thus the intensive theory of pain was sometimes called a summation theory. Goldscheider believed that pain has no separate receptors in the skin, but that pressure or thermal excitations may summate so as to excite, in addition to their proper tracts, the pain tract in the spinal cord, and thus, presumably, a pain center in the brain. This scheme makes of pain a *Mitempfindung*, although actually giving it a specific energy within the central nervous system.

What came later to be regarded as the classical theory of the skin was put forth by Max von Frey (1852-1932) in four papers in 1894-1896. Von Frey was clear, definite, assured, and, what is more, he offered a doctrine that was congenial to current belief in the specific energies of nerves. This in itself was an accomplishment. His contributions were as follows. (1) He made the argument for pain as a fourth separate modality in the skin (1894), demonstrating the existence of pain spots, much more numerous than the other three kinds of spots and not necessarily coincident with them. Between the spots he found regions analgesic to that intensity of the stimulus which was adequate to the spots. (2) He discovered paradoxical cold (1895), the response of a cold spot, with the cold quality, to a warm stimulus in the region of 45° to 50° C., that is to say, far above the threshold for warmth on the warm spots and a little below the threshold for thermal pain. Later Alrutz's doctrine of the heat quality was based upon this fact, but at the time the discovery served only to reenforce the theory of cutaneous specific energies and the separation of warmth and cold as two separate modalities. (3) He found for each of the four modalities a specific end-organ (1895): for pain the free endings between the epithelial cells, for cold the Krause bulbs just beneath the skin, for warmth the Ruffini endings deeper down, for pressure the free-endings around the hair-follicles or, in the palms and soles where there are no hairs, the Meissner corpuscles. This correlation, although it was not correct, was accepted for many years largely because of the weight of von Frey's authority. At the time it seemed to settle the question about four modalities: if there are four kinds of spots and each has its peculiar organ, what more can one ask? (4) Finally, he discovered (1896) that the nature of the stimulus to pressure is not simply the force of the stimulus, nor the force per unit area, but the tension, the force per linear unit of the

depressed area on the skin. On that finding he based his tension theory of cutaneous pressure. To the question of heat and to the significance of the pressure stimulus we must refer again in the proper places, but it is right that we should here see von Frey's important contribution as a whole.

Von Frey's argument about the cutaneous end-organs was mostly indirect. Let us see how he determined the organs for the four modalities.

(1) Most of his pressure spots, he found, lay over hair-follicles, that is to say, to 'windward' of the hair as it emerges from the skin. In one good correlation he found 73 spots for 70 hairs; in another poor one 70 spots for 96 hairs. Naturally he concluded that the free-endings at the follicle are the receptors. It must be observed, however, that all the spot-finders ignored those "dull, contentless contacts," vaguely sensed between the definite positive spots, and that Goldscheider, even when he excluded these intermediates from consideration, got in one case 66 spots for 36 hairs and in another (his worst correlation) 147 spots for 15 hairs. In the palms and the soles, where there are no hairs but where there are Meissner corpuscles, von Frey chose the latter as proper to pressure. They are very numerous and spatial discrimination is quite fine in these regions. Of other evidence he had none.

(2) Because no other endings are numerous enough to account for the great frequency of pain spots, von Frey assigned the free endings between the cells of the epidermis to pain. He was thus required to explain why pressure should be more easily elicited than pain when the organs for pain lie nearer the surface than those for pressure; but he met this objection by noting that the pressure organs, lying in the soft tissue underneath the hard epidermis, would be affected first through the epidermis which would be pressed down as a whole, whereas organs in between the hard epidermal cells would require a more violent stimulus to crush the cells or penetrate between them.

(3) The Krause bulbs he assigned to cold because they occur in the conjunctiva and the glans penis which are insensitive to cutaneous pressure, and in the outer edge of the cornea which is insensitive to warmth. Von Frey himself found one Krause bulb in ordinary skin, but there is no evidence that they are nearly so numerous as cold spots.

(4) Finally, he gave to warmth the Ruffini endings, which are

known to lie in the finger pulp and one of which he found in ordinary skin. Although the fact that they are located deeper in the skin than the Krause bulbs corresponds with the fact that the reaction time for warmth is longer than for cold, in general von Frey was doing little more here than delegating the last unassigned organ to the last available sense.

A strange thing about this theory of von Frey's is that it was formulated by him—and ultimately accepted into textbook psychology—without any reference to the earlier direct experiments of Goldscheider in 1886, which should have been known to von Frey. The fact is that both Donaldson in 1885 and then Goldscheider had excised cold spots and warm spots, yet had been unable by histological examination to locate any receptors but free endings. Inasmuch as these findings were confirmed later by Häggqvist (1913), Dallenbach (1927) and Pendleton (1928), the whole correlation is now discredited after many years of service.

Von Frey's division of cutaneous sensibility into four modalities was challenged only once—by Henry Head, the famous English neurologist, who came at the problem of differentiation from the clinical side. He had observed that, after an injury to a cutaneous nerve, there are generally, besides a region of complete anesthesia, boundary regions in which sensitivity to light pressure is abolished, sensitivity to pain is increased, localization is disturbed, and accurate spatial discrimination is lost. He also found that the return of sensibility to the anesthetic skin, when an injured nerve is regenerating, passes through this same state of crude, primitive sensory response, a state which he later named *protopathic* sensibility. Since clinical patients make poor observers and disappear from experiments as soon as they are nearly well, he decided to substitute the experimental for the clinical method. Severing a nerve in his own arm, he studied the state of cutaneous sensibility after the section and during the regeneration of the nerve. With him were associated Sherren, the surgeon, and Rivers, the psychologist. They published their first report in 1905 and a full account in 1908.

Head's theory—it is impossible at first to separate the facts from the theory—was that three differently distributed neural systems are involved in the normal stimulation of the body's surface. (1) *Deep* sensibility lies in the tissues immediately underneath the skin, is not disturbed by the section of a cutaneous nerve, and func-

tions surprisingly well through the skin for dull pressure and dull pain. Not only will a couple of grams pressure on the skin arouse deep sensibility, but the capacity to localize a single point is about as great for deep sensibility alone as when cutaneous sensibility is added. (2) *Protopathic* sensibility is a primitive cutaneous system, crude and strong in widely dispersed action, overlapping the regions supplied by adjacent nerves, and first to return when a severed nerve regenerates. (3) *Epicritic* sensibility, rarely found alone, is superimposed upon protopathic sensibility in normal sen-

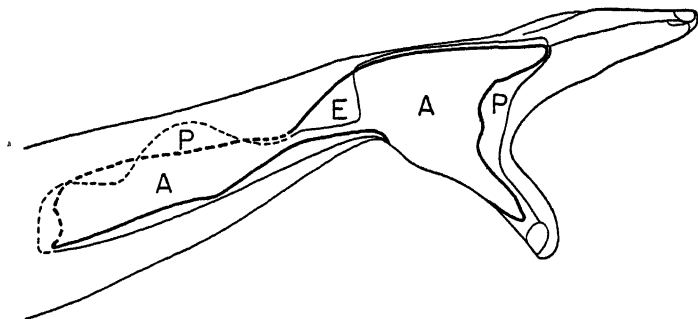


FIG. 78. PROTOPATHIC AND EPICRITIC SENSIBILITY IN HENRY HEAD'S ARM IN MAY, 1903

One month after the radial and external cutaneous nerves had been divided. A = anesthesia; P = protopathic sensibility; E = epicritic sensibility; Normal sensibility, P + E lies outside the enclosed areas. Deep sensibility was not disturbed. The light line is inner boundary of epicritic sensibility, the heavy line the inner boundary of protopathic sensibility. Cf. Table IX.

sibility and is a later and more finely discriminatory capacity, mediating the qualities of light pressure and the lesser degrees of warmth and cold but not pain, inhibiting the more violent reactions of the protopathic system, and returning in regeneration after the protopathic. Certain primitive surfaces, like the glans penis, show only protopathic sensibility. Ordinarily, after a nerve has been injured or severed, there results on the skin a central area of cutaneous anesthesia, where there is only deep sensibility, surrounded by an irregular region of protopathic sensibility, in turn surrounded by a normal region. As sensitivity recovers, both regions shrink, the protopathic returning centripetally in the anesthetic region, and normality—due presumably to the addition of epicritic sensibility to the protopathic—following centripetally. The boundaries are

irregular. Head claimed to have found on his own arm one area where epicritic sensibility existed without protopathic—a situation which, however, has not been duplicated in later experiments (Fig. 78, area *E.*). The list of functions for the two systems, much greater than this paragraph states, is given in Table IX. The distribution of sensitivity in Head's arm one month after the section of his nerves is shown in Fig. 78.

TABLE IX. HEAD'S THREE SYSTEMS OF TACTUAL SENSIBILITY (1905)

Protopathic and epicritic sensibility are cutaneous; deep sensibility is subcutaneous.

<i>Deep</i>	<i>Protopathic</i>	<i>Epicritic</i>
1. Dull pressure.	1. Hair sensibility, a tingling or formication from stimulating the hairs.	1. Recognition of light touch over hairless skin or shaved skin.
2. Dull pain from heavy stimuli.	2. Pain, by pricking, burning, freezing, electric stimulation, or plucking hairs.	2. No pain of any kind.
	3. Heat from temperatures above about 45°C.	3. Discrimination of warmth for temperatures up to about 40°C.
	4. Cold from temperatures below about 20°C.	4. Discrimination of cold for temperatures down to about 25°C.
	5. No thermal adaptation.	5. Thermal adaptation.
3. Localization of single point accurate to ½ inch	6. Localization perverted; sensations radiate widely, are very diffuse or are remotely referred.	6. Cutaneous localization accurate.
	7. All thresholds high.	7. Discrimination of two points as two.
	8. All sensations intense.	8. Partially inhibits over-reaction (both intensity and bad localization) of protopathic system.
	9. All sensations disagreeable.	
	10. Sensory spots have punctiform distribution in skin.	9. Warmth and cold not punctiformly distributed in skin, and pressure punctiform only when associated with hairs.

A careful examination of Table IX raises about Head's theory certain questions that cannot be answered. Protopathic "hair sensibility" seems to be a kind of pressure sense, although the theory asserts that the pressure quality is only epicritic. It is not clear how

in area *E* of Head's arm (Fig. 78) there could have been sensibility to mild temperatures and insensibility to extreme temperatures. If a point can be localized in deep sensibility to half an inch, cannot two points, an inch and a half apart, be discriminated as two without epicritic sensibility? (A later experiment showed that they can.) Yet such matters are trivial. In the main Head's discovery proved correct: there is a genetic course of returning sensibility after nerve injury. The extreme stimuli, thermal and algesic, are effective first, when sensation is intense, unpleasant and badly localized; mild stimuli become effective later as sensitivity improves, as intensity and unpleasantness diminish, and as localization becomes accurate. So far as these facts go, there need never have been controversy about Head's theory.

There was, however, controversy. The theory was one put forth by a neurologist without regard to von Frey's theory of the four modalities already accepted by the psychologists. With warmth and cold each divided between the two systems, it cut across the conventional view. The textbook writers did not know which theory to accept, whether von Frey's four afferent systems or Head's two. Sometimes they printed both without synthesis.

In 1909 Trotter and Davies, two British surgeons, divided seven different nerves in one or the other of themselves and studied the return of sensibility during the period of regeneration. As far as the general statement of facts given above goes, they confirmed Head. Since, however, they did not find the different areas sharply bounded, they preferred not to accept the two-system classification. In 1916 the author of the present book published an account of the return of sensibility in his own arm after a small cutaneous nerve had been divided for experimental purposes. Undertaking this experiment for the purpose of resolving the seeming incompatibility between von Frey's and Head's views, he too confirmed the latter as to the general facts, but failed to get sharp boundaries between the regions, rejecting, on this account, the concepts of protopathic and epicritic sensibility as not useful for explaining the results. Head complained that this experiment, involving the section of too small a nerve, was therefore only a "miniature experiment"; yet there is nothing in Head's theory that ought not to be demonstrable within a single square centimeter. Although there have been other experiments since, the fact seems to be that Head's theory has gradually dropped out of the textbooks while von Frey's has stayed.

There has never been a generally accepted synthesis of the two views, but the basic facts, common to all experiments, appear to be admitted by everyone. All four modalities return during regeneration gradually and continuously; pain tends to be early and pressure late; and in the initial stages there is exaggerated sensory response which is later diminished (inhibited?). The present author suggested that the fact of diminished sensitivity around an anesthetic region after nerve-division shows that there is considerable overlap of nerve supply from different trunks, that most sensory spots are multiply innervated, and that a second fiber growing into a spot may have an inhibitory effect upon a first which is already functioning there—thus explaining what is perhaps Head's most important finding, the inhibitory effect of the 'epicritic' upon the 'protopathic.' Nobody, however, has taken this suggestion very seriously.

The result of the century of cutaneous psychology since Weber is, therefore, that we have settled down to a belief in four modalities, even though we cannot distinguish four kinds of nerve endings. Each of these senses is distributed in a punctiform manner in the skin; there must be receptors and the receptors, being nerve endings, cannot be continuous sensitive films. The spots are the points where stimulation is most effective. The more intense the stimulus, the larger the spots, because they can be activated at a greater distance by a strong stimulus. Although the order of numerosness of the spots tends, in general, to be pain, pressure, cold, warmth, no such statement can be exact, inasmuch as more effective stimuli create larger and fewer spots. In this sense von Frey's notion that a cold spot cannot lie within a warm spot is not correct; albeit we must remember that von Frey was thinking only of the clear strong sensations which come with their characteristic *Merkmale*, not of all the vague sensory ghosts that can be aroused in the less effective regions between the spots.

The Two-Point Limen

For Weber the *Ortsinn* was important because he had a method of measuring it and the measurements seemed to him to have direct physiological meaning. His device was, of course, the compass test, the determination of the distance apart on the skin at which two points will be felt as two. Such a method measured, he thought,

the separation of end-arborizations of the cutaneous nerves. This determination (1834, 1846) of what was called later the *two-point limen* or the *limen of dual impression* he supplemented in 1852 by his "second method" for measuring the perception of tactual space, the determination of the error of localization when the subject tries accurately to indicate the location of a single cutaneous point which has been touched. While this second method has been called

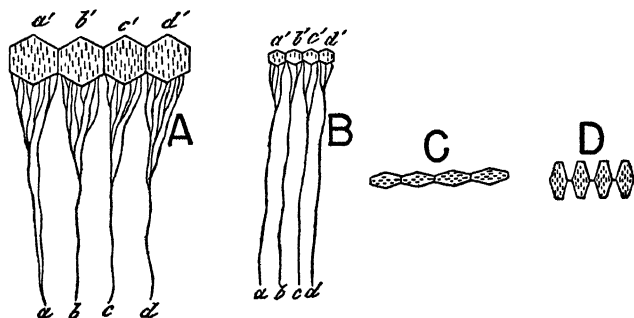


FIG. 79. WEBER'S SENSORY CIRCLES (1852)

Weber's pictures of sensory 'circles' as end-zones of nerve fibers in the skin. *a, b, c, d* = nerve fibers. *a', b', c', d'* = sensory circles. *A* = poor discrimination. *B* = good discrimination. *C, D* = transverse discrimination better than longitudinal, as in forearm.

localization, it is plain that the first method, in a more precise sense, is also a localization. Any localization has to be made with respect to some frame of reference; in the compass test one perceived point becomes the reference for the other, since the subject has to say whether the two are the same or different. Weber in 1846 made the limen of dual impression a measure of the *Ortsinn*, but later this limen was said by him and others to measure the *Raumsinn*, whereas the error of localization was then regarded as an index of the *Ortsinn*. From this early start, there grew up quite a large literature on each of these determinations, and in general the two measures were kept distinct—as they were by Head (1905), who assigned the capacity for localization to deep sensibility, but the power to discriminate two points to epicritic sensibility (Table IX).

It was Weber's belief that the skin is divided into *sensory circles*, a mosaic of small regions each of which represents the termination of a nerve fiber. Weber's picture of these 'circles' as hexagons is re-

produced in Fig. 79, which also shows how they might be shaped when the threshold of duality is greater in one direction than the other. The threshold, according to Weber, measures the average distance between two circles next but one to each other. If the two points of the compass fall in the same circle, one feels, he thought, a single impression; if they fall in adjacent circles, the impression is still single, though elongated; let them skip a circle, however, and then two separate points are perceived.

Although Weber discussed the *successive stimulation* of points, the first systematic investigation of this sort was made by Czermak in 1855. He got smaller thresholds than Weber's, but then one might expect such a difference; adjacent circles in successive stimulation, being discrete in time, would not fuse into a simple impression. Czermak was primarily concerned, however, with the question of whether the sensory circles actually correspond to different nerve endings, as Weber thought. First citing a current neurological investigation to show that the total number of nerve fibers does not increase as the organism grows larger, he then went on to demonstrate that the space threshold for boys is less than for men. The implication he drew was that, since the number of nerve fibers is constant, the sensory circles and the limen of duality grow with the organism.

Weber noted that longitudinal and transverse thresholds may be different in size (Fig. 79, *C* and *D*), and Vierordt in 1869 stressed this fact, presenting the ratios, which ranged from 1.0 to nearly 4.0 in various bodily regions. Discrimination across the arm is, for instance, much more accurate than discrimination up and down, a fact which Weber took as support for his simple physiological hypothesis.

On the other hand, Vierordt was able in 1869 and 1870 to formulate his law of mobility as a condition for the threshold. The threshold on the arm decreases continuously, he found, from the shoulder to the finger tip; in any single rigid member, like the upper arm or the forearm, the decrease is proportional to the distance from the next proximal joint, but the rate of decrease—different in different members—changes at each joint. Fig. 80 shows this relationship. For each member of a limb the threshold can be thought of as the sum of a constant, which is greater the more proximal the member, and a variable, which increases with the proximality of position within the member. The significance of Vierordt's law is that sen-

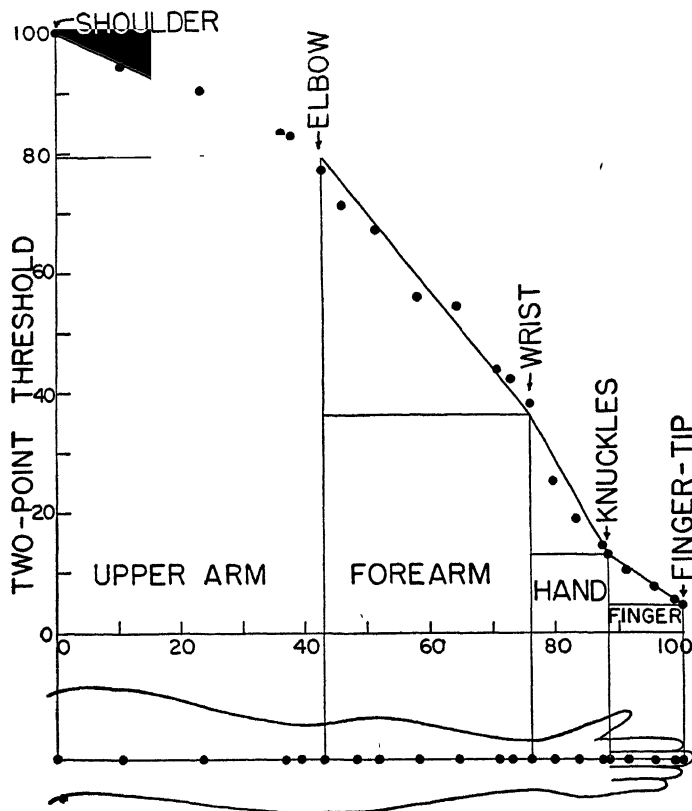


FIG. 80. VIERORDT'S LAW OF MOBILITY (1870)

The size of the two-point threshold (in per cent of the threshold at the shoulder) plotted against the position on the arm (in per cent of the distance from shoulder to finger tip). Averages of longitudinal and transverse thresholds, and of dorsal and volar thresholds. Plotted from Vierordt's data (and similar to a figure of Henri's). The threshold at any point can be regarded as the sum of a constant for the particular member (the rectangle in the diagram for the upper arm, forearm, hand or finger) and a variable proportional to the distance of the position from the nearest proximal joint (triangle above the rectangle). The solid lines are drawn to give the best approximation to the theory. Vierordt himself did not construct this figure.

sitivity increases with mobility—continuously toward the more mobile end of a member, and abruptly where a joint increases mobility. There is a suggestion in this law that the threshold may depend upon use and not merely upon the distribution of nerve-endings.

The discovery of pressure spots in 1882 immediately raised the question whether these spots correspond to Weber's sensory circles and whether every pair of spots can therefore be discriminated spatially. Goldscheider, who found many more spots than did his contemporaries, supported his figures in 1885 by finding also much smaller space thresholds than the others. Von Frey and Metzner in 1902, using successive stimulation as Czermak had done earlier for the same purpose, reported that, under optimal conditions of observation and optimal time between stimulations, they could always get a spatial discrimination of every pressure spot from every other. These results, therefore, favored Weber's original physiological theory.

The situation was, however, complex. Henri, in his classical *Ueber die Raumwahrnehmungen des Tastsinnes* in 1898, was able to add to the simple anatomical basis for the thresholds all the evidence for the effect of practice and fatigue. It was difficult at that time to say whether the threshold was dependent more upon innate anatomy or upon learning.

The complexity of the physiological basis of the threshold was also made apparent by the discovery of *Vexirfehler* or paradoxical judgments. In the earlier psychophysics it was usual to include control stimulations called *Vexirversuche*: in the compass test, for instance, single stimulations were mixed in with the double as a check on the objectivity of the observer and with the expectation that a good observer would never call a single point "two." Kottenkamp and Ullrich, pupils of Vierordt, found, however, in 1870 that a single point was often reported as two. Camerer, applying the method of right and wrong cases in this field in 1883, discovered that these *Vexirfehler* can be so numerous as to interfere with the computation of a limen. For instance, averaging the results of five observers, he found 15 per cent of the single points judged "two," and 10 per cent judged as "more than one." Henri and Tawney made a special study of these judgments in 1895. There are, according to their observations, differences in the frequency of the paradoxical *twos* in different persons as well as on different parts of the body for the same persons; in addition, the paradoxical *twos* are more often perceived as separated in the longitudinal direction on the forearm than in the transverse direction—all data which suggest that the basis for the judgment may lie in the anatomical distribution of the nerve endings. But they also found that expectation

and fatigue increase the number of *Vexirfehler*—facts which indicate a judgmental origin. The paradoxical *twos*, especially numerous for single points which are not mixed in with double stimulations, decrease when wide separations of the double stimuli are used along with the single points. Contrast, in other words, reduces the *Vexirfehler*. Henri summarized the whole matter in 1898 and Foucault again in 1910, but there was no decision on the issue of anatomy vs. judgment.

In 1918 Kincaid reported a case in which the frequencies of *twos* in the psychometric function were as follows:

2 mm.	8 mm.	14 mm.	20 mm.	26 mm.
57%	47%	65%	72%	94%

The *Vexirfehler* make the *twos* more numerous at 2 mm. than at 8 mm. The statistical limen for this function, the separation which should give *one* and *two* equally often, is computed as 4.03 mm., but it is obvious that there are really two limens, one above and the other below 8 mm. The present author suggested in 1916 that the paradoxical *twos* may actually represent the multiple innervation of the spots in the skin, and that the report "two" for multiple innervation may become actual only when favored by attention or other judgmental facilitation. Certainly Head's work indicated the occurrence of many *Vexirfehler* under the conditions of sensibility which he called 'protopathic.'

That something more than anatomy affects the threshold was clearly indicated by the effects of *practice*. The history of that problem goes all the way back to A. W. Volkmann who, after working with Fechner and himself as observers, published in 1858 the determinations of their thresholds for the past two years. There was a striking decrease in the threshold distance, which they took to be the consequence of practice. Camerer in 1881 reported a similar result for two children whose thresholds had been determined on various occasions from 1875 to 1880. In spite of the fact that both children were growing bigger and that the threshold, according to Czermak, grows up with the child, their thresholds decreased in five years to a little less than three-fourths the original value. The most remarkable functions were, however, published by Tawney in 1897. Testing observers on each of twenty successive days, he got decreases in the threshold as great as from 50 mm. to 0.5 mm.! Some of his practice curves were quite smooth and regu-

lar. Although these results cast great doubt upon Weber's theory, Henri in 1898 noted that practice must affect the interpretation of any sensory disparity, and that anatomical differentiation would still represent the limit beyond which practice cannot go.

About this same time Griesbach (1895) and Vannod (1896) discovered that mental *fatigue* increases the threshold. It was their idea to test school children throughout the school day. Vannod found discrimination diminishing from 8 A.M. to 5 P.M.; Griesbach found it improving from 7 A.M. to 10 A.M. and then diminishing to 2 P.M. Griesbach's interpretation was like Henri's. Drawing a picture of overlapping end-arborizations of the cutaneous nerves, he suggested that a certain amount of resulting confusion tends to increase the threshold unless maximal attention is available for discrimination. For a while after 1900, therefore, what Weber thought was a test of differentiation of nerve endings in the skin was used as a practical test of fatigue in school children.

Comparable with these findings for practice and fatigue was McDougall's discovery (1903) that the Murray Island savages of Torres Straits had an average threshold of 19.8 mm. (range, 2-40 mm.), whereas the Englishmen whom he tested when he got back to England had an average threshold of 44.6 mm. (range, 10-90 mm.). It looked as if savages were more practiced, or less fatigued, or at any rate keener than civilized men.

There was, however, thus far no good explanation of how judgmental factors enter in to affect these sensory discriminations; just such an insight was, nevertheless, to attend an understaſding of the categories and criteria of this judgment. Weber originally noted three perceptual patterns that come from two stimulating points: *one* (single sensory circle), *extended* (two adjacent circles) and *two* (two separated circles). Kottenkamp and Ullrich (1870) recorded essentially the same three categories: *point*, *elongated*, *double*. Camerer (1883) used four categories: *one*, *undecided*, *more than one*, *two*. Foucault (1910) was actually able to distinguish eight serial perceptual patterns: (1) a precise point, (2) a circle with a distinct contour, (3) an extended circle without a definite contour, (4) an elongated rectangle, oval or line, (5) an elongated area with two centers of pressure within it, (6) two overlapping areas of pressure, (7) two circles tangent, (8) two circles with empty space between them. Finally Gates (1915) chose five categories, *point*, *circle*, *line*, *dumb-bell*, *two*, added the

four intermediate doubtful categories (like *point-or-circle*) to make nine in all, and then determined the eight thresholds that separate these nine categories.

What does this situation mean? It means that there is a series of differentiated perceptual patterns, that the criterion for *two* must be placed at some critical point in the series, and that the meaning of the judgment *two* is indeterminate unless the criterion has been established. Titchener was the first (1916) to see this point. Against McDougall's interpretation, he argued that the Murray Islanders, trying to do well before the white men, would naturally select some category low in the series as the definition of *two*, a *line* or an *oval*, whereas the cultivated Englishmen, trying to be accurate and logical, would call *one* everything below two impressions with a space between them. Titchener was at pains to show that accurate spatial discriminations of subliminal separations can be made with effort and attention, if *subliminal* means anything below the limen for two-with-a-space-between-them.

Later Friedline, Titchener's student, published in 1918 a systematic investigation of spatial discrimination of two-point stimulation below the normal threshold. On a part of the forearm, where the usual limen is about 20 mm., she found that observers can distinguish, often with 100 per cent accuracy, no separation from 5 mm., 2 from 7 mm., 5 from 10 mm., and even sometimes no separation from 2 mm., and 2 from 5 mm. She found further that for such discriminations these observers required strong motivation and full attention, since fatigue or distraction readily reduced their performance from perfection to randomness. Thus she was in a position to explain the effects of practice and fatigue. Practice is effective when the observer learns to choose a finer but more difficult criterion in the sensory series. The finer criteria are, however, more difficult to use, because they are not so clearly delimited as the gross criterion. Hence fatigue, working against accurate perception, sends the limen up. The Murray Islanders were not tired nor distracted, and they tried hard.

Except perhaps to indicate that the sensory circles are not clear cut, these results do not actually contradict Weber's hypothesis. Presumably the distribution of sensory 'spots' is complicated by overlapping innervation. Nevertheless, the existence of discrete receptors must constitute the basic fact which establishes the

sensory series, whereas the judgmental factors must just as surely be regarded as additional determinants.

The Error of Localization

We must turn now to the second method of Weber (1852), the determination of the error of tactual localization. In this experiment the observer closed his eyes; the experimenter touched his skin with a blunt point dipped in powdered charcoal so that it marked the spot on the skin; then the observer tried to touch the same place with another pointer; and the experimenter measured the distance from the marked spot to its localization. This method, devised in the same year that Lotze formulated his theory of local signs, was destined to seem a truer form of localization than the two-point discrimination; it measures localization with respect to the body as a frame of reference, whereas the first method measures only the localization of one point with respect to the other.

We had better analyze the problem first in order to examine its history afterward. We have here what is, in modern terms, a stimulus-response situation, a discriminatory reaction: the subject, after being stimulated, designates the locus of the stimulation. Many forms of localizing reaction can be used. (1) Kinesthesia (*K*), as when the subject brings the pointer in his hand to what he thinks is the correct position and does not move it after it has touched the skin, or else localizes by pointing at the spot on the skin through a glass plate. (2) Touch (*T*), as when the subject identifies a second stimulation as different from the first or as the same—the procedure of von Frey and Metzner in determining the successive two-point threshold. Though pure touch without a localizing movement is not generally supposed to be a case of localization, logically it should be classed here. Weber's method used kinesthesia and touch together (*KT*), since the subject was allowed to move the localizing pointer after it had made contact with the skin: he could move by trial and error until he got a touch that felt like the first. (3) Vision (*V*), as when the subject localizes the spot on a photograph or plaster model of the bodily member stimulated. In the 1890's there also emerged the question of the use of visual imagery (*v*), so important for localization with most persons. It is possible for a subject to inhibit visual imagery; thus the localizing reaction is

KTV if the subject looks at his arm and moves the pointer around on the skin to get the best localization, or *KTv* in Weber's method, or pure *KT* if visual imagery is completely inhibited. Altogether there are eleven possible combinations of these factors, nine of which were used from 1852 to 1902 (*vide infra*).

It is possible to conceive of ways in which all of these eleven forms of reaction should also be used for the stimulus. *Tactual localization* means, of course, that the stimulus is *T*. Weber's method could be symbolized as *T-KTv*. Henri used a *K-V* method, however, when he held the subject's finger over the point to be localized (stimulus = *K*) and then had him localize the position on a photograph (reaction = *V*). A *V-V* method would be one where the subject is shown a point on his skin and then presently asked to identify it visually. Thus there are possible $11 \times 11 = 121$ methods of localizing cutaneous points. The following list shows the methods (with somesthetic stimulation) that have been used in important ways and the name of the originator.

<i>T-KTV</i> : Volkmann, 1844	<i>T-V</i> : Henri, 1893	<i>T-K</i> : Parrish, 1897
<i>T-KTv</i> : Weber, 1852	<i>TV-KTv</i> : Barth, 1894	<i>TV-Kv</i> : Parrish, 1897
<i>T-Kv</i> : Aubert and Kamm- ler, 1870	<i>K-V</i> : Henri, 1895	<i>T-T</i> : von Frey and Metz- ner, 1902
	<i>T-KV</i> : Henri, 1895	
	<i>T-KT</i> : Pillsbury, 1895	

Weber's finding in 1852 was that the error of localization varies with the bodily region and, although roughly proportional to the two-point limen, is always larger than the two-point limen. The upper half of Table X shows this proportionality, which was actually the reason for Weber's thinking that he had in such a method a second measure of the *Ortsinn*. Kottenkamp and Ullrich verified this relationship in 1870 (lower half of Table X), showing that the error of localization, as well as the two-point limen, follows Vierordt's law of mobility. Because there remains, however, the fact that one measure is three to four times the size of the other, this discrepancy seemed to spoil the possibility that both are measures of the size of sensory circles. By 1898 Henri was ready to argue that the two judgments are based upon different physiological mechanisms—a view which influential leaders, like Wundt, were inclined to accept. Henry Head (1905), as we have seen, assigned the two capacities to different afferent systems. There is really, however, no fundamental incompatibility between these two measures. A single stimulation gives rise to a dispersed region of excita- ✓

tion such that two points at the limen of duality are quite far apart and yet lie at the center of regions tangent in the perceptual pattern. If the error of localization lies always within this region of dispersion, so distributed that on the average it is, say, at half the radius of the region from the center, then the error of localization

TABLE X. RELATION OF THE TWO-POINT LIMEN TO THE ERROR OF LOCALIZATION

WEBER (1846, 1852)

<i>Location</i>	2-pt. limen = <i>L</i> (mm.)	Error local. = <i>E</i> (mm.)	<i>L/E</i>
Anterior thigh	67.5	15.7	4.3
Volar forearm	40.5	8.5	4.8
Back of hand	31.5	6.5	4.8
Forehead	22.5	6.3	3.6
Lips	4.5	1.1	4.5
Finger tip	2.3	1.1	2.3

Av. = 4.0

KOTTENKAMP AND ULLRICH (1870)

<i>Location</i>		2-pt. limen = <i>L</i> (mm.)	Error local. = <i>E</i> (mm.)	<i>L/E</i>
<i>Volar</i>	5.9	37.4	14.2	2.6
<i>upper arm:</i>	13.6	36.2	10.6	3.4
<i>cm. from</i>	21.3	33.8	9.4	3.6
<i>shoulder</i>				
<i>Volar</i>	2.2	27.0	8.7	3.1
<i>forearm:</i>	6.4	22.6	8.4	2.7
<i>cm. from</i>	11.3	20.7	8.0	2.6
<i>elbow</i>	16.2	21.0	6.2	3.4
	21.1	14.2	4.4	3.2

Av. = 3.1

would be half a radius, and the two-point limen would be the distance between the two centers, or two radii. The supposed discrepancy would seem to be more a function of method than of physiological fact. This argument, made first by Czermak in 1855, has been recently repeated (1930, 1935).

In making tactual localization most persons use visual imagery. The conscious 'local sign' of a touch is the 'look' of the place where it is. Only the congenitally blind and a few other persons who lack visual imagery make their localizations entirely in kinesthetic terms. In the 1890's, when consciousness was so important in psy-

chology, there was considerable discussion as to the extent to which visual imagery aids localization. Pillsbury found in 1895 that the error is largest in the Weber method when visual imagery is inhibited by the subject ($T-KT$), smaller when visual imagery is freely allowed ($T-KTv$), smallest of all when vision is encouraged by allowing the subject to look at his skin while he is making the localization ($T-KTV$). Parrish similarly reported in 1897 that the error is largest when the subject localizes kinesthetically by pointing at the spot without touching it and also inhibits visual imagery ($T-K$), smaller when visual imagery is allowed ($T-Kv$), smallest when the visual imagery is encouraged by allowing the subject to watch the stimulation, having closed his eyes during the localization ($TV-Kv$). In short, the error diminishes as follows:

$$\begin{array}{l} \text{Pillsbury: } (T-KT) > (T-KTv) > (T-KTV) \\ \text{Parrish: } (T-K) > (T-Kv) > (TV-Kv) \end{array}$$

The more vision is favored, the smaller the error, but it was never clear that the effort of inhibiting visual imagery by normal visualizers did not act as a distraction and increase the error in that manner.

Another fact of importance in localization is that there are usually large constant errors. Both Lewy (1895) and Parrish (1897) found that points in the middle of the volar forearm tend to be localized considerably toward the wrist, that points near the wrist are also displaced toward the wrist, although less, but that points near the elbow are apt to be shifted in localization toward the elbow. Such errors Henri attributed to attention. In perception the skin is laid out in respect of certain points or regions of anchorage (*Anhaltspunkte*), which provide a frame of reference. Localizations of points remote from these orienting regions tend to migrate toward them, because attention is upon the frame and there are no definitely perceived intermediate points to make the migration seem wrong. Not often does a localization on a finger spread across a knuckle or from one finger to the next.

There has been other work on the error of localization, such as the determination of the effect of the time-interval between stimulation and localization (Barth, 1894) and the measurement of the size of the error for different modalities (Ponzo, 1911-1913), but in general the history of this problem has shown less progress

toward psychological insight than the history of the other measure of cutaneous space perception, the limen of duality.

Stimulation

How the psychophysiology of smell and taste have been held back by the failure to discover the essential natures of the olfactory and gustatory stimuli we have already seen. The study of cutaneous sensibility has been only a little more successful: we know something of the characteristics of the stimuli, but we have not yet identified the receptors.

To Weber and his predecessors it was obvious that the perception of pressure is caused by pressure; thus the sensation could properly be named for its stimulus. As early as 1859, however, Meissner—the man who discovered the ‘tactile’ receptor-corpuscles in the skin of the palms and soles—pointed out that physical pressure is actually in itself imperceptible. If a hand or a finger is immersed in water or mercury of the same temperature as the skin, there is no consequent sensation except at the ring where that member emerges from the liquid. For this reason Meissner concluded that the stimulus to the pressure sensation is not physical pressure itself, but the deformation of the skin, that the true stimulus is, in modern phrase, a pressure-gradient in the skin. Meissner even made paraffin casts of regions of the skin, applied them so carefully that the entire surface of the cast came in contact with the skin at the same instant, and decided that only the edges of the cast could then be perceived.

The gradient theory of the pressure stimulus was carried further by von Frey, at first in independent publication (1896), and later in more detail with Kiesow (1899). For this investigation von Frey invented two instruments that later became classical. The first was the limen-gauge (Fig. 81). It is an instrument through which force can be applied mechanically to a spot on the skin at any predetermined rate of loading. A kymograph actuates it; a scale indicates the maximal force applied. The stimulus is often a blunt wooden point on a lever-arm, but for what von Frey called “macroscopic” stimuli (0.5 mm. diameter and larger) a cork or cardboard disk of the right size was placed beneath this point. The other instrument which von Frey invented was the stimulus-hair

(Fig. 82), which he used for "microscopic" stimulation (of the order of 0.2 to 0.05 mm. diameter). After the end of the hair, *S*, has been applied to the point to be stimulated, the stick-handle is depressed in such a manner as to keep the point of inflection, *P*,

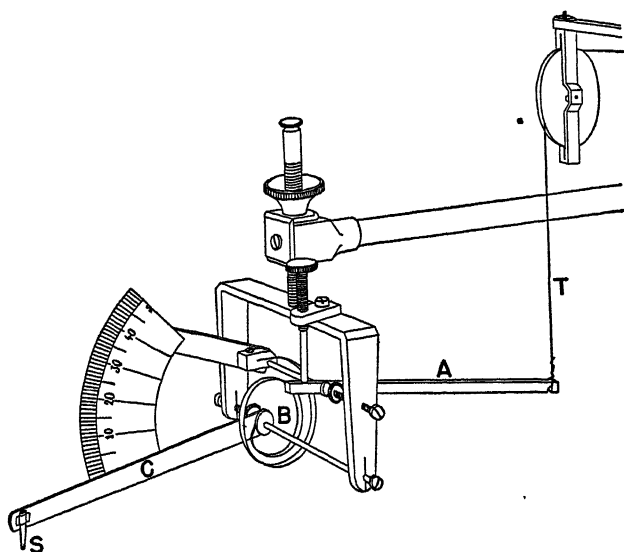


FIG. 81. LIMEN GAUGE: VON FREY (1896)

A spring-driven kymograph deflects a lever (not shown) at a predetermined rate. This lever pulls the thread, *T*, raising the lever, *A*, and exerting downward force at *S* through the spring, *B*, and the lever, *C*. The scale shows the amount of force. *S* is a blunt wooden stimulus-point, but "macroscopic" stimuli of different areas can be had by placing small cork or cardboard disks of the desired sizes between *S* and the skin.

directly over the point of application, *S* (Fig. 82B). With such use the force remains perpendicularly directed upon the skin, and the loading is practically instantaneous, because the hair acts like a loaded long column in which the maximal force is required to make it bend at the start. Hairs of different diameters are chosen; for a given hair the force depends upon the length of the hair. Von Frey also constructed another hair stimulator in which the hair projected through a long narrow hole in an adjustable sleeve which permitted the extrusion of a short length of the hair for heavy loading or a long length for light loading.

The two chief results of von Frey's investigation were as follows.

(1) The threshold for pressure varies inversely with the rate at which force is applied; it is less the more rapid the loading. For instance, in one experiment with a macroscopic stimulus of about 21 sq. mm., the threshold force decreased from 2 to 1 gm. when the rate of application was increased from 1 to 2 gm. per sec.

(2) With microscopic stimuli and rapid loading—that is to say, with the hair-stimuli—the threshold is constant for various sizes of stimuli if expressed in gm. per mm., the quotient of the force by the average radius of the hair. (See Table XI.) This rule does not hold, however, for larger stimuli, for which presumably more than one receptor is excited. With large stimuli the threshold is higher the larger the area of application, even when expressed in gm. per sq. mm.

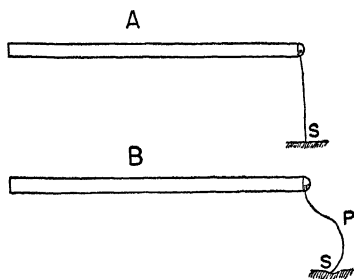


FIG. 82. VON FREY HAIRS (1896)

Hairs of varying diameters (horse, human) and varying lengths are fastened with sealing wax to the ends of sticks. When the stick is depressed, the force is applied perpendicularly to the skin if the point of inflexion, *P*, is kept directly above the point of application, *S*. These are "microscopic" stimuli and the loading is "rapid" because the hair acts like a long column in which the maximal bending load must be applied at the beginning of the bending.

On the invariance of the threshold in gm. per mm. with microscopic stimulation von Frey based his theory of the pressure stimulus. The effectiveness of this stimulus is not measured by the total force (gm.), nor by the hydrostatic pressure (gm. per sq. mm.), but by the tension (gm. per mm.). He drew, consequently, an analogy with surface tension which also depends, not on the area, but on the linear dimensions of the surface film. The adequate stimulus to the pressure sensation under these conditions seems to be tension set up within the skin or its own stretching. Another way of stating the same generality is to say that stimulation depends on the deformation of the skin where the tensions are set up, principally, that is to say, at the edges of the deforming stimulus. This view that a pressure gradient excites the receptors for pressure was supported further by von Frey's discovery that traction is as good a stimulus as a load; lifting an object attached to the skin elicits the pressure sensation as readily as applying downward force. A pres-

TABLE XI. LIMINAL PRESSURE STIMULI: VON FREY AND KIESOW (1899)

For "microscopic" stimuli (hair-stimuli) the "tension" is constant for different sizes of stimulus; force ("loading") and pressure ("atmospheres") are not. This relationship does not hold, however, for the larger ("macroscopic") stimuli. These data are interpolated from the continuous functions of von Frey and Kiesow.

	Radius mm.	Area mm. ²	Force gm.	Tension gm./mm.	Pressure gm./mm. ²
Micro- scopic stimuli	.02	.0013	.017	.835	13.30
	.04	.0050	.034	.850	6.76
	.06	.0113	.051	.858	4.55
	.08	.0201	.068	.850	3.38
	.10	.0314	.085	.852	2.71
Macro- scopic stimuli	.40	.5026	.152	.380	.30
	.50	.7854	.251	.502	.32
	.60	1.1310	.547	.911	.48
	.70	1.5394	.970	1.386	.63

sure gradient is established, of course, for either direction of the force which deforms the skin and introduces tensions within it.

The specification of the *thermal stimulus* is complicated by the fact of thermal adaptation. In his account of the nature of secondary qualities John Locke in his famous *Essay* of 1690 touched on this problem. Inasmuch, he observed, as the same water may feel hot to one hand and cold to the other, the reason must be that the sensations of heat or cold depend on the "increase or diminution of the motions of minute parts of our bodies, caused by the corpuscles of any other body." Thus, "if that motion be greater in one hand than in the other, [and] if a body be applied to the two hands which has in its minute particles a greater motion than those in one of the hands, and a less than those of the other, it will increase the motion of the one hand and lessen it in the other; and so cause the different sensations of heat and cold that depend thereon." Thus Locke's 'theory' of the thermal stimulus is that the hand feels warm when it is being warmed, cool when it is being cooled, and that the same stimulus that warms a cold hand may cool a warmed one.

Weber's theory in 1846 was the same as Locke's: when the temperature of the skin rises, one perceives warmth; when it falls, cold. Vierordt, however, having at first accepted Weber's theory, objected in 1871 that a flow of heat without a change of temperature may elicit the thermal sensation—cold when the flow is out-

ward, warm when it is inward. For illustration he cited the persistence of the sensation of cold when a very cold object, having been in contact with the skin, is removed. It was his contention that the outer skin is in this case so cooled by the object that heat from the blood continues to flow toward the skin while the skin is warming up. On Weber's principle warmth should be felt because the skin is warming up; on Vierordt's, cold because the conduction is outward. As a matter of fact, this situation probably involves a positive after-sensation, a neural rather than a thermal latency, yet Vierordt's case is also supported by other factors. Warmth, for example, can be felt for such long times in the presence of radiant heat that it would seem stable temperatures must be established before the sensation ceases and while the flow of heat still continues. The converse situation also holds, of course, for cold.

The next step in establishing a theory was taken by Hering. Having published in 1877 a theory of thermal sensation, modelled after his theory of vision, and having written in 1879 the section on *Temperatursinn* in Hermann's *Handbuch*, Hering noted certain difficulties with Vierordt's theory. Since the blood is warmer than the skin, there is a constant outward flow of heat, which is not sensed as cold and indeed gives rise to no thermal sensation at all. Thus, if the skin is warmed up from neutrality, the initial stimulus to warmth is not conduction inward, but a diminution of conduction outward. Arguing that a diminution of outward conduction can be equivalent to inward conduction only if conduction has some relation to an equilibrium of the sense-organ, Hering undertook to describe the operation of the thermal organ in terms of *fatigue* (adaptation), *contrast* and *physiological zero* (*Nullpunkts-temperatur*). The stimulus to temperature, he said, is the amount of heat conducted to the sense-organ, and conduction outward must be considered as a greater or lesser amount of negative conduction inward. Positive conduction inward sets up in the organ a process that may be called *dissimilation*; negative conduction inward, the reverse process of *assimilation*. When such stimulation is continued, however, the organ becomes fatigued until the process, diminishing, ceases: there is adaptation, and a new *Nullpunkts-temperatur* is established. Thereafter, increase in positive conduction again arouses the dissimilation process, whereas decrease in positive conduction (or an increase in negative conduction)

arouses assimilation, a contrast effect. There is no problem as to why a decrease in positive conduction should have the same effect as an increase in negative conduction, because dissimilation and assimilation are probably both going on all the time, and what is meant by dissimilation is merely the preponderance of the dissimilative process over the assimilative, and conversely. Although Hering's notion of these two antagonistic excitatory processes has not been generally accepted, his basic principle is regarded as valid today: the organ adapts to any continuous thermodynamic situation, establishing a new point of equilibrium or physiological zero. Thereafter, any deviation from this steady state toward the conduction of more heat to the receptor is a stimulus to warmth, and the contrary deviation a stimulus to cold.

In this connection certain facts must be recalled. Weber, believing that the temperature and pressure senses are one, considered that he was dealing only with different modes of stimulation of the same sense. Hering, however, decided that these two senses are independent, although warmth and cold he thought of as belonging to a single temperature sense. Von Frey, on the contrary, held that warmth and cold are separate senses with different organs. Von Frey's theory would have been fatal to Hering's had it not been for the fact of successive contrast between warmth and cold, a fact that John Locke used to prove these qualities secondary, a fact that made it plausible to suppose the existence of some connection between the organs for cold and warmth.

In spite of the ascription of thermal sensibility to a temperature sense and the habitual specification of thermal stimuli in terms of temperature, it is true that no sophisticated investigator of these perceptions had ever, since Weber, regarded temperature as an adequate measure of the thermal stimulus. Hering was at pains to show that thermal conductivity of the stimulus-object is important: at a given temperature wood is less cold or warm than metal. Sherrington in 1900 listed the effective properties of the thermal stimulus: temperature, thermal conductivity, specific heat, intimacy of contact with the skin, and the increments or decrements of radiation. He might have added mass, for a tiny drop of boiling water has in it insufficient heat to excite even a single warm spot.

Von Frey carried Hering's neural theory of thermal sensitivity one step further in 1895 by his discovery of paradoxical cold, the stimulation of a cold spot with a warm stimulus (above 45° C. in

some parts of the skin) resulting in the elicitation of the sensation of cold—as ought to happen under the theory of specific nerve energies. Although recent research makes it doubtful whether the phenomenon is as universal as von Frey believed, this discovery has nonetheless been amply verified. We shall see later how Alrutz argued that the perception of heat is a fusion of normal warmth and paradoxical cold (pp. 504 ff.). If the same stimulus can give warmth at one place and cold at another, then there must be two kinds of receptors.

Two recent theories of thermal stimulation are Jenkins' and Nafe's. Jenkins, who failed to verify von Frey's exact localization of cold and warm spots and also the universal occurrence of paradoxical cold, formulated in 1938 a theory analogous to Hering's. Assuming the existence of a "pseudo-reversible reaction," in which stimulation causes the reaction and a catalyzer the reversal, he explained most of the normal phenomena in this fashion. Nafe and his associates, on the other hand, also opposing von Frey's theory, have presented evidence that warmth is the proprioception of the relaxation of the vascular muscles in the skin, and cold of the contraction of the vascular muscles. This theory also is a return to Hering in so far as it makes of warmth and cold a single system. It cannot be said, however, that either of these theories has as yet received general acceptance.

The adequate stimulus for cutaneous *pain* has never been so precisely specified as it has for the thermal sensations and for pressure. Although a sharp needle or thorn penetrating the cells of the epidermis will elicit pain, the sharpness of the stimulus seems to be more a way of getting at the receptor than the property for exciting it. It is not easy to give precise specification to the sharpness of a pricking point, and the threshold for prick-pain, when force is varied, seems to depend primarily upon the hardness of the superficial layers of the epidermis. For quantitative determinations von Frey used a flat areal surface and a force far above the threshold for pressure; yet it does not seem that a blunt applicator should constitute a normal stimulus. Since it is true, moreover, that the pain threshold thus determined may be no higher than the pressure threshold if enough of the superficial cells of the epidermis have been scraped away, one seems here also to be measuring the adequacy of an obstruction to excitation rather than the nature of excitation. The best algesic stimulus, the most constant

and reliable, is radiant heat, as Hardy and his associates have recently (1940) shown. They found that the threshold for pain on the forehead is remarkably constant from point to point, from time to time, and from person to person, and that sensitivity to pain is diminished by opiates in the system, by acetylsalicylic acid at the spot, by intense pain elsewhere in the body, but not by occlusion of the blood supply. Even this excellent method provides, however, no hint as to the fundamental nature of the algesic stimulus.

The persistent ignorance about the nature of the algesic stimulus is, of course, consistent with the equivocal status of pain as an independent sense. We have already seen how Weber consigned pain to the *Gemeingefühl*, and how later investigators regarded it as common sensation or the consequence of intense excitation of any sense. Goldscheider especially made the argument that pain is aroused by the summation of pressure excitations. It was not until 1895 that von Frey seemed to establish pain as a separate modality with independent receptors of its own, the free nerve-endings in the epidermis. With the discovery that the free endings must also mediate warmth and cold and perhaps some pressures too, however, his argument lost some of its force. Head (1905 *et seq.*) associated pain with extreme warmth and cold in protopathic sensibility. It is no wonder then that the stimulus for pain is unknown, when the existence of pain as an independent sense with separate receptors has until recently been in doubt.

It is true that von Frey had a theory of pain, but it was tied up with his belief about the receptors for pressure and pain. Having demonstrated, as he thought, that pressure depends upon the excitation of the free nerve endings about the hair follicles beneath the epidermis, and that pain depends upon the excitation of the free nerve endings between the cells of the epidermis, he had to show why the deeper organs are so much more readily excited than the more superficial ones. His answer was, of course, that the tough epidermis, protecting, as it does the endings lying within it, transmits a displacing force to the organs in the softer underlying tissue. Thus he showed, using hair-stimuli with enough force to elicit pain, that the pain threshold is about constant for different-sized hairs when hydrostatic pressure (grm./mm.^2) is used, and that surface tension (grm./mm.) is not a measure of stimuli equivalent for pain as it is for pressure. Such a result would be natural: the pres-

sure per unit area would be the most likely measure of the force necessary to get at receptors within the leathery epidermis. The theory, however, gives us insight neither into the properties of the pain receptor nor into the means whereby intensive degrees of pain can be uniformly aroused.

In general, then, it may be said that the psychology of cutaneous sensations is held back by lack of knowledge of the nature of the adequate stimuli and of the receptor organs. The history of the field waits upon new insights.

Weber Function

When Weber found that $14\frac{1}{2}$ and 15 ounces, and also 29 and 30 ounces, as well as $14\frac{1}{2}$ and 15 drams, placed successively on the finger, could be discriminated "only with the greatest difficulty," he naturally concluded, without investigating a long range of weights, that Weber's law, as it was called later, holds for weight in the *Tastsinn*. The discrimination ratio for the finger he therefore set down at $\frac{29}{30}$, so that the Weber fraction, in modern phrase, was $\frac{1}{30}$.

Next he raised the question as to how discriminatory capacity would vary with sensitivity of the skin. The *Ortsinn* (two-point threshold) is one-ninth as fine on the forearm as on the finger, and a five-ounce weight on the forearm feels about as heavy as a four-ounce weight on the finger. Weight-discrimination in these two regions turned out, however, to change in an amount proportional to neither of these other changes. On the finger the ratio was $\frac{19.2}{20}$ (ca. $\frac{29}{30}$), and on the forearm $\frac{18.2}{20}$. If we convert these fractions into comparable figures, add the value that Weber got for muscle (weights held by the fingers in a cloth sling), and write the Weber fractions, we have:

Finger muscles:	$\frac{5}{200} = 0.025$
Finger skin:	$\frac{8}{200} = 0.04$
Forearm skin:	$\frac{18}{200} = 0.09$

We must now, as we trace the history of Weber function for the skin, exclude from consideration the accurate muscle sense. The first determination of this function, the variation of the Weber fraction throughout a considerable range of weights, was made by Biedermann and Löwit and reported by Hering in 1875 when he

was discussing Weber's law. It is natural that Weber should not have made observations through a long range of intensities; he came to the problem with a conviction that the 'Weber fraction' is constant and that the 'Weber function' is therefore a straight horizontal line. Biedermann and Löwit's values are given in Table XII and Fig. 83. This figure shows how much they vary and how they vary, a continuous function except at one end. What impressed the psychologists of the nineteenth century and even later was, however, the relative constancy of the fraction in the middle

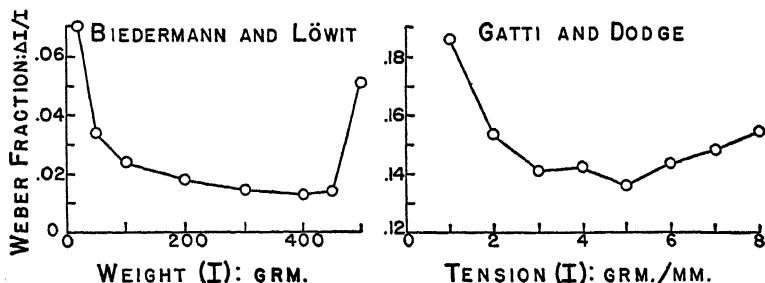


FIG. 83. WEBER FUNCTIONS FOR PRESSURES: BIEDERMANN AND LÖWIT (1875) AND GATTI AND DODGE (1929)

Biedermann and Löwit used deep pressure, Gatti and Dodge cutaneous pressure on an isolated pressure spot. The function obtained by Gatti and Dodge is smoother than Kiesow's (1922).

range. If the absolute weight is quadrupled from 100 to 400 grm., the fraction is not quite halved (0.024 to 0.013); from 200 to 450 grm. it is even more constant. These values of the stimulus, 10 to 500 grm. mean, of course, that deep pressure, not truly cutaneous pressure, was being studied. Neither Weber nor any other of the psychophysicists before von Frey (1895) and Head (1905) realized that it takes very light loads to test only the skin and not the underlying organs.

There seems to have been no thorough determination of the Weber function for the skin alone until the investigation by Kiesow in 1922. He used von Frey hairs on single pressure spots, establishing the function for intensities from 1 to 8 grm./mm. Gatti and Dodge by an even more careful technique verified his findings in 1929. See Table XII and Fig. 83. In one respect both curves resemble the Weber functions in many other departments of sense (though not all): they show that the Weber fraction, far from

being constant in accordance with Weber's law, has a minimum in the middle ranges of intensity.

The problem of the validity of Weber's law for temperature at once comes up against the question of the location of zero intensity in the scale of temperature. The I for warmth must be

TABLE XII. WEBER FUNCTIONS FOR DEEP PRESSURE AND CUTANEOUS PRESSURE

I = intensity. ΔI = just noticeable difference. $\Delta I/I$ = Weber fraction. Biedermann and Löwit (1875), who used weight in grm. as I , were determining a function for deep pressure. Kiesow (1922) and Gatti and Dodge (1929), using tension in grm./mm. as I , were determining functions for isolated pressure spots in the skin.

Weight in grm. I	Biedermann and Löwit $\Delta I/I$	Tension in grm./mm. I	Kiesow $\Delta I/I$	Gatti and Dodge $\Delta I/I$
10	.070	1	.2100	.1854
50	.034	2	.1504	.1529
100	.024	3	.1647	.1411
200	.018	4	.1404	.1425
300	.015	5	.1385	.1356
400	.013	6	.1453	.1442
450	.014	7	.1705	.1486
500	.051	8	.1948	.1548

measured up from zero temperature, and the I for cold measured down. Weber did not attempt to settle the question. Fechner, however, in 1855 conducted some experiments, adapting two fingers to the same temperature in water, and testing differential sensitivity by immersing one finger in water of one temperature and then the other finger in water of another temperature. For the zero of his system he chose 18.5°C., which is halfway between the temperature of the blood, 37°C., and freezing, 0°C. (Actually he used the Réaumur scale of temperature, but we may translate his figures into Centigrade.) His results in terms of temperature, T , and in terms of degrees above and below this arbitrary zero, I , were as follows:

<i>Cold</i>	$T = 5$ to 13° C.	$I = -13.5$ to -5.5 ° C.	$\Delta I = 0.002734 I$
	$T = 13$ to 18.5° C.	$I = -5.5$ to 0° C.	ΔI is too small to measure accurately
<i>Warmth</i>	$T = 18.5$ to 25° C.	$I = 0$ to 6.5° C.	ΔI is too small to measure accurately
	$T = 25$ to 39° C.	$I = 6.5$ to 20.5° C.	ΔI is approximately proportional to I

With this zero, Weber's law is approximated only above 25°C., but this is a strange zero. Fechner did not know the temperature of the skin. He did know that body temperature is too high to be the zero, for body temperature feels warm to the skin. Thus he took the average between body temperature and the artificial zero of the Réaumur thermometer (freezing point of water).

In 1867 Nothnagel determined the temperature of the skin as lying between 33° and 36°C. when room temperature is about 18°C. By a method similar to Fechner's and by the use of cold and warm metal cylinders, he satisfied himself that ΔI is minimal in the region of skin temperature and just below, whereas above and below this region it increases slowly at first and then rapidly. Such a mode of variation, although it lies in the direction of Weber's law, does not validate the strict proportionality that it requires.

Notwithstanding the fact that there have been other attempts to determine this relation, it has been hard to get consistent functions. Miss Abbott in 1914 published a thorough study in which she showed (1) that ΔI is minimal at skin temperature (32.5°C.) whatever the state of adaptation of the skin, (2) that ΔI increases steadily above and below skin temperature although not strictly in accordance with Weber's law, and (3) that the rate of increase in ΔI is more rapid the greater the deviation of adaptation-temperature above or below 32.5°C. In other words, normal skin temperature furnishes a fixed point of orientation for the sensory system irrespective of the migration of physiological zero above or below it.*

The failure to discover the nature of the intensive stimulus to pain has left the investigation of the algesic ΔI and of Weber's law for pain an impracticable undertaking.

Summation

That temperature sensations are stronger when the area of stimulation is larger was observed by Weber. Warm water feels warmer to the immersed hand than to a single immersed finger, cold water colder to the hand than to a finger. "It appears," he said, "that impressions of warmth elicited by many sensory points summate in the brain, whither they are conducted, bringing about a total impression." This thermal summation he had to localize in the brain because he believed that all the sensory circles are isolated at the

skin and projected upon the brain, thus accounting for spatial discrimination under the *Ortsinn*. Similarly he explained the elicitation of pain by extreme warm or cold stimulation as due to summation. In addition he noted that thermal summation occurs less readily when the points of stimulation are far removed from one another than when they are adjacent.

That Weber's theory of summation for warmth and cold is correct has been subject to some doubt. Barnholdt and Bentley in 1911 thought he was wrong. Their experiments indicate that the larger area feels warmer (1) because it more often includes the more sensitive warm spots and (2) because the larger area is a more adequate stimulus for every receptor near its center. A similar argument would hold for cold.

We have already seen how Goldscheider in 1892 elaborated the notion that pain is due to summation, especially—and in this he was unlike Weber—the summation of successive pressures (pp. 468 f.). While this theory of pain had its adherents for a long time, von Frey's notion that pain is a separate modality with its own receptors finally won out. Hardy and his associates (1940), using radiant heat as a stimulus, moreover, found no evidence at all for algesic summation.

It was von Frey's pupil, Brückner, who in 1901 established the fact of the summation of pressures. Although Weber's theory of the *Ortsinn* seemed to require that pressures should not summate, Brückner showed that there was no reason why two adjacent stimulations, fused into one, should not give a single impression that appears more intense than either of the component stimulations alone. His data exhibit many instances of two weak pressures, whose stimuli are separated by distances of from 2 to 30 mm., yielding a single more intense pressure when applied simultaneously. He also described, less extensively, cases of inhibition or "subtraction," as he called it. Inhibition occurs at wider separations (e.g., 62 mm., 84 mm.); at such separations two sensations may be weaker, stimulated simultaneously, than either of them when the stimulation is given separately. Brückner also found that two weak supraliminal stimuli, when given simultaneously and not too close to each other, sometimes give rise to but a single sensation; and in such a case he believed that it is attention that determines which of the two is felt.

•

Adaptation

Because of the reciprocal relation of thermal adaptation and sensitization in respect of warmth and cold, more has been written about adaptation to temperature than to pressure. The fact of *thermal adaptation* has, indeed, always been embodied in common sense. John Locke in his discussion of primary and secondary qualities (p. 490) simply alluded to the phenomenon as something known to all. Weber based his theory of the *Temperatursinn* upon it. Hering's concept of a variable *Nullpunktstemperatur* is a theory of it. The principle, in short, was indubitable. It was necessary only to work out the quantitative functions.

Of these quantitative studies there have been a number, but, since they have not settled the question of the nature of the thermal stimulus, they furnish little more than handbook data—the upper and lower limits of adaptation in terms of temperature, the rate of adaptation, the effect of adaptation upon differential sensitivity. Miss Abbott (1914), in the study already cited (p. 498), was able to adapt the skin of the fingers to all temperatures between 17° and 40°C., in the sense that 18° would feel warm to skin long exposed to 17° and that 39° would feel cool to skin long exposed to 40°. Under these extremes of adaptation, skin temperature does not vary so much from its norm (32.5°C.) as does adaptation temperature, a fact which favors Hering's theory over Weber's. Miss Abbott also found, as we have seen, that normal skin temperature remains the orientation point of the functions for discrimination, even though the actual skin temperature may differ from it somewhat and the adaptation temperature even more. Some of these findings Gertz confirmed in 1921, although he was not able to get complete adaptation to temperatures above 35°C. Jenkins in 1937, studying adaptation for isolated cold spots, found that complete adaptation is often attained in four seconds, but that long exposure to cold after adaptation renders recovery slow and sensitivity still unstable after apparent recovery.

Adaptation to *pressure* has also been long embedded in common sense—the unfelt ring on the finger, the spectacles lost because they have been pushed up on the forehead. It is taken for granted that sensations should 'fatigue.' There has not, however, been much measurement of pressure adaptation, perhaps because the phenomenon has not had as great theoretical significance as

thermal adaptation. The early phenomenological papers, in describing the course of the pressure sensation with a continuing stimulus, note how its intensity diminishes or how the sensation fades out. Both von Frey (1896) and Kiesow (1896) gave such accounts. Many of the quantitative relations have recently been determined by Zigler (1932), who has shown that adaptation time is directly proportional to the weight of the stimulus and inversely proportional to its area. On the hand he got complete adaptation in 2.4 sec. for 50 mgrm., and in 9.5 sec. for 2000 mgrm. The times for the face were longer.

The occurrence of adaptation for *pain* has been doubted. The lapse of time does not seem to relieve the pain of neuralgia, of peptic ulcers, or of cutaneous burns. Cutaneous pain, however, the kind that comes from a light prick of a needle or a thorn, should not be judged by these traumatic instances. There is evidence that the initial prick of a needle may fade out quickly if the needle is kept perfectly still. Miss Murray in 1908 suggested that it is "quite likely that cutaneous pain is mediated by two sets of endings, the intense but quickly fading variety . . . and the more severe and lasting (including ache)." Strauss and Uhlmann in 1919 obtained complete adaptation to a 3-grm. load on a needle in from 4 to 26 sec., and to an 8-grm. load in 13 to 72 sec. The interpretation of this result is not certain, since nobody knows how the pain receptors work or whether, indeed, the free nerve endings are receptor-organs or simply free endings, excited directly after the manner of pain fibers in a pricked nerve trunk. If the pain sensation fades out merely because the needle throws the fibers out of function by creating a tiny lesion in the neural tissue, then we should not be considering the kind of effect that is ordinarily called adaptation.

After-Sensations

Tactual sensation may persist after the removal of its stimulus, or, having been lost under adaptation, may recur upon the removal of its stimulus; it may even recur a brief interval after the removal of the stimulus. The hat on the head is no longer sensed but is felt after being taken off. Although not so frequently as adaptation, the fact of tactual after-sensation is often recognized in common sense. Nevertheless Weber did not mention it, nor Fechner.

As soon as the active period of research on cutaneous sensibility

got under way, however, the after-sensation began to be mentioned in print, either as an incidental phenomenon, or as a difficulty which interfered with an experiment. Kottenkamp and Ullrich, for instance, in their study of tactual localization in 1870, found that it was necessary to have the subject wait, before making the localization, until the after-sensation from the stimulus had disappeared. In 1880 Funke mentioned after-sensations for pain as well as pressure; in 1881 Preyer noted after-sensations for warmth and cold—the positive after-sensations of persistent warmth or persistent cold, not the familiar negative after-sensations where warmth follows cold, or cold warmth, due to the shift of the physiological zero. Thereupon, after Goldscheider (1882–1885) had described the after-sensations for all four qualities, Urbantschitsch, in 1887, measured their duration and, in those cases where the sensation lapsed and then recurred, the duration of the latent interval. In the 1890's there were at least a dozen descriptions of these phenomena, including von Frey's accounts of adaptation and the persistence of sensation after the removal of the stimulus, experiments which we have just cited.

It was in 1892 that Dessoir pointed out that we have here really two phenomena: (1) the persistence of sensation, which he called "continuous after-sensation," and (2) the recurrence of sensation after an interval, which he called "intermittent after-sensation." This is a useful distinction, because all sensations necessarily have some lag in cessation, but recurrence after cessation implies the operation of some special receptor process. Later the two kinds were distinguished respectively as primary and secondary after-sensations, and also as after-sensations and after-images.

Likewise in 1892 Goldscheider published, with the assistance of Gad, an elaborate study of after-sensations, a study which was directed almost entirely to the support of the summation theory of pain which we have considered elsewhere (pp. 468 f.). It was their discovery that weak electrical or mechanical stimulation yields pressure, strong stimulation pain, and a controlled series of stimulations (like four or five induction shocks 30–60 millisecon. apart) a primary sensation of pressure but a secondary after-sensation, following an interval, of prick pain (*eine stechende Empfindung*). One shock was never enough to give the after-sensation of prick, which could, however, be got with from two to seven shocks, provided the timing was right. The greater the number of shocks, the

more rapidly did they have to be given. This secondary pain, Goldscheider inferred, is due to summation of successive effects of stimulation. Occasionally the after-effects appeared, not as pain, but as a secondary pressure—a case of their being felt without summation, he thought.

Thunberg in 1902, although concerned to discredit Goldscheider's summation theory, reported in great detail on 'double pains.' A small disk of metal—he called it a *Temperator*—could be taken from hot water and laid on the skin. It cools off rapidly, but in so doing it gives rise to a primary sensation which Thunberg thought was a mixture of pain and pressure, and then, after a brief interval, there is a secondary after-sensation which Thunberg described as pure pain without the pressure. Needles too, applied with constant pressure and without subsequent movement on the skin, give rise to two successive pricks, a fact which Miss Murray, as we have just seen, thought might mean the successive excitation of different receptors. Although the conclusions here are confusing, the fact that the secondary pricks are not felt until after the stimulus has ceased to act was nevertheless established without question.

Urbantschitsch in 1905 reported a study of the localization of after-sensations. The secondary after-sensation, he found, may be localized differently from the primary sensation, may "irradiate" over an area, or migrate within the area. If a second stimulus be applied at the time when the after-sensation is about to appear, then the after-sensation is likely to migrate toward the second stimulus. It is also true that the appearance of an after-sensation is likely to bring with it—especially in the case of cold—the re-arousal of sensations localized at other points which have been recently stimulated. There are, in other words, after-effects which may result in after-sensations; in a tactual mechanism so complex a sensory spot does not act in isolation. One has thus to regard fairly large areas of skin as interrelated in an excitatory system.

Mrs. Hayes, publishing in 1912 a study of the time-relationships of primary and secondary after-sensations and of the intervening intervals, found great variability. By limiting stimulation to the skin, Dimmick in 1916 sought to get more constant results for pressure. With von Frey hairs too weak to excite subcutaneous pressure, he worked on isolated pressure spots. These ratios of durations he found to be approximately constant—primary after-sensation:

latent interval : secondary after-sensation = 2:1:7. There were, however, huge individual differences in actual times, which were, indeed, about seven times as large for the slowest observer as for the fastest.

Finally Holland showed in 1920 that the instability and variability of after-sensations is dependent in part upon the adventitious introduction of central factors. It makes a great difference whether the observer assumes a passive receptive attitude, or an active attitude in which he attempts to secure as much cutaneous experience as possible. With the passive attitude the primary after-sensations lasted only from 0.7 to 2.0 sec. and the secondary after-sensations were few and brief, whereas with the active attitude the primary after-sensations lasted on the average about 4 sec. (although sometimes as long as 200 sec.) and the secondary after-sensations were both very numerous and often of very long duration. Whether these numerous delayed persistent after-sensations were true after-sensations (peripherally excited) or voluntary imagery (centrally excited sensations), Holland could not, of course, tell.

Heat

One of the interesting and controversial problems of thermal sensitivity concerns the nature of the perception called *heat*, a special quality to be distinguished from warmth. Von Frey, as we have seen, discovered in 1895 the paradoxical arousal of cold from a cold spot by a warm stimulus, a discovery which was crucial in supporting the theories that cold and warmth have different specific energies, are in fact independent senses. In 1896 Alrutz, working in the Physiological Laboratory at Upsala, having verified von Frey's findings on the independence of the two thermal senses, proposed the theory that the quality heat depends upon the excitation of warm receptors simultaneously with the paradoxical excitation of adjacent cold receptors. Such heat is qualitatively unique, he argued. Coming from the concurrent stimulation of warmth and cold, it occurs under ordinary conditions when stimulus-objects with temperatures above the threshold for paradoxical cold (ca. 45°C.) and below the threshold for pain (ca. 50°C.) are in contact with both cold and warm spots. Using Thunberg's *Temperatoren*, the thin silver plates whose effectiveness depends upon their temperature and mass, he arranged graded series of stimuli

extending from one giving no thermal sensation up to one giving painful heat. On regions that contained both warm and cold spots he got, with this series of stimulators, first warmth, then a new stinging quality which he named *heat*, and finally at the top of the series painful heat. On regions where there were warm spots but no cold spots, warmth passed directly over into pain without heat as an intermediate. On regions where there were cold spots but no warm spots, he got no thermal sensation in the lower regions where he had before got mild warmth, then paradoxical cold, then

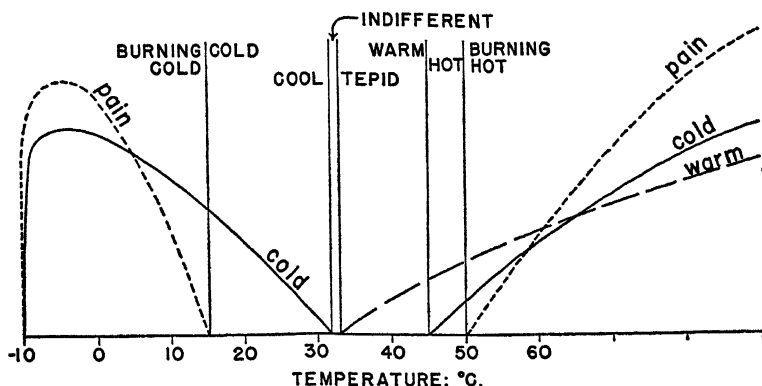


FIG. 84. HEAT AND THE THERMAL SERIES OF SENSATIONS: VON FREY (1895, 1904) AND ALRUTZ (1897)

The curves show the excitation of the receptors for warmth, cold and pain as a function of stimulus temperature. The terms at the top of the diagram show the resulting thermal sensations, with heat as a fusion of warmth and paradoxical cold. The idea is Alrutz's, but the diagram is von Frey's.

pain, but no heat. Thus *heat*, he concluded, is a fusion of warmth and cold, just as gray may be a fusion of blue and yellow, gray being unique and neither bluish nor yellowish. Sometimes, however—so Alrutz reported—the heat can be analyzed introspectively into warmth and cold (especially on the forehead), an imperfect fusion that makes heat resemble more the blending of red and yellow in orange.

Von Frey was happy enough to accept Alrutz's theory which supported so well his own. In 1904 he published the diagram of Fig. 84 showing how the three excitations for warmth, cold and pain would combine to give a thermal series of perceptions: burning cold (freezing), cold, cool, thermal indifference, warmth, heat,

burning heat. This theory, being simple, positive and empirically founded, was generally accepted by the textbooks; among the experts, nevertheless, controversy arose. The controversy has centered upon the question as to whether the heat quality is unique (like

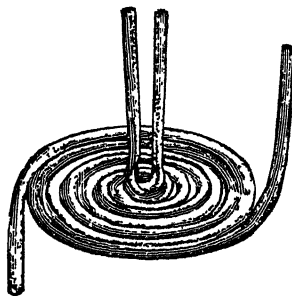


FIG. 85. THUNBERG'S THERMAL GRILL (1896)

Two adjacent metal tubes, each 3 mm. diam., are coiled into a double spiral of 5.5 cm. diam., which is applied to the skin. Cold water is passed through one tube and warm through the other. Although Thunberg reported a resultant cold-warm *Mischempfindung*, the numerous progeny of this apparatus (1918 *et seq.*) are called heat grills, because their constructors accepted Alrutz's theory that warm + cold = heat.

gray) or merely a *Mischempfindung* in which the elements remain introspectively recognizable (like a tonal dyad, *c-g*), and more recently as to whether *heat* is a good word with which to describe the fusion.

Thunberg, before Alrutz published, had built what others later called a heat grill. In 1896 he devised an instrument consisting of two coiled spirals of parallel metal tubes, so arranged that cold water (ca. 24°C.) could be run through one tube, and warm water (ca. 44°C.) through the adjacent tube, to stimulate the same general area of the skin simultaneously with both temperatures. (See Fig. 85.) He got imperfectly the Alrutz phenomenon. He wrote that the perception, when the cold was added to the warmth, was "as if the temperature were suddenly raised and a feeling of 'hot' ensued." Thunberg, however, held that the perception was but a *Mischempfindung* in

which the warmth and cold could still be separately perceived in an imperfect fusion.

Kiesow in 1901 and Hacker in 1913 agreed with Thunberg that the fusion is imperfect. Meanwhile Goldscheider insisted in 1912 that the perception is not what can be called *heat* unless pain is introduced.

At this point in the history both Rubin and Goldscheider, publishing independently in 1912, discovered paradoxical warmth. Rubin reported that cool stimuli just below skin temperature (0.1–1.5°C. below) arouse from warm spots a faint sensation of warmth, whereas Goldscheider reported getting faint warmth from

warm spots stimulated at 6–10°C. below skin temperature. This discovery creates a difficulty with von Frey's diagram of Fig. 84. Heat ought to be felt for a cool areal stimulus. Paradoxical warmth + cold ought to feel the same as warmth + paradoxical cold; paradoxicality refers only to the inadequate mode of stimulation and not to the resultant innervation or quality. This point made no difficulty for Goldscheider, of course, because he believed that heat requires the admixture of pain in any case. On the other hand, Rubin, who accepted Alrutz's theory, suggested that the mild paradoxical warmth might simply inhibit the cold incompletely and thus give rise to the perception *cool*, whereas the paradoxical cold, which is always intense, when mixed with intense warmth gives a different resultant. Inasmuch as there have remained still other doubts about the Alrutz theory, this matter has never been settled.

Next Cutolo at Cornell, the headquarters of introspective psychology in America, undertook in 1918 a careful study of the quality of the warm-cold fusion. After first localizing warm and cold spots he then used a thermesthesiometer, an instrument with two blunt hollow points which can be fixed at a predetermined separation on a bar. Putting cold water (ca. 13°C.) through one point and warm water (ca. 44°C.) through the other, he applied the cold point to a cold spot and the warm point to a warm spot. He also built a heat grill of apposed glass tubes, in which cold water ran through the odd tubes, and warm water through the even. From both these experiments his observers got what was certainly at times a unique experience, for one of them remarked that "it is laughable to think of heat as conditioned upon warmth and cold." The experience was, they said, like pain or a "sting" or a "smack." Since they were agreed that the quality is more like pressure and pain than like warmth and cold, Titchener later assigned heat a place intermediate between pressure and prick in his qualitative continuum of the touches.

Cutolo decided that this kind of heat is not a *Mischempfindung*; nevertheless, even with punctiform stimulation, his observers often reported the simultaneous occurrence of warmth or cold or both along with the heat, the three qualities being interpenetrated in a complex pattern of localizations. In 1930 Ferrall and Dallenbach, using a grill, came to the same conclusion, that the warm-cold fusion is a unique quality in the pressure-prick continuum, somewhat nearer pressure than prick.

There were also at this time at least three other papers which supported the Alrutz theory by getting the observers to identify this kind of heat and then determining some of the properties of its arousal. Alston in 1920, using an esthesiometer like Cutolo's, found that the warm-cold fusion can occur when the stimulated spots are as far apart as 10 to 15 cm. Burnett and Dallenbach in 1928, using a grill, compared the intensity of a heat in one arm with the intensity in the other, and concluded that the intensity of heat varies directly with the quantity $(32-C) + 3(W-33)$, where C is the Centigrade temperature of the cold water in the grill and W the temperature of the warm water. Miss Knight in 1922 showed that warmth, stimulated by radiant heat, and pain, got from a sharpened hair, can fuse to give a burning heat, quite in accordance with the von Frey diagram (Fig. 84).

Until W. L. Jenkins attacked it in 1938, this theory was, therefore, pretty well established. Jenkins had not been able to elicit paradoxical cold as regularly as had previous investigators. Now, working with a large number of untrained subjects, he discovered that *hot* is not an adjective which people ordinarily apply to describe the warm-cold fusion. Among his 126 subjects, when he stimulated them by means of heat grills, he failed to get anyone to describe the experience as hot. When he added electric shock to the warm-cold stimulation, he got frequent reports of heat. By adapting a region to cold, so that paradoxical cold could not be elicited from it, he was still able to get 'heat' if the temperature was high enough to arouse pain.

The final status of the problem is really not so confused as Jenkins' polemics against Alrutz and Dallenbach imply. It is becoming clear that the four tactual modalities are related. Warmth and cold fuse to give a something which in part resembles both pressure and pain. Yet this fusion is often so imperfect that warmth and cold can still be perceived in it or at least in part of the pattern. A heat grill, however, favors analysis because it gives a widespread pattern of warmth, cold and the fusion. When the fusion is poor with a grill you call it a *Mischempfindung*, but at the same time may note, as have hundreds of college students to whom the grills have been demonstrated, the unique and interesting novelty of the fusion. When the fusion is good, you believe in Alrutz, especially if you have been told to call the fusion *heat*.

The rest of the problem lies in vocabulary. Mildly hot objects

can feel only warm. Hotter objects may arouse paradoxical cold with warmth, and small wonder is it if some persons learn to call the resultant perceptual fusion heat. The object is actually fairly hot (45° – 50° C., say). If the temperature is above this narrow range, pain is elicited; since obviously that object is hot too, everyone calls it *hot*. If Alrutz had been content to call the fusion *stick*, let us say, then he never would have had Jenkins upon his back, and he would have been quite free to add that the stimulus to the 'sticking' quality may be a fairly hot object that stimulates both warm and cold spots, and is even sometimes called *heat*. The words that people use about perceptual fusions and patterns are something different from the facts of sensory fusion.

Touch Blends

The perceptual world of touch presents us with many stable fusions, blends, spatial patterns and temporal modes of change, all complexes which represent special objects, like metal or cloth, or special secondary qualities, like roughness, hardness or stickiness. Heat and burning heat, as we have studied them in the last section, are such perceptions. So also are the organic patterns, like hunger, suffocation and effort, which we shall consider in the next chapter. Table XIII lists, in addition to four thermal complexes, nine of these cutaneous blends and patterns which have been subject to experimental analysis. These perceptions are, of course, the modern descendants of the contents of Weber's old *Gemeingefühl*, now at last brought under analysis into the elements of the *Tastsinn*.

This particular series of studies, undertaken almost entirely at Cornell, starts off with Bentley's synthetic experiment for the perception of *wet*. It is easy in most of these blends (except perhaps in the fusion for *hot*; *vide supra*) to note introspectively the involvement or absence of the classical tetrad: pressure, pain, warmth and cold. It is also easy to apply common knowledge of the nature of the objects perceived in specifying spatial, temporal and intensive characteristics for the stimulus. Hard objects ought to yield more pressure than soft; wet objects should be moist. Such an analysis may, however, be in error. Not only is the mere listing of qualitative components never enough, but the analyst's attention may go to the wrong aspects of the stimulus. Thus a synthetic check upon analysis is needed. To make this point Bentley undertook in

1900 to synthetize *wet*. Introspection analyzes *wet* into pressure and cold. Bentley asked: can *wet* be synthetized merely from a pressure and a cold? Trying out a great variety of cold, warm and thermally indifferent pressures, he came to the conclusion that a pressure, to be perceived as wet, must always be cold, and that it must also be as uniformly distributed over an area as is the actual pressure from a liquid. Cold cotton does not give a very convincing *wet*, but cold liquid does. At first it seems like begging the question, to say that liquids feel wet. Of course they do. Bentley's contribution lay, however, in his discovery that a dry cold liquid feels just as wet as a moist cold liquid; moisture is not essential to the perception of *wet*. His dry liquids Bentley got by covering the finger with a tight-fitting thin rubber membrane or with lycopodium powder, and then immersing it in cold water. If moisture were essential to wetness, then the membrane or the powder, since they keep the finger dry, would prevent the perception.

Except for Miss Murray's analysis of tickle (1908) into an unstable, spreading pattern of light pressures plus unpleasantness and the kinesthesia of the reflex tendency for withdrawal, Bentley's experiment was not followed up until America felt the influence of the Germans' concern with perception—that concern which resulted in the Gestalt school. Then in 1922–1927 Titchener, facing the new perceptual problems and convinced that the old four conventional qualities must still constitute the bases of description, put his students—for the most part Miss Sullivan and Zigler—on to the synthetizing of seven of the perceptions listed in Table XIII. It is impossible to retail here the various ingenious devices which they contrived for controlling the stimuli in order to duplicate these perceptions. The table shows the gross analyses that resulted. We may mention a few special matters.

Tung (1921), Bershansky (1923) and Sullivan (1923) all confirmed Bentley on the finding that wet is pressure and cold. Miss Sullivan, studying especially the series from liquidity to solidity, argued that temperature is most important at the liquid end and pressure at the solid end. The nature of this series she indicated by listing twelve points on it: vaporous, wet, oily, gelatinous, slimy, greasy, muddy, mushy, soggy, doughy, spongy, dry—a list of adjectives which indicates how great may be the task of phenomenologizing the *Gemeingefühl*.

The movement of the stimulus in relation to the skin, either ac-

TABLE XIII. TOUCH BLENDS

The table indicates (as far as a table can) the analysis of thirteen cutaneous perceptions. The abbreviations correspond to the headings at the tops of the columns

<i>Perception</i>	<i>Author</i>	<i>Date</i>	<i>Pressure</i>	<i>Pain</i>	<i>Warmth</i>	<i>Cold</i>	<i>Movement</i>	<i>Other characteristics</i>
1. Hot (cf. 2)	Alrutz	1897	—	—	W strong	C strong		
2. Cool (cf. 1)	Rubin	1912	—	—	W weak	C weak		
3. Burning hot	Knight	1922	—	Pn	W strong	C?		
4. Burning cold	von Frey	1904	—	Pn	—	C strong		
5. Wet (cf. 6)	Bentley	1900	Pr. even	—	—	C		
6. Oily (cf. 5)	Cobbey and Sullivan	1922	Pr. weak	—	W	—	M?	
7. Hard (cf. 8)	Sullivan	1927	Pr. even	—	—	C		good boundary
8. Soft (cf. 7)	Sullivan	1927	Pr. uneven	—	W	—		poor boundary
9. Smooth (cf. 10)	Meenes and Zigler	1923	Pr. even field	—	—	—	M	
10. Rough (cf. 9)	Meenes and Zigler	1923	Pr. uneven field	—	—	—	M	
11. Sticky (cf. 10)	Zigler	1923	Pr. variable	—	—	—	M	tend. to withdraw; unpleasant
12. Ticklish (cf. 11)	Murray	1908	Pr. variable, spreads	—	—	—	M reflex	
13. Clammy (cf. 8)	Zigler	1923 ^a	Pr. uneven	—	—	C	M	poor boundary; unpleasant imagery

tively or passively, is important. A hard object (Sullivan) is not smooth to the finger, unless it moves in relation to the finger (Meenes and Zigler). Roughness, stickiness and probably oiliness also require movement.

The table is, however, almost a caricature of these studies because its perceptions are not simple qualitative blends, but complex temporal-spatial patterns. Take clamminess (Zigler). It is essentially a cold softness perceived with movement and supplemented by unpleasant imagery. An objective attitude by the perceiver kills the imagery and thus the perception. Sullivan, however, had found that softness without movement involves warmth. Is there thus warmth as well as cold in clamminess, and would such warmth and cold make heat? (Cf. items 8 and 13 in the table.) Seemingly not. The warmth becomes unnecessary to softness when these other factors are introduced.

There would seem to be no question about the scientific importance of this type of perceptual analysis, for if all tactual perceptions can be reduced to spatial-temporal patterns of the four basic qualities, then there is no need to look for more than four kinds of cutaneous receptors or more than four afferent systems. Nevertheless, the job seems now to have been pushed far enough. Since Titchener's death in 1927, interest in the reduction of the many kinds of objective perceptions to their basic afferent components has almost disappeared, and the phenomenologists of the Gestalt school have taken over. They have published two elaborate accounts of the phenomenology of touch; one in 1925 is by Katz and another in 1938 by Révész. Katz, consistent with his earlier work on color (1911), distinguished the modes of appearance of the touches: surface touches, unlocalized touches (analogous to film colors), volumic touches, and the mediately perceived surfaces (like the paper at the end of the pencil). (For the analogous phenomenology of color, see pp. 135, 255 f.) Révész's phenomenology, drawing especially upon the space perceptions of the blind, goes broader and includes all of somesthesia. The importance of such phenomenology only the future can decide. It was many years before Purkinje's comparable descriptions of visual phenomena became the basis of important chapters in the psychology of vision.

Notes

Only recently has there been available a thorough handbook for the tactual sense. E. von Skramlik, *Psychophysiologie der Tastsinne*, *Arch. ges. Psychol.*, Ergbd. 4, 1937, 935 pp. (1-510 for the skin). There are a number of good chapters in earlier handbooks, of which the two best (as of their dates) are C. S. Sherrington in E. A. Schäfer's *Text-Book of Physiology*, 1900, II, 920-1001, and T. Thunberg in W. Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 647-733. A good, briefer but more recent, English chapter is in L. Luciani, *Human Physiology*, IV, trans. 1917, 1-56.

Tactual Differentiation

The pre-experimental and early experimental history of tactual differentiation has been given by K. M. Dallenbach, *Pain: history and present status*, *Amer. J. Psychol.*, 52, 1939, 331-347.

The experiments of E. H. Weber begin in his *De pulsu, resorptione, auditu et tactu: annotationes anatomicae et physiologiae*, 1834, esp. 44-175 (*De tactu*), but the classical paper which drew attention and exerted influence was his *Der Tastsinn und das Gemeingefühl*, a chapter in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 481-588 (reprinted separately in 1851 and 1905). As to what Weber meant by *Sinn*, we may note the opening sentence of his later paper on the two-point threshold and the error of localization, a paper in which he had substituted the term *Raumsinn* for *Ortsinn*; Ueber den Raumsinn und die Empfindungskreise in der Haut und im Auge, *Ber. sächs. Ges. Wiss. Leipzig*, math.-phys. Cl., 1852, 85-164. The sentence reads: "Der Raumsinn ist ein besondere Sinn, aber nicht ein Specialsinn, sondern

ein Generalsinn." It is a distinct general capacity, not a special sense.

Two papers that, following Weber, argued for the identity of the pressure and temperature senses, are A. Wunderli (an address paraphrased by A. Fick), *Experimentelle Beiträge zur Physiologie des Tastsinnes*, [Moleschott's] *Untersuchungen zur Naturlehre des Menschen und der Thiere*, 7, 1860, 393-400, esp. 394-396; M. Szabadföldi, *Beiträge zur Physiologie des Tastsinnes*, *ibid.*, 9, 1865, 624-631, esp. 625 f. J. M. Schiff, *Lehrbuch der Physiologie des Menschen*, I, 1858, 237-262, gave an elaborate discussion of conduction of pressure and pain in the spinal cord, concluding that the tracts are separate. The first authoritative support for the separation of pressure and temperature was agreed upon by Funke and Hering in their related chapters in Hermann's *Handbuch*: O. Funke, *Der Tastsinn und die Gemeingefühle*, in L. Hermann's *Handbuch der Physiologie*, III, ii, 1880, 298-414; E. Hering, *Der Temperatursinn*, *ibid.*, 415-439. Hering's chapter contains his theory of warmth and cold as dependent upon antagonistic processes, analogous to his color theory. A. Herzen, *Ueber die Spaltung des Temperatursinnes in zwei gesonderte Sinne*, *Arch. ges. Psychol.*, 38, 1885, 93-103, esp. 94, argued for the independence of warmth and cold, presumably without knowing of Blix's and Goldscheider's finding of separate spots.

The original papers of M. Blix are *Experimentala bidrag till lösning af fragan om hudnervernas specifika energier*, *Uppsala Lakfören. Förh.*, 1 ser., 18, 1882, 87-102; 18, 1883, 427-440. The first paper was published as of October, 1882 in the 1882-1883 vol., and is generally incorrectly given as 1883, because the

volume generally bears that date. This date demonstrates Blix's priority, for his first short paper was published in October, 1882, and Goldscheider's first short paper in July, 1884. Blix failed to get discovered, of course, until German translations of his papers were published in 1884, so that in German he and Goldscheider were almost synchronous. The translations of Blix are *Experimentelle Beiträge zur Lösung der Frage über die spezifische Energie des Hautnerven*, *Z. Biol.*, 20, 1884, 141-156; 21, 1885, 145-160. The first paper declares for four modalities and four kinds of spots; the second rejects pain spots and pain as a modality.

The initial papers of A. Goldscheider are *Die spezifische Energie der Temperaturnerven*, *Monatshefte prak. Dermatol.*, 3, 1884, 198-208, 225-241; *Die spezifische Energie der Gefühlsnerven der Haut*, *ibid.*, 3, 1884, 283-300; *Ueber Wärme-, Kalte- und Druckpunkte*, *Verhandl. physiol. Gesell. Berlin*, 10, 1885, 21-26. These are respectively reprinted in his *Gesammelte Abhandlungen*, 1898, I, 53-76, 77-93, 100-106. Goldscheider acknowledges Blix's priority, of course. In these papers he was against pain as a fourth modality, but in his full account he accepted it: *Neue That-sachen über die Hautsinnesnerven*, *Arch. Physiol. Leipzig*, 1885, Suppl.-Bd., 1-110 (*Ges. Abhandl.*, I, 107-218). His final view for pain as a summation of intensive sensations appears in J. Gad and Goldscheider, *Ueber die Summation von Hautreizen*, *Z. klin. Med.*, 20, 1892, 339-373 (*Ges. Abhandl.*, I, 397-432), and in Goldscheider, *Ueber den Schmerz in physiologischer und klinischer Hinsicht*, 1894, esp. 3-36, for the rest is clinical (not in *Ges. Abhandl.*). In this connection, see a later review of the literature and ultimate defense of the intensive theory of pain: A. J. McKeag, *The*

Sensation of Pain and the Theory of Specific Sense Energies, 1902, K. M. Dallenbach, *Pain: history and present status*, *Amer. J. Psychol.*, 52, 1939, 331-347.

The third independent discoverer of temperature spots is H. H. Donaldson, *On the temperature sense*, *Mind*, 10, 1885, 399-416. He was working under G. Stanley Hall in America's first founded psychological laboratory at Johns Hopkins.

M. von Frey's four classical papers are: *Beiträge zur Physiologie des Schmerzsinns*, *Ber. sachs. Ges. Wiss., math.-phys. Cl.*, 46, 1894, 185-196, 283-296; *Beiträge zur Sinnesphysiologie der Haut*, *ibid.*, 47, 1895, 166-184; *Untersuchungen über die Sinnesfunktionen der menschlichen Haut; Druckempfindung und Schmerz*, *Abhandl. sächs. Ges. Wiss., math.-phys. Cl.*, 23, 1896, 175-266. The first two papers (1894) insist on pain spots; the third (1895) is the argument for four organs for four modalities (paradoxical cold, pp. 172 f.); the fourth (1896) is on the pressure and pain limens.

The experimenters who excised warm and cold spots and found nerve fibers and free endings, but no corpuscles or special endings, are Donaldson, *op. cit.*, 1885, 408 f.; A. Goldscheider, *Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen*, *Arch. Physiol. Leipzig*, 1886, Suppl.-Bd., 191-231 (reprinted in *Gesammelte Abhandlungen*, 1898, I, 219-249); G. Häggqvist, *Histophysiologische Studien über die Temperatursinne der Haut des Menschen*, *Anat. Anz.*, 45, 1914, 46-63, esp. 57; K. M. Dallenbach, *The temperature spots and end-organs*, *Amer. J. Psychol.*, 39, 1927, 402-427; C. R. Pendleton, *The cold receptor*, *ibid.*, 40, 1928, 353-371. Dallenbach, whose enthusiasm is responsible for the rediscovery of this 'lost' paper of Goldscheider's, dis-

cusses this matter in A bibliography of the attempts to identify the functional end-organs of cold and warmth, *ibid.*, 41, 1929, 344.

Sherren cut the two nerves in Henry Head's arm on April 25, 1903. The papers are H. Head, W. H. R. Rivers and J. Sherren, The afferent nervous system from a new aspect, *Brain*, 28, 1905, 99-116, reprinted in Head, *Studies in Neurology*, 1920, I, 55-65, but without the plates; Rivers and Head, A human experiment in nerve-division, *Brain*, 31, 1908, 323-450 (*Studies in Neurology*, I, 225-329). The later paper is the thorough account; the first establishes the date. In general, see *Studies in Neurology*, I, for these and for a long clinical paper by Head and Sherren, and II, 810-832, for references to Head's critics, including von Frey, and for Head's trenchant criticism of his critics. The first two experimental papers, which verified Head's most important findings and rejected his interpretation of them, are W. Trotter and H. M. Davies, Experimental studies in the innervation of the skin, *J. Physiol.*, 38, 1909, 134-146; E. G. Boring, Cutaneous sensation after nerve-division, *Quart. J. exper. Physiol.*, 10, 1916, 1-95. On how the discrimination of two points may be mediated by deep sensibility, see Boring, The relation of the limen of dual impression to Head's theory of cutaneous sensibility, *Proc. VIII Internat. Congr. Psychol.*, 1923, 57-62.

Space Perception

The question of tactual space perception in connection with nativism and empiricism and Lotze's theory of local signs has already been discussed, in Chapters 1, 2 and 7 (pp. 29-31, 79-82, 226-228, 234-236). Thanks to Weber, the study of tactual space came early, before the study of tactual qualities.

Two-Point Limen

The classical exposition and standard secondary source for the work in the nineteenth century on both the two-point limen and the error of localization is V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898. Much less general and on the two-point limen only is M. Foucault, *L'illusion paradoxale et le seuil de Weber*, 1910.

For E. H. Weber on the *Ortsinn*, see his *opp. citt.*: *De pulsu, resorptione, auditu et tactu*, 1834, 45-81; Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 524-543; also, for his "second method" and the further discussion of sensory circles, *Ueber den Raumsinn und die Empfindungskreise in der Haut und im Auge*, *Ber. sächs. Ges. Wiss., math.-phys. Cl.*, 1852, 85-164.

For the first systematic work with successive stimulation and the evidence that the limen grows larger with the growth of the organism, see J. N. Czermak, *Beiträge der Physiologie des Tastsinnes, Sitzungsber. Akad. Wiss. Wein., math.-phys. Cl.*, 15, 1855, 466-521; reprinted in his *Gesammelte Schriften*, 1879, I, 303-353.

K. Vierordt discussed longitudinal vs. transverse limens and first mentioned the law of mobility in *Ueber die Ursachen der verschiedenen Entwicklung des Ortsinnes der Haut*, *Arch. ges. Physiol.*, 2, 1869, 297-306. He developed the law of mobility in *Die Abhängigkeit der Ausbildung des Raumsinnes der Haut von den Beweglichkeit der Korpertheile*, *Z. Biol.*, 6, 1870, 53-72.

For A. Goldscheider's small limens, see his *Neue Thatsachen über die Hautsinnesnerven*, *Arch. Physiol. Leipzig*, 1885, Suppl.-Bd., 84-87, reprinted in his *Gesammelte Abhandlungen*, 1898, I, 194-196. For the spatial discrimination of pressure spots by successive stimulation,

see M. von Frey and R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Z. Psychol.*, 29, 1902, 161-182.

On the paradoxical twos (*Vexirfehler*) see R. Kottenkamp and H. Ullrich, Versuche über den Raumsinn der Haut der oberen Extremität, *Z. Biol.*, 6, 1870, 37-52; W. Camerer, Versuche über den Raumsinn der Haut nach der Methode der richtigen und falschen Fälle, *ibid.*, 19, 1883, 280-300; V. Henri and G. A. Tawney, Ueber die Trugwahrnehmung zweier Punkte bei der Berührung eines Punktes der Haut, *Phil. Stud.*, 11, 1895, 394-405; M. Foucault, *L'illusion paradoxale et le seuil de Weber*, 1910, 13-118 (history of paradoxical judgment, 13-28); M. Kincaid, An analysis of the psychometric function for the two-point limen with respect to the paradoxical error, *Amer. J. Psychol.*, 29, 1918, 227-232; E. G. Boring, Cutaneous sensation after nerve-division, *Quart. J. exper. Physiol.*, 1-95, esp. 93.

On reduction of the limen with practice, see A. W. Volkmann, Ueber den Einfluss der Uebung auf das Erkennen räumlicher Distanzen, *Ber. sächs. Ges. Wiss., math.-phys. Cl.*, 10, 1858, 38-69; W. Camerer, Versuche über den Raumsinn der Haut bei Kindern, *Z. Biol.*, 17, 1881, 1-22; G. A. Tawney, Ueber die Wahrnehmung zweier Punkte mittelst des Tastsinnes, mit Rücksicht auf die Frage der Uebung und die Entstehung der Vexirfehler, *Phil. Stud.*, 13, 1897, 163-221; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 28-36; Friedline, *op. cit. infra*, esp. 413-415.

On the increase of the limen with fatigue, see H. Griesbach, Ueber Beziehungen zwischen geistiger Ermüdung und Empfindungsvermögen der Haut, *Arch. Hygiene München*, 24, 1895, 124-212; T. Vannod, La fatigue intellectuelle et

son influence sur la sensibilité cutanée, *Rev. méd. suisse Romande*, 16, 1896, 712-751; Henri, *op. cit.*, 33-35; Friedline, *op. cit. infra*, esp. 415-419.

On savage and civilized limens, see W. McDougall, *Reports of Cambridge Anthropological Expedition to Torres Straits*, II, ii, 1903, 189-193.

On sensory categories under dual stimulation, see Kottenkamp and Ullrich, *op. cit.*; Camerer, *op. cit.*, 1883, 282; Foucault, *op. cit.*, 124 f.; E. J. Gates, The determination of the limens of single and dual impression by the method of constant stimuli, *Amer. J. Psychol.*, 26, 1915, 152-157. On the utilization of different sensory categories as the criterion for duality, see E. B. Titchener, On ethnological tests of sensation and perception with special reference to the tests of color vision and tactile discrimination described in the reports of the Cambridge Anthropological Expedition to Torres Straits, *Proc. Amer. phil. Soc.*, 55, 1916, 204-236, esp. 206-215; C. L. Friedline, Discrimination of cutaneous patterns below the two-point limen, *Amer. J. Psychol.*, 29, 1918, 400-419; E. G. Boring, The stimulus-error, *ibid.*, 32, 1921, 449-471, esp. 465-470.

Error of Localization

For a thorough discussion of the research on the error of localization, see V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 90-141.

This is E. H. Weber's "second method," and it is given briefly with the results in his Ueber den Raumsinn und die Empfindungskreise in der Haut und im Auge, *Ber. sächs. Ges. Wiss., math.-phys. Cl.*, 1852, 85-164, esp. 87-89. The fact is that A. W. Volkmann, in his investigation of vision, had used a form of the method earlier in order to study

the relation of tactual to visual localization: see his discussion in R. Wagner's *Handwörterbuch der Physiologie*, II, 1844, 570-572.

Weber's first method and the concept of sensory circles became known in 1846. Weber's second method and his further discussion of the *Raumsinn* were published in 1852, which was also the year in which R. Lotze, *Medicinische Psychologie*, 1852, 325-435, put out the theory of local signs. Lotze discussed the relation of sensory circles to local signs, 395-410, and Czermak, *op. cit. infra*, 1855, was largely concerned with bringing Weber and Lotze into relationship. See pp. 233-237 on the empiricistic and nativistic theories of space which grew out of Lotze's original genetic theory.

The other important papers cited are: H. Aubert and A. Kammler, *Untersuchungen über den Druck- und Raumsinn der Haut*, [Moleschott's] *Untersuchungen zur Naturlehre des Menschen und der Thiere*, 5, 1858, 145-179, esp. 176-179; V. Henri, *Récherches sur la localisation des sensations*, *Arch. Physiol. norm. pathol.*, 5 ser., 15, 1893, 619-627; *Récherches sur la localisation des sensations tactiles*, *Année psychol.*, 2, 1895, 168-192; W. Barth, a Russian paper, 1894, [n.v.], which Henri cites and translates as *Études de la sens du lieu et sur la mémoire de ce sens*; W. B. Pillsbury, *Some questions of cutaneous sensibility*, *Amer. J. Psychol.*, 7, 1895, 42-57; C. S. Parrish, *Localization of cutaneous impressions by arm movement without pressure upon the skin*, *ibid.*, 8, 1897, 250-267.

On the argument that the error of localization would necessarily be a third or a quarter of the two-point limen because of the relation of the methods, see J. N. Czermak, *Weitere Beiträge zur Physiologie des Tastsinnes*, *Sitzungsber. Akad. Wiss.*

Wein, math.-naturwiss. Cl., 17, 1855, 577-600, esp. 582-584, reprinted in his *Gesammelte Schriften*, 1879, I, 370-391, esp. 374-376; E. G. Boring, *The two-point limen and the error of localization*, *Amer. J. Psychol.*, 42, 1930, 446-449; M. J. Zigler, *The experimental relation of the two-point limen to the error of localization*, *J. general Psychol.*, 13, 1935, 316-332.

On the displacement of localization toward points of orientation, see Pillsbury, Parrish, *op. cit.*; W. Lewy, *Experimentelle Untersuchungen über das Gedächtnis*, *Z. Psychol.*, 8, 1895, 231-292, esp. 254-292, and on this particular point, 277-281; M. Ponzio, *Récherches sur la localisation des sensations tactiles et des sensations dolorifiques*, *Arch. ital. Biol.*, 55, 1911, 1-14.

For the fact that the error of localization changes with modality, getting larger for pain, pressure, cold and warmth (in that order), see Ponzio, *Étude de la localisation des sensations thermiques de chaud et de froid*, *Arch. ital. Biol.*, 60, 1913, 218-231.

Stimulation

For G. Meissner on cutaneous deformation as the stimulus to touch, see his *Untersuchungen über den Tastsinn*, *Z. rat. Med.*, 3 ser., 7, 1859, 92-118. For M. von Frey on the pressure stimulus, see his *Untersuchungen über die Sinnesfunktionen der menschlichen Haut: Druckempfindung und Schmerz*, *Abhandl. sachs. Gesell. Wiss.*, math.-phys. Cl., 23, 1896, 175-266, esp. 223-237 (specifications for three equivalent hairs, 229 f.); *Beiträge zur Sinnesphysiologie der Haut*, *Ber. sachs. Gesell. Wiss.*, math.-phys. Cl., 49, 1897, 462-468; and von Frey and F. Kiesow, *Ueber die Function der Tastkörperchen*, *Z. Psychol.*, 20, 1899, 126-163, esp. 143-155. The

last article is the fullest and the one that all the texts cite.

The term *pressure-gradient* (*Druckgefälle*) was first used by von Frey and Kiesow, *op. cit.*, 150-155, although they had more to say about deformation and the consequent intracutaneous tension. The theory is that a pressure-gradient produces deformation which in turn produces tension, and texts in citing von Frey on the pressure stimulus have sometimes spoken of the *pressure-gradient*, sometimes of *deformation*, but infrequently of *tension*.

On the nature of the thermal stimulus, see John Locke, *An Essay Concerning Human Understanding*, 1690, Bk. 2, Chap. 8, sect. 21 (which should be taken in context with the discussion of secondary qualities in general, Chap. 8 entire); E. H. Weber, *Temperatursinn*, in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 100-110, esp. 100 f.; K. Vierordt, *Grundriss der Physiologie*, 2 ed., 1862, 241 f. (where he accepts Weber's view); 4 ed., 1871, 301 f. (where he objects to Weber's view and gives his own); E. Hering, *Grundzüge einer Theorie des Temperatursinns*, *Sitzungsber. Akad. Wiss. Wien*, math.-naturwiss. Cl., 75 (3), 1877, 101-135 (the thorough paper); *Der Temperatursinn*, in L. Hermann's *Handbuch der Physiologie*, III, ii, 1879, 415-439 (briefer and more conventional); C. S. Sherrington, in E. A. Schäfer's *Text-Book of Physiology*, 1900, II, 950-954.

On paradoxical cold, see M. von Frey, *Beiträge zur Sinnesphysiologie der Haut*, *Ber. sächs. Ges. Wiss.*, math.-phys. Cl., 47, 1895, 155-184. On the verification of this phenomenon by five other investigators, see L. Luciani, *Human Physiology*, IV, 1917, 33; but Jenkins failed with naive observers to get confirmation in most cases: W. L. Jenkins, *Adaptation in isolated cold spots*, *Amer.*

J. Psychol., 49, 1937, 1-22, esp. 13-15; *Studies in thermal sensitivity: minor contributions*, *J. exper. Psychol.*, 22, 1938, 178-185, esp. 178 f.

W. L. Jenkins' theory of the reversible reaction in the receptor is in his *Studies in thermal sensitivity: adaptation with a series of small annular stimulators*, *J. exper. Psychol.*, 22, 1938, 164-177, esp. 172-177. For Nafe's vascular theory of thermal sensation, see J. P. Nafe, *Handbook of General Experimental Psychology*, 1934, 1047-1049, 1056-1058; Nafe and K. S. Wagoner, *The experiences of warmth, cold and heat*, *J. Psychol.*, 2, 1936, 421-431.

For M. von Frey on pain, see his first two *opp. citt.*, esp. the second (1895), in the early section of this chapter's notes, *Tactual Differentiation*. On the use of radiant heat to elicit pain, see J. D. Hardy, H. G. Wolff and H. Goodale, *Studies on pain: a new method for measuring pain threshold; observations on spatial summation of pain*, *J. clin. Invest.*, 19, 1940, 649-657; Wolff, Hardy and Goodale, *Studies on pain: measurement of the effect of morphine, codeine and other opiates on the pain threshold and analysis of their relation to the pain experience*, *ibid.*, 19, 1940, 659-680; G. A. Schumacher, Goodale, Hardy and Wolff, *Uniformity of the pain threshold in man*, *Science*, 92, 1940, 110-112.

• Weber Function

For Weber on the Weber fraction, see E. H. Weber, *De pulsu, etc.* (*op. cit.*), 1834, 86-92, 132-142, 159-161; *Der Tastsinn und das Gemeingefühl*, in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 546-549 (96-100 in the 1905 reprint).

For the early determination of the Weber function for what was really deep pressure, see the data of W. Biedermann and Löwit, published

by E. Hering, *Zur Lehre von der Beziehung zwischen Leib und Seele, Sitzungsber. Akad. Wiss. Wein, math.-naturwiss. Cl.*, 72 (3), 1875, 310-348, esp. 342-345. The text does not mention Stratton's classical paper for the reason that he used weights varying from 10 to 200 grm. and was thus really determining a function for deep pressure. Working in Wundt's laboratory at Leipzig, he invented, for the controlled successive application of two pressures, the *Druckwaage* that in finished form is seen in so many of the older laboratories. See G. M. Stratton, *Ueber die Wahrnehmung von Druckänderungen bei verschiedenen Geschwindigkeiten, Phil. Stud.*, 12, 1896, 525-586, esp. 535-538 for the tables.

For the Weber function as determined with von Frey hairs on isolated pressure spots, see F. Kiewow, *Ueber die taktile Unterschiedsempfindlichkeit bei sukzessiver Reizung einzelner Empfindungsorgane, Arch. ges. Psychol.*, 43, 1922, 11-23 (p. 16 for the data of Table XII); *Zur Frage nach der Gültigkeit des Weberschen Gesetzes im Gebiete der Tastempfindung, ibid.*, 47, 1924, 1-13; A. Gatti and R. Dodge, *Ueber die Unterschiedsempfindlichkeit bei der Reizung eines einzelnen isolierten Tastorgans, ibid.*, 69, 1929, 405-425 (p. 406 for the data in the table). For a comparison of these functions with others from other departments of sense, see A. H. Holway and C. C. Pratt, *The Weber-ratio for intensive discrimination, Psychol. Rev.*, 43, 1936, 322-340.

On the Weber function for temperature, see G. T. Fechner, *Elemente der Psychophysik*, 1860, I, 201-210 (the experiments were performed in 1855); H. Nothnagel, *Beiträge zur Physiologie und Pathologie des Temperatursinns, Deutsch. Arch. klin. Med.*, 2, 1867, 284-299, esp. 287; E. Abbott, *The effect of*

adaptation on the temperature difference limen, Psychol. Monogr., 16, 1914, no. 2.

Summation

On summation of thermal stimulation, see E. H. Weber, *Der Tastsinn und das Gemeingefühl (op. cit.)*, 1846, 553 f.; S. E. Barnholdt and M. Bentley, *Thermal intensity and the area of the stimulus, Amer. J. Psychol.*, 22, 1911, 325-332; J. D. Hardy and G. W. Oppel, *Studies in temperature sensation: the sensitivity of the body to heat and the spatial summation of the end organ responses, J. clin. Invest.*, 16, 1937, 533-540.

On pain as summation, see the notes on Tactual Differentiation, *supra*. See also J. D. Hardy, H. G. Wolff and H. Goodale, *Studies on pain: a new method for measuring pain threshold; observations on spatial summation of pain, J. clin. Invest.*, 19, 1940, 649-657.

For summation of adjacent pressures and inhibition of somewhat more remote pressures, see A. Brückner, *Die Raumschwelle bei Simultanreizung, Z. Psychol.*, 25, 1901, 33-60, esp. 38-47.

Adaptation

On thermal adaptation, *vide supra* under Stimulation and Weber's Law; and for the history of experimentation on it, see E. Gertz, *Psychophysische Untersuchungen über die Adaptation im Gebiet der Temperatursinn und über ihren Einfluss auf die Reiz- und Unterschiedsschwellen, Z. Sinnesphysiol.*, 52, 1921, 1-51, 105-156, esp. 1-18; also Abbott, *op. cit.* (in the preceding section), 1-7. For the papers cited in the text, besides Gertz and Abbott, see W. L. Jenkins, *Adaptation in isolated cold spots, Amer. J. Psychol.*, 49, 1937, 1-22.

On adaptation* to pressure, see M.

von Frey, Untersuchungen über die Sinnesfunctionen der menschlichen Haut, *Abhandl. sächs. Gesell. Wiss., math.-phys. Cl.*, 23, 1896, 175-266, esp. 177-188 (Die Wahrnehmung andauernder Belastung); F. Kiesow, Sur l'excitation du sens de pression produite par des déformations constantes de la peau, *Arch. ital. Biol.*, 26, 1896, 417-442; M. J. Zigler, Pressure adaptation-time: a function of intensity and extensity, *Amer. J. Psychol.*, 44, 1932, 709-720.

On adaptation to pain, see E. Murray, A qualitative analysis of tickling, *Amer. J. Psychol.*, 19, 1908, 289-344, esp. 304 f.; H. H. Strauss and R. F. Uhlmann, Adaptation to superficial pain, *ibid.*, 30, 1919, 422-424.

After-Sensations

An excellent history of the experimental psychology of after-sensations is given by M. H. S. Hayes, A study of cutaneous after-sensations, *Psychol. Monogr.*, 14, 1912, No. 3, 1-29 (bibliography of 39 titles, 87-89). The important experiments mentioned in the text are: R. Kottenkamp and H. Ullrich, Versuche über den Raumsinn der Haut der oberen Extremität, *Z. Biol.*, 6, 1870, 37-52, esp. 41; V. Urbantschitsch, Ueber den Einfluss von Trigemini-Reizen auf den Tast- und Temperatursinn der Gesichtshaut, *Arch. ges. Physiol.*, 41, 1887, 46-69, esp. 61, 65 f.; M. Dessoir, Ueber den Hautsinn, *Arch. Physiol. Leipzig*, 1892, 175-339, esp. 195; J. Gad and A. Goldscheider, Ueber die Summation von Hauteizen, *Z. klin. Med.*, 20, 1892, 339-373 (in Goldscheider's *Gesammelte Abhandlungen*, 1898, I, 397-432); T. Thunberg, Untersuchungen über die bei einer einzelnen momentanen Hauteizung auftretenden zwei stechenden Empfindungen, *Skand. Arch. Physiol.*, 12, 1902, 394-442, esp. 432-438; Urbantschitsch, Ueber Sinnesempfind-

ungen und Gedächtnisbilder, *Arch. ges. Physiol.*, 110, 1905, 437-491, esp. 442-471; Hayes, *op. cit.*; F. L. Dimmick, On cutaneous after-images, *Amer. J. Psychol.*, 27, 1916, 566-571; R. T. Holland, On the 'after-sensation' of pressure, *J. exper. Psychol.*, 3, 1920, 302-318.

Heat

For von Frey on paradoxical cold, see the notes on Tactual Differentiation, *supra*.

S. Alrutz first published a paper confirming von Frey on the independent specificity of the warm and cold spots (he began his research in April, 1896): Bidrag till kännedom om hudens kall- och varmpunkter, *Uppsala Läkforen, Forh.*, 2 ser., 2, 1896, 246-263; German trans., Zu den Kälte- und Wärmepunkten, *Skand. Arch. Physiol.*, 7, 1897, 321-341; Eng. abstract, Experimental contributions towards our knowledge of the cold- and warm-spots of the skin, *Mind*, 2 ser., 6, 1897, 445-448. His theory of heat is in his Om fornimmelsen "hett," *Uppsala Läkforen. Forh.*, 2 ser., 2, 1897, 340-359; German trans., Die Hitzeempfindung, *Skand. Arch. Physiol.*, 10, 1900, 340-352; Eng. abstract, The sensation "hot," *Mind*, 2 ser., 7, 1898, 140-144. For the diagram of the thermal series of perceptions (Fig. 84), see M. von Frey, *Vorlesungen über Physiologie*, 1904, 313.

T. Thunberg anticipated Alrutz in the study of the effects of warm-cold stimulation: Förmimmelserne vid till samma ställe lokaliserad, samtidigt pågående köld- och värmeretning, *Uppsala Läkforen. Förh.*, 2 ser., 1, 1896, 489-495 (the picture of the grill is on p. 489).

Alrutz tried to discover paradoxical warmth and failed. It was discovered independently by E. Rubin, Beobachtungen über Temperaturempfindungen, *Z. Sinnesphysiol.*, 46,

1912, 388-393, esp. 392 f., and by A. Goldscheider, Ueber die Empfindung der Hitze, *Z. klin. Med.*, 75, 1912, 1-14, esp. 4 f.; Revision der Lehre vom Temperatursinn, *Ber. V. Kongr. exp. Psychol.*, 1912, 222-228, esp. 227 f.

On heat as an imperfect fusion of warmth and cold, see F. Kiesow, Zur Analyse der Temperaturempfindung, *Z. Psychol.*, 26, 1901, 230-246; F. Hacker, Beobachtungen an einer Hautstelle mit dissoziierter Empfindungslähmung, *Z. Biol.*, 61, 1913, 231-263, esp. 239-253. On heat as necessarily involving pain, see Goldscheider, *op. cit.*, *Z. klin. Med.*, 75, 1912, 1-14.

F. Cutolo used two-point warmth stimulation and also a heat grill to make his introspective analysis of heat as a pressure-pain quality: A preliminary study of the psychology of heat, *Amer. J. Psychol.*, 29, 1918, 442-448. E. B. Titchener therefore placed heat between pressure and prick in his touch pyramid: Models for the demonstration of sensory qualities, *ibid.*, 31, 1920, 212-214. L. Knight showed that prick pain is an element in burning heat: The integration of warmth and pain, *ibid.*, 33, 1922, 587-590. S. C. Ferrall and K. M. Dallenbach placed heat between pressure and prick and showed that prick enters into burning heat: The analysis and synthesis of burning heat, *ibid.*, 42, 1930, 72-82. N. C. Burnett and Dallenbach worked out the formula for the intensity of heat as a function of the intensities of the constituent warm and cold: Heat intensity, *ibid.*, 40, 1928, 484-494. J. H. Alston showed that the warm and cold stimuli can be widely separated: The spatial conditions for the fusion of warmth and cold in heat, *ibid.*, 31, 1920, 303-312.

W. L. Jenkins' attack upon the Alrutz theory is to be found for the most part in his The reactions of untrained subjects to simultaneous

warm + cold stimulation, *J. exp. Psychol.*, 22, 1938, 451-461; The reactions of untrained subjects to simultaneous warm + cold + shock, *ibid.*, 22, 1938, 564-572. Further synthetic evidence against the Alrutz theory, *ibid.*, 23, 1938, 411-416; Analytic evidence against the Alrutz theory, *ibid.*, 23, 1938, 417-422.

For different forms of heat grills, see Thunberg, *op. cit.*, 1896; Cutolo, *op. cit.*, 1918; N. C. Burnett and K. M. Dallenbach, The experience of heat, *Amer. J. Psychol.*, 38, 1927, 418-431, esp. 419; H. Schlosberg, A simple heat grill, *ibid.*, 43, 1931, 119 f.; G. L. Freeman, A simplified heat grill, *ibid.*, 45, 1933, 344.

Touch Blends

On heat, burning heat and cool, see the notes of the preceding section. Here are the references for the other perceptions.

Wet as a cold pressure: M. Bentley, The synthetic experiment, *Amer. J. Psychol.*, 11, 1900, 405-425, esp. 414-425; S. Tung, The integration of punctiform cold and pressure, *ibid.*, 32, 1921, 421-424, I. Bershansky, Thunberg's illusion, *ibid.*, 34, 1923, 291-295 (after-sensations of cold and pressure give an illusory wet); A. H. Sullivan, The perceptions of liquidity, semi-liquidity and solidity, *ibid.*, 34, 1923, 531-541.

Oily as warm pressure: L. W. Cobbey and A. H. Sullivan, An experimental study of the perception of oiliness, *ibid.*, 33, 1922, 121-127. Punctiform warm pressures do not, however, yield any common objective perception: R. S. Malmud, The integration of punctiform warmth and pressure, *ibid.*, 32, 1921, 571-574; I. Bershansky, The areal and punctiform integration of warmth and pressure, *ibid.*, 33, 1922, 584-587.

Tickle: E. Murray, A qualitative analysis of tickling: its relation to

cutaneous and organic sensation, *ibid.*, 19, 1908, 289-344.

Hard and soft: A. H. Sullivan, The cutaneous perceptions of softness and hardness, *J. exp. Psychol.*, 10, 1927, 447-462.

Smooth and rough: M. Meenes and M. J. Zigler, An experimental study of the perceptions of roughness and smoothness, *Amer. J. Psychol.*, 34, 1923, 542-549.

Sticky: M. J. Zigler, An experimental study of the perception of

stickiness, *ibid.*, 34, 1923, 73-84.

Clammy: M. J. Zigler, An experimental study of the perception of clamminess, *ibid.*, 34, 1923, 550-561.

D. Katz published a monograph: *Der Aufbau der Tastwelt*, *Z. Psychol.*, Ergbd. 11, 1925 (see esp. 21-74 for touch patterns). G. Révész published a two-volume book: *Die Formenwelt des Tastsinnes*, 1938 (see esp. I, 83-161 for the perception of space, things and forms).

Chapter 14

ORGANIC SENSIBILITY

ORGANIC sensibility is a loose term used to classify events dependent upon the excitation of receptors inside the skin—sensations from the tissues immediately beneath the skin, from the muscles, tendons and joints (kinesthesia or proprioception), from the viscera, alimentary canal, thoracic and sexual organs, and sometimes—though it is stretching a definition—the semicircular canals. These internal ‘senses’ are subject to all the classificatory equivocalities which we have considered in the introductory paragraphs of the preceding chapter (pp. 463 f.). Their classification may be physiological by organ (muscle sense), functional by meaning (appetite), introspective by quality (ache), or perceptual by pattern (nausea or dizziness as analyzed into a sensory complex). In this chapter we shall proceed by considering the sensibility of organs or groups of organs, treating the perceptual and functional aspects secondarily.

Deep Sensibility

In the nineteenth century, tactual and cutaneous sensibility were thought to be identical. Warmth, cold, pressure and pain were elicited by thermal, forceful and penetrating stimuli applied to the skin. Since a needle, loaded within the ordinary limits of toleration, pricks only the skin and since the receptors for warmth and cold lie near the surface, no great confusion could result in respect of these three senses. Pressure, on the other hand, was usually applied with loadings that brought into function a second set of subcutaneous receptors. All the work of the last century on the validity of Weber's law for pressure, the tactual two-point limen and the error of tactual localization involved both cutaneous and subcutaneous receptors. Research like Goldscheider's, in which the skin of the arm was anesthetized and deep pressures and aches brought out by heavy loadings, was really concerned with the sensibility

of muscle, and thus with what came to be called kinesthesia or proprioception. Von Frey, on the other hand, limited his study of pressure to the skin by his use of small forces—the “microscopic” hair stimuli—applied solely to the pressure spots.

Deep pressure was not, therefore, recognized as dependent on a separate subcutaneous system of receptors until Henry Head in 1905 made the distinction (pp. 471-474). The cutaneous and the subcutaneous nerves are differently distributed, for the former come to the surface and often progress for a short distance before reaching the outskirts of the region where their receptors lie. The course of such a nerve can be traced on the skin by faradic stimulation with a single electrode, which produces a fluttering sensation referred to the region where the receptors lie. Head, severing such a cutaneous nerve, found that the center of the region which it supplies, although anesthetic to warmth, cold, prick and light pressure, was nevertheless sensible to heavy pressure. A fairly heavy pressure gave the dull pressure quality, a very heavy pressure gave dull pain. Sensed pressures could still be localized as accurately as when the skin was normally sensitive. Thus Head established deep sensibility as a special sensory system, to which many of the older experiments which used loadings of several grams or more had thereafter to be referred.

The distinction between deep sensibility and kinesthetic may be only functional, the former having to do with stimuli superficially applied and the latter with movement of the joints. The Pacinian corpuscles directly beneath the skin are usually assigned to deep pressure, but they also lie deep in the tissues, especially around the joints, and thus presumably in that location have a kinesthetic function.

Kinesthesia

Though his eyes be shut, a man knows about the position and posture of his body, and whether its parts are moving or exerting effort in sustaining a weight or in pressing against resistance. If this be kinesthesia, then no philosopher can ever have doubted its existence. Descartes in 1637 remarked on how a man is aware of distance by perceiving the directions of his two eyes, just as a blind man might feel out a distance with two staves, one in each hand. Berkeley in 1709 included the perception of straining in the eyes as one of the criteria of the perception of distance. Conver-

gence and accommodation—both kinds of kinesthesia—were thus early given this formal status. Some have said that Aristotle implied the existence of muscular sensibility; be that as it may, it was certainly Aristotle who so long delayed the recognition of a sixth sense by his doctrine that there are but five senses.

The classical and most successful argument for the independent status of the muscle sense was Charles Bell's in 1826. Although the muscle spindles were not discovered until 1863, physiologists after Bell all accepted the muscle sense as a "sixth sense," arriving in the latter part of the nineteenth century at a considerable body of research on the perception of weight, effort, resistance, movement and position. It was Bastian who in 1880 pointed out that these perceptions are so complex—involving, as they do, the sensibilities of muscles, tendons, joints and skin—that it is better to refer them to a "Sense of Movement" or *kinesthesia*. In general, the psychologists accepted this word *kinesthesia* as proper, and the term loomed large in the sensory introspections, so important in the experimental psychology of 1890 to 1920. The physiologist, Sherrington, on the other hand, having contributed to Schäfer's *Text-Book* in 1900 a chapter on "The Muscular Sense," invented in 1906 a new word with a physiological sound, *proprioception*. The proprioceptors, as distinguished by him from exteroceptors and interoceptors, mediate the kinesthetic sensations. It was the ultimate decline of introspective psychology and the consequent rise of physiological psychology that has today gone so far toward the replacement of Bastian's word by Sherrington's.

All through the second half of the nineteenth century there persisted confusion and controversy about the distinction between muscular sensations and sensations of innervation. Most of the important physiologists and psychologists of this period, including both Helmholtz and Wundt, believed that the innervation of efferent tracts in voluntary action establishes sensations that arise wholly within the brain and not by way of afferent fibers from the activated muscles. These sensations of innervation, forming the sensory conscious basis of the experience of volition, were relinquished, along with the will, only reluctantly by the psychology of the twentieth century. In the nineteenth century it was never certain whether an experience of effort was a muscular sensation or a sensation of innervation.

Now we take up these matters separately. •

1. *The Muscle Sense.* According to Sir William Hamilton, who sought in 1846 to set down the history of the muscle sense, the earliest recognition of this sixth sense as independent of Aristotle's five was due to two Italian philosophers, Julius Caesar Scaliger in 1557 and Caesalpinus of Arezzo in 1569. It is doubtful, however, if Descartes and Berkeley needed any such formal sanction to justify their appeals to the muscular sensibilities of the eyes in the formation of their theories of space perception. In the early part of the nineteenth century the French physiologist, Bichat, in 1812 presented evidence for sensitivity of the muscles, and the Scottish philosopher, Thomas Brown, in 1820 insisted that "our muscular frame" forms a "distinct organ of sense," without which the perception of spatial extension could not be developed. It is also true that Steinbuch in 1811 based a theory of space perception—all these men were anticipating the space theories of Lotze and Wundt—upon a sense of movement, but he, as we shall see presently, thought of the *Bewegungsideen* as occurring for innervation before the actual movement of the muscles and was thus really the originator of the conception of sensations of innervation.

The muscle sense, since it was not discovered by Charles Bell, might be said to have been 'founded' by him in 1826, when he read before the Royal Society his paper "On the Nervous Circle which Connects the Voluntary Muscles with the Brain." At that time the hard-and-fast distinction between motor and sensory nerves, the Bell-Magendie law as it came to be called, was still new. Bell had shown, in an obscure pamphlet in 1811, that the fibers of the posterior nerve roots at the spinal cord are all sensory, and the fibers of the anterior roots all motor, but the world at large did not soon learn about his discovery, for Magendie in Paris made the discovery again in 1822. Now, four years later, Bell was arguing that the nerves of the muscles must contain sensory fibers as well as motor. He cited the sensory experiences of overexertion and muscle spasm, of running and walking, and of the maintenance of balance by the blind. He remarked on the capacity of a person to judge small differences in the weights of objects by weighing them in the hand—that was eight years before Weber first gave $\frac{1}{40}$ as the just noticeable difference. He noted how muscle sensations, as well as sensations from the skin, must participate in the tactual perception of distance, size, form, weight, hardness and

roughness, and how visual perception is dependent upon awareness of the movements of the muscles of the eyes.

Bell also described—and this instance was crucial to his argument—a case of a woman with an insensitive arm. She could hold her baby to her breast only so long as she looked at her arm; when she looked away, the arm would drop and the baby start to fall. The case was probably one of *tabes dorsalis*. In the latter part of the century many such cases were cited as indication of the important function of kinesthesia in directing movement. For instance, in locomotor ataxia the patient cannot walk without leaning forward slightly in order to use vision for the correct placement of his feet. This is exactly the point that Bell was making about the “nervous circle”: precise movement depends not only on motor innervation which originates in the brain, but also on muscular sensations which originate in the muscle. “Between the brain and the muscles there is a circle of nerves,” he wrote; “one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscle to the brain.” In other words, the brain can make the muscles move correctly only because it has from them a continuous report as to how the movement is getting on. Bell’s argument about this nervous circle finds itself amply sustained today in our knowledge of proprioceptive reflexes and of the reflex circle, where kinesthesia is necessary for correct movement unless vision be substituted for the kinesthesia.

After Bell the existence of muscular sensation was good doctrine. Both Weber and Johannes Müller accepted it, and Kühne’s establishment in 1863 of the muscle spindles as sensory receptors in the muscle clinched the matter, if anything more were needed. One could, however, believe in the muscle sense and still hold the conviction about the existence of sensations of innervation.

2. *Sensations of Innervation.* It was the conviction that the will is an experienced entity which supported the belief in sensations of innervation. At the beginning of the nineteenth century British empiricism had become sensationistic. Locke (1690) analyzed experience into ideas, but Hume (1739) and James Mill (1829) put sensations first and regarded ideas as but copies of them. Certainly willing is conscious. One knows that and how he moves or is about to move. If consciousness is sensory, there ought to be will-

ing sensations. Steinbuch in 1811 called them *Bewegungsideen*, and argued that they arise directly from the innervation of motor nerves. That was the beginning of this theory.

Johannes Müller in 1838 accepted sensations of innervation, in addition to the muscle sense: "The idea of weight and pressure in raising bodies or in resisting forces may in part arise," he wrote, "from the consciousness of the amount of nervous energy transmitted from the brain rather than from a sensation in the muscles themselves." Sir William Hamilton in 1846 believed in them and argued for them, calling them the "locomotive faculty," which provides for our appreciation of the "mental motive energy" that initiates movement. Ludwig, the physiologist, in 1852, Wundt in 1863, Bain in 1864, Helmholtz in 1866, and many others after them, like G. H. Lewes in 1878 and Mach in 1886, all held to these sensations of central origin. Wundt even listed them in his inventory of sensations as late as 1910.

There were three arguments. (a) A person with an anesthetic arm may will to make a given motion and think that he has made it until he looks at the arm and sees that he has failed. Thus he has, it was said, the consciousness of movement without the correcting perception that the muscle sensations would have added. (b) A person can be conscious of an amputated limb and of moving it, even though the muscles which would have been involved have been amputated too. (c) If a finger is immobilized, one can will to move it, and the effort of will does not vary in degree with the strain in the muscle. This experiment, however, seems not to have been adequately controlled.

One of the first to oppose this doctrine was Landry in his study of paralyses and anesthetics in 1859. He considered all these phenomena and, admitting that the mind must be aware of its own volition, concluded nevertheless that the perceptions of resistance, weight and movement cannot be precise without the mediation of the muscle sense. Bastian came to the same conclusion in 1869; and Ferrier in 1876. James formulated the argument against the sensations of innervation in 1890. The case was not open and shut.

Now what was it that took these sensations out of the literature in the twentieth century? Why did Sherrington discuss them in detail in his chapter on the muscle sense in 1900 and ignore them completely in his *Integrative Action of the Nervous System* in 1906?

The answer lies in the coming of age of the theories of ideomotor action and of reflex action.

The theory of ideomotor action was not new. Lotze (1852) had given examples of it. So had many others. James (1890) stressed it. In the present century it became more important because interest shifted away from observation toward correlation, away from the direct observation of functions toward the discovery of functional relations between observed terms. In 1850 or 1890 it was enough to perceive directly that innervation is immediately sensed. In 1920 it would have been necessary to show, at the very least, that the innervation occurs before its sensation is felt and that there is no nervous circle operating by way of the muscle between it and the sensation. Attention to the *Bewegungsideen* showed merely that they were present before the consequent and appropriate movement, often a long time before it. Since movement followed them, they could not be caused by the movement. They were sensations *for*, not *of*, innervation.

For a similar reason the theory of the reflex, furthered by Sherrington, has flourished in the present century. Interest in the reflex takes attention away from mere excitation or mere movement and places it upon the causal relation between excitation and movement. This theory is, moreover, physiological in that it accepts conventional physiology and ignores introspection. Since nothing is known of the activation of sensory centers by motor centers, there is left for the explanation of this phenomenon only the nervous circle through the muscles—or through the eyes when the subject's own movement becomes for him a visual stimulus. Such a reflexological picture was early adequate to the facts. Since then the increase of knowledge about proprioceptive reflexes, as accounting for postural, articulatory and motor mechanisms, has fixed it firmly in psychophysiological thought.

That is why sensations of innervations went out of style after 1900.

3. *Kinesthesia*. Certainly kinesthesia got its full share of attention from 1850 to 1920. Fechner, with his *Elemente der Psychophysik* in 1860, made the discrimination of lifted weights the representative psychophysical experiment. There must have been lifted within the next sixty years hundreds of thousands of pairs

of weights, all for the purpose of studying sensitivity and judgment, for the lifted weights contributed much more to the development of the psychophysical methods than to a knowledge of kinesis.

The important kinesthetic problems were the determination of sensitivity in the perceptions of weight, resistance, effort, position and movement, and the discovery of the sensory mechanisms for these perceptions. Here Goldscheider in a series of papers published from 1887 to 1893 made the most notable contribution, while the physiologists established the existence of receptors in muscle and tendons and around the joints. Let us consider first the discovery of the kinesthetic receptors.

What we now call Pacinian corpuscles, or Vater-Pacini corpuscles, were first discovered by Abraham Vater sometime prior to 1741. He described them as tiny bodies lying along the cutaneous nerves. After that they were forgotten, until Pacini rediscovered them along the nerves of the finger in 1835. By 1840 he had found many bodies of this sort, differing greatly in size and lying in the deep subcutaneous tissue, the mucous membrane and the internal organs. Pacini's name became attached to the corpuscles because of the completeness of his description and also because he made his discovery at a time when the new sixth sense needed deep-lying organs to mediate it. It was Rauber who, in 1865, showed that these corpuscles are associated with the tendons and the ligaments of the joints, although they do not lie directly in the joint surfaces. Naturally then Goldscheider and others—since the joints were known to be sensitive—attributed the sensibility to these corpuscles and supposed that they function in the perception of the position and movement of a limb and to some extent also in the perception of weight and resistance. This argument was strengthened by the fact that introspection seems to show a qualitative similarity between deep pressure and articular sensibility—perhaps even an identity.

The second kinesthetic organ to be discovered was the muscle spindle. After 1850 there were several histological descriptions of small structures in voluntary muscle, structures not identified as to their functions. Kölliker in 1862 certainly did describe the muscle spindles—"nerve-buds" he called them—but he thought they were centers of growth. Then in 1863 Kühne gave a more precise account of these organs, named them *muscle spindles*, and sug-

gested cautiously that they might be receptors for the muscle sense. In 1892 Ruffini actually identified the spindles as neural tissue with nerve fibers leading from them, and Sherrington in 1894, by sectioning the nerve root at the spinal cord, showed definitely that their function is sensory. Finally, in 1928 Fulton and Pi-Suñer, studying neural action currents from muscle, came tentatively to the conclusion that the spindles are actuated by stretching the muscle, an hypothesis that was fully confirmed by Matthews in 1931 when he succeeded in isolating a single muscle fiber and spindle from the muscle of a frog and in showing that stretching is the true stimulus for this receptor. This finding implies that muscular contraction is perhaps unconscious, that the direct sensibility of the muscles occurs only on stretching. Such a conclusion would have seemed a strange outcome for the analysis of the muscle sense—a conclusion that the muscles are only slightly involved in Charles Bell's "muscular sense"—had it not been for the fact that other researches showed most of the kinesthetic perceptions, like movement, position, weight and resistance, to be dependent on the sensibility of the tissue near the joints and, when intense, of the tendons.

The receptors in the tendons were the last to be found. They are called the Golgi spindles but were first described by Rollett in 1876, who studied them in the frog's tendon. Golgi gave them their complete description and his name in 1880, and the argument as to their sensory function and innervation was made in 1900 by Huber and DeWitt. In general, it has been supposed that tendinous sensibility enters, as additional to articular sensibility, in the perception of heavy weight, great resistance and perhaps extreme effort.

There was also in the late nineteenth century a great deal of interest in kinesthesia and movement. Five fairly important texts, published between 1887 and 1903, dealt with this topic, but there were no far-reaching systematic discoveries. We must content ourselves, therefore, with noting certain important conclusions, no one of which ought to be entirely omitted from this book.

(a) Goldscheider in 1889 established the fact that sensitivity of a limb to movement is great—seemingly more precise than cutaneous space perception. At a rate of 0.3 degrees per sec., the threshold for passive movement of the shoulder lies between 0.2 and 0.4 degrees. The ankle is from a third to a fifth as sensitive, and the thresholds for seven other joints lie in between the thresholds for

the ankle and the shoulder. If sensitivity is measured in terms of speed of movement, then the shoulder still appears as the most sensitive joint, for movement in it is perceived at slower speeds than in other joints. Active voluntary movement shows sensitivity a little greater than passive involuntary movement. The fact that such small angular displacements can be sensed at such relatively slow speeds did much to direct attention upon the importance of kinaesthesia.

(b) It was also Goldscheider's opinion that the joint contributes more than the muscles to the perception of movement. He anesthetized the skin and the underlying tissues with cocaine and found that sensitivity is not greatly diminished. Also he partially anesthetized the joint by passing a faradic current through it, finding that sensitivity is then greatly decreased. Other clinical evidence bore further on this point. Duchenne about 1853, describing a case where muscular and tactual sensitivity were lost though sensibility was retained in the joint, noted that the perception of posture and movement were but little affected. Much later Strümpell in 1902 described the opposite condition: the joint was anesthetic, the tendon and muscle not, and the patient's perception of posture and movement was abolished. Pillsbury in 1901 verified Goldscheider's finding that faradization of the elbow reduces its sensitivity to movement, but showed that faradization of the wrist also reduces the sensitivity to movement of the elbow—as if faradization acted more as a distraction than as an anesthetic. In general, nevertheless, Goldscheider's analysis has been accepted.

(c) It was Goldscheider again who formulated the conventional threefold classification of kinaesthesia into muscular, tendinous and articular sensibility. He was supported by the finding of three appropriate receptors, by such analyses as we have noted in the preceding paragraph, and to some extent by introspection.

(d) In 1889—the year of Goldscheider's most important contributions—Müller and Schumann published their famous analysis of the mechanism of the discrimination of lifted weights. It was their contention that a second weight is judged heavier or lighter than an immediately preceding first weight, because the subject, being set (*eingestellt*) for a certain effort by the first weight, exerts the same effort upon the second. If the second comes up easily and quickly, then it seems lighter than the first. If it comes up slowly, then it is judged heavier. This view, which, because of

the importance of its authors, received much attention in psychophysics, was not taken by Müller and Schumann to support the theory of sensations of innervation. They held that the *Einstellung* or motor impulse is unconscious until its effect, the speed with which the hand lifts the weight, is noticed. Certainly an *Einstellung* of this sort enters into the size-weight illusion (a large weight feels lighter than a small weight which actually weighs the same). Thus it appears that besides kinesthesia we have in this field to take account of motor predispositions.

(e) Finally a word must be said about the increasing importance of kinesthesia in the introspection of those days. The Wundtian school reduced all space perception to a consciousness of movement. Visual extent was supposed to be due to an awareness of eye-movements, and accommodation and convergence to furnish kinesthetic cues in the visual perception of depth. Then, besides muscular sensation, Wundt, in his tri-dimensional theory (1896 *et seq.*), introduced strain and relaxation as a dimension of feeling, thus sanctioning the constant use of these terms in the description of consciousness.

Later, when Külpe's Würzburg School had failed to validate the *Bewusstheit* as an imageless element of thought, Titchener proposed as a substitute the conscious attitude, which again and again found its description in terms of kinesthesia. Introspective protocols on the thought processes were filled with sensory accounts of what the observer's body was doing. Take, for instance, Miss Clarke's introspective study of fifty-nine conscious attitudes—such conscious states as approval, awfulness, baffled expectation, caution, comfort, confidence, and so on down the alphabet. According to the present author's count, she listed here 166 mental processes—sensations and images—distributed as follows:

Kinesthetic or organic	113
Verbal (kinesthetic, auditory, visual)	21
Visual	18
Tactual	9
Auditory	5

Some of these introspections make biological sense. For instance: *Pride* = "slight tendency to straighten up my neck and smile." *Irritation* = "sensation from frowning; visual image of frowning face; tendency to lower and shake the head; hot sensation in head

and back." Others were more general. *Disgust* = "organic sensations throughout the body." *Strangeness* = "weak organic feeling all over body; strain; sensations from breathing; and special sensation from diaphragm." There were many papers at this time (1909-1916) which showed how pervasive is the kinesthetic content of consciousness. "The great god Kinesthesia!" someone remarked at a meeting of the Society of Experimental Psychologists when several of these introspective studies had been reported.

(4) *Proprioception*. In 1895 Mott and Sherrington showed conclusively what Bell had asserted—that sensory impulses are necessary for accurate voluntary movement. They sectioned at the spinal cord the posterior nerve roots of a monkey's limb and found that the animal let the limb drag and did not use it in locomotion or even to get food when prevented from using his other limbs. Nevertheless the limb with the cut sensory connections was activated in violent general action, like struggling, showing that the motor fibers, which pass through the anterior root, were still intact. Of course, this finding was not novel: it was simply new and excellent proof, after seventy years, that Bell's conception of the 'nervous circle' was correct.

The next decade saw the birth of reflexology—in psychology, in physiology. In psychology there was John Dewey's defense of the reflex arc concept in 1896, Pavlov's work on the conditioned reflex which began about 1903, and of course ultimately behaviorism, the stimulus-response psychology of Watson after 1913. Sherrington, however, made an equal contribution on the side of physiology. The general effect of the Bell-Magendie law's separation of the sensory and motor neural systems and of the discovery of sensory and motor centers in the brain had been to separate the afferent and efferent neural systems. Sherrington, following the path marked out by Bell and seeing that the entire reflex arc must be considered as a whole, finally formulated this point of view in 1906 in his Silliman Lectures at Yale and in the resultant *The Integrative Action of the Nervous System*, a very important book which was long fundamental to thought in neural physiology and in behavioristic psychology.

It was in an article published just before these lectures that Sherrington invented the term "proprio-ception": "since in the deep field the stimuli to the receptors are delivered by the or-

ganism *itself*, the deep receptors may be termed *proprio-ceptors*, and the deep field a field of proprio-ception," he wrote. He also coined at the same time the terms *intero-ceptor* and *extero-ceptor*. Proprioception is, of course, nothing else than kinesthesia, except that it is the afferent part of a reflex physiologically considered, whereas kinesthesia is sensation considered in its own right and functioning for introspection. The decline of interest in the sensory data of consciousness and the rise of physiological conceptualism in modern psychology has led, however, almost to the abandonment of Bastian's word and to the general acceptance of Sherrington's.

Proprioception in the Inner Ear

The history of what has been called vestibular equilibration, the static sense, ampullar sensation, giddiness, vertigo, the sense of rotation, and the sensibility of the semicircular canals is voluminous and simple. It is voluminous because there has been so much written about it: in 1922 Griffith cited 1685 titles from 1820 on. It is simple because it can all be organized about Purkinje's descriptions of dizziness (1820-1825), Flourens' discovery that lesions of the semicircular canals produce muscular incoordination in the plane of the affected canals (1824-1830), the Mach-Breuer-Brown experiments and their theory of the function of the canals (1873-1875), and the discovery of vertiginous habituation by the psychologists of the U.S. Army (1918), Griffith (1920) and Dodge (1923). Here is the story.

Vertigo in its dependence upon bodily rotation, the motion of a ship at sea or vision downward from a great sheer height did not have to be discovered by scientists; such things man could notice without special training or technical aids. It required scientists, however, to describe the phenomenon, and one of the early descriptions was assembled in 1801 by Erasmus Darwin, the grandfather of Charles Darwin and the author of *Zoonomia*. He told how on spinning around one sees the environs rotating in an opposite direction, slowing up, and then, if the actual rotation is stopped, appearing to revolve in the opposite direction. Apparent negative after-rotation thus became a matter of record. The rest of Darwin's discussion had to do with dizziness from the tops of tall towers, sea-sickness, nausea, and the inability of a "hoodwinked" person to walk a straight line.

From 1820 to 1827 Purkinje, the phenomenological physiologist (pp. 117 f.), contributed five descriptive and experimental papers about vertigo. He established the method of rotation—spinning the observer around—for the study of the phenomenon; other methods of inducing vertigo were used later. He described fully the apparent movement of the visual field during rotation and after rotation has ceased. He discovered that the axis of the apparent after-rotation moves with the head, a fact which indicated to him that the organ for this perception must lie in the head. He suggested that the brain itself, being soft and pliable, might lag behind the rotation of the hard skull, thus giving rise to this perception. That was an appeal to the principle of inertia which Mach and the others used in the 1870's to explain the action of the semicircular canals. Purkinje, when he suggested this theory (November, 1825), had only just learned of Flourens' initial experiment (*vide infra*) in which he sectioned the canals of pigeons, but, since the interpretation of Flourens' results was not yet clear, Purkinje pointed out that the inertia of the brain might explain, not only the perception of rotation, but also the perception of rectilinear movement. By 1827 he had ceased to mention this theory, being content to note the similarity of the effects of rotation to what happens when a lesion is effected in the cerebellum (Flourens).

Meanwhile on November 15, 1824, Pierre Flourens, the outstanding physiologist to use in that half century the method of ablation for the determination of the functions of the different parts of the brain, sectioned two corresponding semicircular canals in a pigeon, one on each side, and allowed the wounds to heal. He found that the bird had become, as a result of this operation, incapable of accurate coordinated movement, showing the maximal disturbance of movement in the plane of the sectioned canals. An amazing discovery—that spatial ordering of bodily movement should depend upon the operation of the organ of hearing! He took the pigeon to a session of the Académie des Sciences on January 10, 1825, for exhibition. Later in that year he published his new fact briefly, and then in 1828 undertook careful systematic research on the functions of the canals, publishing those results in 1830.

Flourens made it clear in these papers that sections of the canals give rise to radical disturbances of equilibrium, incoordinations similar to what he had found for experimental lesions of the cere-

bellum and Purkinje had found for vertigo. The pigeons could not walk or fly. They 'tried' to move, and did indeed move, yet could not move as they 'wished.' Instead they tended to jerk their heads back and forth violently and purposelessly, mostly in the plane of the cut canals. The section of a single canal, he found, had only a small effect. The violent movements came from the section of corresponding canals on both sides. Cutting of all six canals produced convulsions. Flourens verified these conclusions first with more than half a dozen other kinds of birds, and then with rabbits.

After Flourens nothing much happened for forty years. In 1853 both Harless and Brown-Séguard told how they had verified Flourens' experiments. Harless' account was brief, but Brown-Séguard described the turning movements in frogs that occur on section of the canals and also on experimental injury to the auditory nerve. He disagreed with Flourens' conclusions, preferred to account for the turning movements as a result of the nerve injury. In 1866 Toynbee, in a six-line note, reported the case of a patient whose vertigo disappeared permanently when his inner ear was extruded. Toynbee removed a piece of bone from the external meatus only to find on examination that it consisted of the vestibule, cochlea and canals of that ear!

The 1870's, on the other hand, were the important decade in this history. Out of more than a hundred published papers there were contributions by such well known men as Goltz, Hitzig, Mach, Breuer and Crum Brown. Let us consider first some of the fact-finding and then the Mach-Breuer-Brown theory.

We must begin by noting the development of methods of research and thus of the knowledge of the essential stimulus for these phenomena. Mach (1873-1875) arranged for the experimental control of rotation by constructing his huge rotation chair (Fig. 86), in which an observer, seated erect in the chair, could be rotated about his own vertical axis (*aa*), or about another axis (*AA*) eccentric to his body. This gigantic instrument, built of 4-by-4-inch timbers, over four meters long and rotating consequently through a circle of four meters diameter, was later reproduced in several American laboratories which could in the early days extract sufficient space from a reluctant administration.

The surgical method of Flourens was perfected by Cyon (1873-1878) and Böttcher (1874). Cyon acquired a high degree of operative skill, learning even to insert foreign matter into a single

canal (*vide infra*). Goltz (1870) found that hot and cold water in the external ear produce vertigo, nausea and nystagmic eye-movements, and Hitzig (1871) perfected the method of arousing vertigo and eye-movements by passing an electric current through the head, a technique already mentioned by Purkinje (1826).

Goltz's experiments (1870) were designed to show that the ear

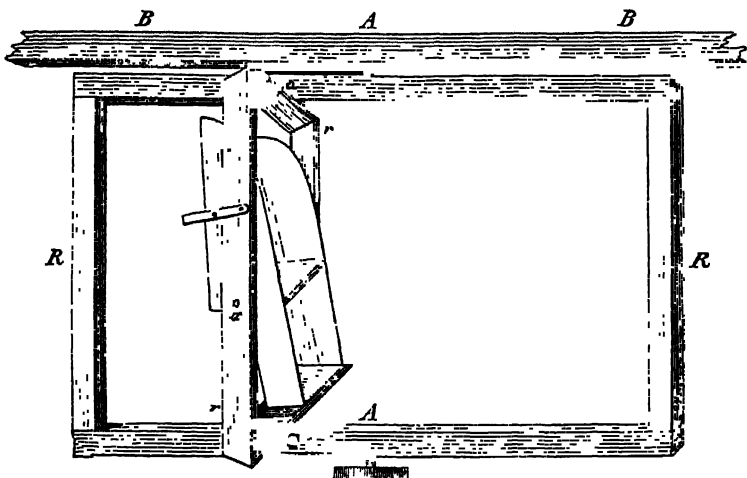


FIG. 86. ROTATION FRAME: MACH (1873)

The subject sat blindfolded in the chair, whose angle could be changed about the horizontal axis aa . The back and sides protected him from air-currents on rotation and he could also be boxed in with cardboard. He could then be rotated by hand about his own vertical axis, aa , or about the eccentric axis, AA . In later models the radius aA could be altered and there were counterweights on the frame RR to balance the subjects. In the original instrument RR was 4 m. long, built of 4×4 -in. timbers.

furnishes a basis of orientation primarily for the head, that orientation is changed when the head is moved after the rotary effect has been established. Löwenberg (1873) devised other experiments to prove that the disequilibrium arising from stimulation of the canals or injury to them depends upon positive excitation by the canals and is not merely a partial paralysis due to the canals' ceasing to function.

Now for the theory. Purkinje had suggested that vertigo of rotation might depend upon the inertia of the brain tissue, but by

now it was pretty certain that the cause of the perception must be found in the ear. Goltz (1870), noting what would be the effect of inertia within the canals, argued that orientation must be due to a pattern of pressures set up in the three canals. Since the canals are at right angles to each other, these three pressures could, by a sort of Cartesian coordinates, come to represent any position in space.

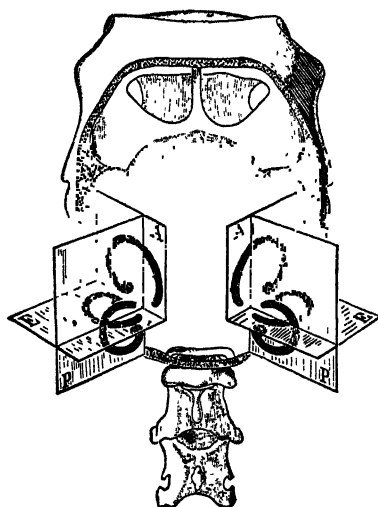


FIG. 87. SPATIAL RELATIONS OF THE SEMICIRCULAR CANALS

The canals, tremendously magnified with respect to the skull, are shown from behind as if on glass plates. The anterior canal on the left is paired with the posterior canal on the right and symmetrical to it. The left posterior canal is also symmetrically paired with the right anterior canal, and the two horizontal canals form the third symmetrical pair. After Ewald.

In 1873 Mach developed this idea by showing that rotation might be expected to create in any canal a pressure which varies in amount according to the component of the rotation that lies in the plane of that canal. Horizontal pressure would affect only the horizontal canals. Rotation about a transverse horizontal axis would affect all four vertical canals, since they lie at about a forty-five-degree angle with the plane of rotation (Fig. 87). But Mach went further. He pointed out that the principle of inertia would operate, not for rotation, but only for a change in speed of rotation, that is to say, for acceleration or deceleration. Thus his theory made angular acceleration the stimulus to the pressure-pattern upon the nerve endings in the ampullae of the canals. This conception explains, of course, why rotation is perceived during the acceleration of starting rotation from rest, why there is adaptation (no perception of rotation) when speed is steadily maintained, why deceleration or stopping arouses the negative after-effect (perception of reversed rotation). In part Mach was arguing tele-

ologically: the structure of the canals is such that they ought to work thus. The chief difficulty with his theory was that the canals are so small (about half a millimeter diameter inside the bony tube) that the lymph could not be expected to flow within them. Mach believed that it did not actually flow, but that the pressures would be created by its tendency to move.

Mach also proposed that the macula in the vestibule may be a similar organ for the perception of acceleration in a straight line. Such acceleration can be perceived; the canals ought not to be affected by it; the macula is an extra organ waiting to have a function assigned to it; it could work as he suggested.

In 1873, the year of Mach's first contribution to this theory, Breuer also had the same idea, deriving it, as did Mach, from Goltz's discussion. The theory was thus independently conceived, except as Purkinje, Goltz and others had already indicated its general nature. Mach was explicit in 1873, but Breuer's first full discussion came out in 1874, when he argued that the dynamic system of the canals can work only if the lymph flows freely in the canals. Mach had thought that the lymph could not flow in the tiny canals, that only differentials of pressure would be set up by angular acceleration. Breuer held that pressure differences would too soon be equalized unless the liquid were free to lag behind in positive acceleration or coast ahead in negative acceleration. There is a real dilemma involved in this discrepancy: each argument seems adequate against the other theory.

Breuer's most important paper, developing his theory, criticizing Mach, and recounting experiments on birds and dogs which verified Flourens' results, came out in 1875. Here, being concerned more with equilibration than with rotation, he applied to the sensory functions of the canals the name *static sense*, a designation that held for a long time, even though the perception of rotation, not the maintenance of balance, was so often the subject of investigation.

The third independent originator of this theory was Crum Brown in England, who discovered Mach's and Breuer's first papers after he had formulated the theory himself but before he published in 1874. His name has, therefore, rightly been added to what is called the Mach-Breuer-Brown theory.

Crum Brown, agreeing with Breuer as to the actual movement of the lymph in the canals, added one new feature to the theory.

Mach had supposed that each canal mediates all the components of rotation that lie in its own plane, both positive and negative. Clockwise acceleration about a vertical axis for the erect observer would move the ampullar organs in both the right and left horizontal canals in the same angular direction, and counterclockwise acceleration (clockwise deceleration) would exert pressure in the opposite direction. Crum Brown doubted that the same ampullar organ could operate for both directions of rotation, could have two specific energies, as it were, and he observed that such a double functioning would be unnecessary because of the symmetrical arrangement of the canals. The canals are paired. The left canal is always the reversed pattern of its mate on the right. Thus Brown assumed that the left canal might function for one direction of acceleration, say clockwise, and be 'silent' for counterclockwise acceleration. Its mate on the right, being reversed anatomically, would also be reversed in function, operating when the left canal was silent. It was a plausible view which fitted in with Flourens' finding that disequilibrium in a given plane is relatively small when only one canal in that plane is sectioned and more than doubled when its mate is also cut. Mach presently accepted Brown's contribution.

Crum Brown also accepted Mach's idea that the macula in the vestibule should be regarded as an organ for the perception of rectilinear motion. Having simplified the functions of the ampullar organs so much, he could hardly avoid relieving them also of the capacity for excitation in rectilinear acceleration.

Not all the theorizing of the 1870's was done, however, by Goltz, Mach, Breuer and Crum Brown. Böttcher (1874), after criticizing the whole history of thought on this problem from Flourens on, decided that the function of the entire ear is auditory. Cyon, in a polemical paper in 1873, made a similar argument, maintaining, however, that auditory space perception is basic to the perception of bodily space in general and to the orientation of the head in particular. The canals seemed to him to be, because of their anatomical arrangement, a natural spatial organ, so of course their injury would lead to disorientation. (See pp. 386 f., 401-403.) In 1878 he used his operative skill to drain the canals of pigeons, fill them with gelatin, and plug them with filaments of dry sea-weed (*Lamanarian*) which would presumably absorb moisture, swell, increase the internal pressures, and in any case immobilize the

lymph. When he found that the pigeons did not show disequibration after this violent treatment, he concluded that Mach's theory of pressure as the stimulus to the ampullar organs must be wrong. Nevertheless the Mach-Breuer-Brown theory remained in favor.

It prevailed, but not everyone was sure that the ampullar sense is like other senses in giving rise to sensation. Both Mach and Cyon noted that introspection fails to reveal for rotation any unique quality. The perception consists rather of a pattern of kinesthetic and cutaneous sensations referred to the periphery of the body. They concluded, therefore, that the ampullar sense is a reflex sense, giving rise directly to reflexes which establish (or disestablish) equilibrium and only indirectly by way of these reflex changes to the characteristic kinesthesia.

Confirmation of the theory that vertigo depends on the action of the semicircular canals was next got from the behavior of deaf mutes. Crum Brown had suggested in 1878 that, since many deaf mutes are found to have impairment of the canals as well as of the cochlea, these persons ought not to be subject to vertigo. It was William James who put this idea to the test in 1882. He arranged a sort of swing, a board suspended by two ropes. The subject sat on the board; the ropes, kept apart by a cross-bar above the subject's head, were twisted together above the bar by turning the board around continuously. Then, when James let go of the board, the subject spun around rapidly as the ropes untwisted themselves, and told, after he had come to rest, whether he was dizzy. Of two hundred Harvard students and instructors, none of whom was a deaf mute, all but one were made dizzy. Of 519 deaf mutes, 199 were dizzy, 134 others only slightly dizzy, and 186 not dizzy at all. They all had their eyes closed, of course, and some performed the experiment with their heads on one side, a disorientation which normally gives greater vertigo if the head is moved afterward. James also reported that some deaf mutes cannot swim, because they lose orientation almost completely when the visual and kinesthetic cues for position are diminished under water. He noted, too, that there are cases of swimmers who could no longer swim after they had lost their hearing—their hearing, and presumably also their ampullar sense.

Kreidl in 1891, confirming James' finding, amplified it. About fifty per cent of his deaf mutes failed to become dizzy or to show nystagmic eye-movements after rapid rotation. A quarter of them

failed to note a displacement of the vertical on rotation. Later, he found that 84 per cent of congenital deaf mutes show no vertigo after rotation. The pathological findings that he cited from post-mortems gave the figure of 56 per cent of deaf mutes with defective semicircular canals. Pollak in 1893 failed to get vertigo from the passage of an electric current through the head in 30 per cent of the deaf mutes on whom he tried this experiment. The point here is, of course, that the percentage of live deaf mutes without vertigo is roughly comparable to the percentage of dead deaf mutes with defective canals.

Subsequently there was enough research on vestibular equilibration—over a thousand papers published from 1900 to 1920—but no radical change in basic theory until 1918. Gaglio in 1899 introduced the anesthetic method of research, destroying the sensitivity of the canals by the use of cocaine and noting consequent disequilibrium. Alexander in 1910 demonstrated clearly what others had reported incidentally, that pressure on the ear produces nystagmus. But these discoveries applied only to methods of research and did not change the basic facts.

In the World War of 1914–1918 the French, as aviation became important, introduced the Bárány test for aviators. That is a test of sensitivity to rotation, and it was argued, in analogy to the deaf mutes who cannot swim under water, that equilibratory sensitivity ought to be a property of a good aviator. After the United States had taken over the test, it turned out that good experienced pilots were often not very susceptible to vertigo, showing up poorly on the test. Griffith, with the War over in 1920, was one of the first to bring this problem to a laboratory solution. All the effects of rotation—nystagmus, nausea, changes of circulation—wear off, he proved, with practice. They decrease rapidly from day to day, and some decrease in noticeable degree in ten successive trials on a single day. He did not find, however, that such vertiginous habituation decreases sensitivity to the perception of rotation: practice destroys, not the basic perception, but the accessory phenomena.

Dodge in 1923, however, showed that the basic perception, when carefully studied, is much more complicated and subject to adaptation than the simple dynamics of the semicircular canals lead one to expect. He found (a) a positive after-image of rotation which precedes the negative whenever both occur, (b) adaptation, not

merely to constant speed, but actually to constant angular acceleration, (c) an occasional negative after-image during acceleration, and (d) adaptation to oscillatory movement. These phenomena occur both for the perception itself and for the accompanying eye-movements. It is true the perception of such anomalies may not be as intense as the experiences which occur according to the now classical theory, yet Dodge worked under certain limitations—such as the fact that the acceleration of an observer cannot with safety be continued indefinitely in order to get adaptation. His findings seem to mean that the organism adjusts itself, temporarily or even relatively permanently (consider the whirling dervishes) in such a manner that frequently repeated conditions come to have less effect upon it. Since such adjustments cannot occur in the mechanics of the ear, they must, it would seem, depend upon the central nervous system. Dodge called rotational adaptation of the kind that is explained by the Mach-Breuer-Brown theory “peripheral sensory refractoriness, mechanical type.” He ascribed these other adaptive phenomena to the operation of a “central compensating factor.” As in vision and hearing, so now in the vestibular sense, the progress of research had forced theory to take account of the brain as well as the sense-organ.

The question of a unique ampullar quality was settled by Griffith in 1920. Mach and Cyon had doubted that there is any special sensation, available to introspection, for this sense. They noted that the conscious data of the perception are visual and kinesthetic. Griffith confirmed them. Dizziness is (a) kinesthesia from the eyes, neck and arms, (b) pressure from the regions of the viscera, chest and head, and (c) sensory processes which depend upon vascular changes. This pattern becomes less intense and less complex under habituation. It varies with attention, attitude, the character of the visual field, and the mode of ocular fixation. Since these phenomena are all accessory, arising immediately from other receptors, it would seem that the semicircular canals do actually provide us with a sense that furnishes us with no sensation, a ‘reflex sense,’ as Cyon called it.

Internal Sensibility

Except of the muscular sense, not much of scientific importance was said concerning internal sensibility before the present century. Everyone knew about hunger and thirst, and a great deal was

written about these two experiences; yet the writers had little comprehension of the nature of these perceptions either as interoception or as motives. So too everyone knew about bodily pain that occurs in injury and sickness; yet there was no method for discovering how it is aroused or where it arises. Being inaccessible, the insides of the body seem to be fairly insensible. The philosophers and physiologists tended, moreover, to ignore the viscera in their discussions of sensation, because sensation, which was regarded as the avenue by which the mind learns about things, must normally have an object, whereas pain, pleasure, tickle, shudder, hunger, nausea, and the sense of well-being appear to have no objects.

Albrecht von Haller (1763) in his systematic inventory of the sensory capacities of the human body mentioned under *Tactus* only the sensations from sweating and from vasomotor changes. After finishing with the five senses, he wrote, indeed, a chapter on *Sensus interni*, but there he considered perception, imagination, attention, memory, thought, judgment, delirium, emotion, will, pain, pleasure, desire and sleep—all the mental events that are, as it seemed to him, wholly contained within the skin. It is true that elsewhere he commented on internal pain as well as hunger and thirst, that he had written an article in 1752 showing how insensitive are the internal organs to ordinary handling, although responding readily to their adequate stimuli; nevertheless, the problem of internal sensibility was not clearly drawn for him.

Trying to be systematically complete in 1829, James Mill could add to the five senses only "the sensations of disorganization," by which he meant pain, both external and internal, muscular sensations, and the sensations of the alimentary canal. Bain, however, in 1855, could discuss organic muscular feelings (pain, fatigue, sensations from the bones and ligaments), sensations of nerve (pain, fatigue, health), organic feelings of circulation and nutrition (thirst, starvation, consciousness of animal existence), feelings of respiration (relief, suffocation), and sensations of the alimentary canal (digestion, hunger, disgust). Sir William Hamilton in 1859 also considered the problem of the general tactual sensitivity of the body, the consciousness of well-being and ill-being, listing the facts under the conventional terms, *coenesthesia* and *common sensibility*, that were then available. Weber, of course, in 1846 had already given the *Gemeingefühl* its status as a class, including in it all pain and also the other inaccessible internal sen-

sations. In spite of this preparation, physiological psychology's greatest systematist, Wundt, turned out later to have almost nothing to say about internal sensibility in any of the successive enlargements of his great handbook that appeared from 1874 to 1910. In his last edition he gave but six pages out of his 2185 to kinaesthesia, and but four to tickle, shudder, itch and internal pain as representatives of the *Gemeingefühl*. All in all there was, in the nineteenth century, more silence than wisdom about internal sensibility.

In the present century there have been four chief lines of research: (1) the study of the sensibility of the viscera, especially of the alimentary canal, in respect of pressure, pain, warmth and cold; (2) the analysis, both physiological and introspective, of hunger; (3) the similar analysis of thirst; and (4) the introspective description of other organic perceptions, like appetite and nausea. Let us consider these topics in order.

Visceral Sensation

That the viscera can give rise to pain is the direct evidence from disease and injury. The sensory content of 'bodily suffering' or 'a painful death' does not seem to be entirely cutaneous. Common sense has long asserted the existence of internal algæsia.

Weber, however, in his well-known chapter of 1846, undertook to inquire into the exact nature of organic sensibility. The *Gemeingefühl*, including as it did pain, he assigned to the internal organs and tissues, but he denied the existence of the *Tastsinn* inside the body. Since he was convinced that the *Tastsinn* consists of the *Temperatursinn*, the *Drucksinn* and the *Ortsinn*, this conclusion meant for him that warmth, cold and pressure, as well as their precise localization, must occur only in stimulation of the body surface. He thought it obvious that touch is not an internal sense, since food disappears after it passes the pharynx and is seldom perceived during its passage through the alimentary canal. Thus, if the viscera lack the *Tastsinn*, they ought to be insensitive to temperature as well as to touch. Weber, to find out about this matter, drank quickly large doses of ice-water and of hot water, also injecting them into the colon. Ordinarily he felt on these occasions no cold or warmth. When the amount of water was great and the temperature was extreme, it is true that he got

sometimes a faint cold or warmth, but he ascribed such sensations to thermal conduction to or from the body-wall, being able at times to observe a slight change of temperature ($1^{\circ}\text{C}.$) in a thermometer bound against the skin at a spot nearest the internal stimulation. That, then, was Weber's dictum: only pain and the other nondescript perceptions of the *Gemeingefühl* come from the internal organs, not pressure nor warmth nor cold.

Psychologists could accept Weber's rule and still not be disturbed about perceptions like hunger, nausea and suffocation because those sensations, being nondescript, might well belong to the *Gemeingefühl*. Conceivably a considerable account of internal sensations might have come to be based on introspection alone, had doubt not been thrown on introspection as a reliable index of the place of origin of internal sensations. It was Hilton in 1863 who first described referred pain. He called it "sympathetic pain," showing how disease in an internal organ is often accompanied by a pain that is localized at some characteristic place on the surface of the body, and warning physicians, therefore, against trying to find disease in the region of the pain. He believed that afferent nerves from the internal organs are in some cases so associated in the central nervous system with particular afferent nerves from the skin, that algescic stimulation at an organ leads to excitation of nerves of pain from the surface of the body. Disorders of the viscera habitually give rise to pains between the shoulders, he said, and diseases of the spine to pains in the back of the head. His finding thus robbed introspection of its validity for the localization of organic processes. If what one feels on the outside may 'really' be inside, then what one localizes somewhere inside may easily turn out to be somewhere else. Even hunger, which seems so surely to be in the stomach, may actually depend on a lack of nutrition in the blood, being 'referred' to the stomach only because the hungry organism ought to pay attention to his stomach.

Although the late nineteenth century was not without its researches on visceral sensibility, interest in the problem did not become acute until after 1900. Then, in 1901, Lennander published some observations on abdominal operations performed under local cocaine anesthesia. He found that he got no sensation at all from cutting, pinching or burning the tissues of the stomach, intestines, liver, gall bladder and kidneys. From the parietal peritoneum, on the other hand, he easily got pain from cutting and

from heat, although not from pressure or cold. Thus he concluded that all abdominal pain must come from the parietal peritoneum, explaining how colic might produce such tension on the peritoneum as to produce pain. For a few years surgeons accepted Lennander's conclusion as final, and, indeed, their work seemed to confirm it.

Presently, however, Lennander's generalization was challenged. Kast and Meltzer in 1906 and Ritter in 1908 showed that the superficial injection of cocaine reduces the sensibility of underlying organs and that exposure to air also diminishes their sensitivity. Dogs, under the effects of morphine but without other anesthetic, gave evidence of pain when the visera were pinched or burned and the peritoneum left untouched. This discovery was also consistent with the finding of Head, Rivers and Sherren who, presenting evidence in 1909 that very hot or cold water introduced into the severed colon of an operated patient produces a mild thermal sensation, concluded that "protopathic" sensibility is thus shown to extend to the viscera.

Meanwhile the increasing importance of the James-Lange theory of emotion was disposing psychologists toward a belief in a variety of visceral sensations. Meumann, first in 1907 and later in 1909, undertook to examine the whole situation, reviewing the literature, inventorying his own visceral experience (he seems to have been habitually aware of his own alimentary processes), performing a few experiments and criticizing Lennander. He described as different perceptions hunger, stomachic emptiness and repletion, heart oppression and suffocation. Becher, on the other hand, performing experiments in 1908 and finding the esophagus everywhere sensitive to pressure, cold and warmth, was unable to discover sensibility in the stomach. Hot and cold water in small amounts produced for him no thermal sensations, whereas amounts large enough to yield warmth or cold were suspect as possibly affecting the thermal receptors in the body-wall. So Becher decided for stomachic insensibility and there was, therefore, a little controversy between him and Meumann until the problem passed into other hands.

It was also in 1908 that Hertz, Cook and Schlesinger got results similar to Becher's, although somewhat more inclusive. They showed that the esophagus is everywhere sensitive to warmth, cold and pressure. They aroused the thermal sensations by inject-

ing hot or cold water through a thick-walled stomach tube which was introduced to different points in the esophagus, and got pressure, not by contact, but by the inflation of a rubber balloon on the end of a stomach tube. It was muscular pressure (although from plain muscle), continuing as long as the balloon was kept inflated and disappearing at once when the air was let out. Rapid inflation of the balloon gave rise to deep pain, an ache. None of these effects could they get, however, from the stomach, which proved, nevertheless, to be chemically sensitive to alcohol, oil of peppermint and oil of cloves. Hertz argued later that the occasional apparent thermal sensitivity of the stomach must really come from the esophagus, sometimes because the stomach tube had walls that were too thin, sometimes, when no tube was used, because the hot or cold water accumulated at the end of the esophagus before the final deglutition. Hertz also found in 1911 that the colon responds as does the esophagus for the inflation of a balloon, yielding pressure for slow inflation and pain for rapid. He pointed out at this time that cutting, pinching and touching are probably not adequate stimuli for the plain muscle of the alimentary tract, whereas distension is.

The present author in 1915 verified the general conclusions of Hertz, Cook and Schlesinger in respect of thermal and chemical stimulation and of distension, but showed further that the stomach is after all sensible to warmth and cold. The earlier experimenters, it appeared, had failed to find thermal sensitivity in the stomach, because the gastric tissues and contents tend to neutralize the temperature of the stimulus. In this new experiment the actual temperature of the stomachic contents was measured by a thermocouple, with its wires running through a small rubber tube inside the larger stomach tube. Thus it turned out that 25 cc. of water at 60°C. would raise the contents of the stomach to 40°C. in about 6 sec., producing a sensation of warmth, and that 25 cc. of water at 0°C. would lower the contents of the stomach to 30°C. in about 6 sec., producing a sensation of cold. No change of temperature could with these stimuli be found on the skin of the adjacent body-wall, where it required 500 cc. of water at 0°C. to lower the temperature by 0.05°C.

There still remained at this time, however, a contradiction between common sense and experiment. Casual experience shows that food seems to disappear after it passes the throat and may, if

very warm or very cold, seem to reappear in the region of the stomach. Experiment, on the other hand, shows that the esophagus is sensitive to its adequate stimuli throughout its length. Herein lies a contradiction; what is its explanation? It was again the present author who resolved the difficulty by showing that the sensations from the esophagus are referred either up or down (or occasionally both up and down); they seem to come either

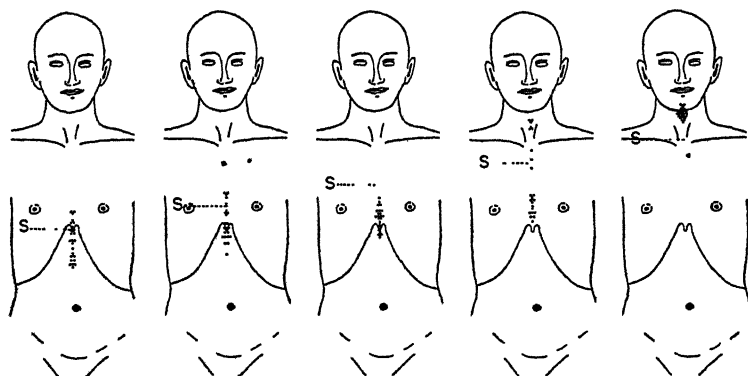


FIG. 88. EXTERNAL LOCALIZATION OF ESOPHAGEAL STIMULATION: BORING (1915)

The higher stimulations are referred up; the lower, down. S marks the level of the stimulus (electric shock). The 25 dots on each diagram indicate the level of the 25 localizations on the chest or neck, but are not correct as to transverse localization which was almost always close to the median line. From *Amer. J. Psychol.*, 26, 1915, 38.

from the throat or from the stomachic region. He placed in the esophagus a stomach tube carrying on its outside five metal rings, spaced evenly, five centimeters apart. Then, producing electric shocks at each of these positions, he had the subject localize the region stimulated by touching the proper place on his chest or neck. The results of this experiment are diagrammed in Fig. 88. The esophagus, separated from the skin of the chest by the hard thoracic wall, turns out not to be associated, each point with the nearest external position on the chest. The higher esophageal stimulations are localized outside above the clavicle, the lower below the sternum, and the stimulations in the middle either up or down or occasionally both up and down.

The general conclusion is, then, that the viscera are sensitive,

but only to their adequate stimuli—as, indeed, Haller had said in 1752. Cutting, pinching and touching are inadequate for most tissues, but adequate for the peritoneum. Distension may excite pressure or, if rapid, pain from most portions of the alimentary canal. Warmth and cold are adequate stimuli for the esophagus and stomach, though probably not for the intestines. At this point the unpopular teleological argument usually slips in to increase assurance about the thermal insensitivity of the intestines. The esophagus and stomach can easily be stimulated thermally and might therefore be endowed with means for thermal perception; but why should the intestines have thermal receptors, when from birth to death they meet with almost no thermal change except from enemas, surgery, accidents or conceivably hara-kiri?

Hunger

The important word *hunger* is ambiguous. It means (a) the desire for food and also (b) a pattern of dull gnawing pain referred to the region of the stomach. The hunger pangs are almost always accompanied by the desire for food, but the converse is not true. A little food or other substance in the stomach may abolish the hunger pangs but leave the phageal desire remaining. A man who is always well-fed or who has had his stomach removed may never feel the pangs, yet often he experiences a strong urge to dine. This distinction has long been recognized. Haller vaguely suggested the duality when, in 1747, he wrote: "We are solicited to take food, as well from the sense of pain which we call hunger, as from that pleasure which is received by the taste." Magendie in 1817 separated painful hunger from pleasant hunger, noting that in fasting a man may desire food while feeling no pangs. Beaumont in 1833 drew explicitly and at length this distinction between hunger and appetite. Busch wrote in 1858: "In hunger two sensations must be distinguished," and went on to show that one is dependent upon a general condition of the nervous system created by need of the body for food, and the other upon the specific state of the stomach. Longet said in 1861 that appetite may occur without stomachic pains, and that these pains, when they do occur, are caused by appetite and are not the cause of appetite.

One would think, therefore, that there could have been no confusion on this matter, and yet there was. The question arose

as to whether hunger is a general sensation (appetite) or specific gastric sensation (the pangs). When one man argued for the seat of hunger in the stomach another met him with evidence that hunger can exist without the stomachic pains or with the vagus nerves to the stomach cut. Even Magendie and Longet, who pointed out the ambiguity, took the trouble to 'prove' that 'hunger' is not gastric in origin. For nearly a century this unnecessary verbal difficulty impeded the clarification of the problem of hunger, for Cannon and Carlson had to make the argument about dual meaning all over again in 1912, and Cannon found it necessary to keep insisting upon the distinction for twenty years thereafter. In this case, as in so many others, words have obscured ideas.

It follows, of course, that the main distinction in theories of hunger arose along these lines: is hunger a general or a specific sensation?

Magendie in 1817 lent the weight of his opinion to the theory that hunger is general. "Hunger is produced like all other internal sensations by the action of the nervous system, and it has no other seat than this system itself, and no other cause than the general laws of organization." If the body needs aliment, its whole state is affected, and the nerves bear information of this state to the brain—that is what Magendie thought. The 'seat' of hunger is in the whole body or in the nervous system or, if you please, in the brain. The sensation, as regards its origin, is general—if, indeed, such hunger can be sensation.

In 1829 Sédillot showed that section of the vagus nerves in animals does not abolish the desire for food, thus throwing his support to the theory that hunger is general by proving that stomachic sensations are not essential to appetite. Indirectly, Tiedemann in 1830 also supported the notion that hunger is not specific by discussing in great detail the needs of animals and plants for nutriment and their ways of getting it without (at least in plants) the functioning of a stomach. Similarly Busch in 1858 described the case of a woman with a fistula just below the duodenum. Her intestine was entirely separated and no food from her stomach could be assimilated until later when Busch had connected the two parts with a rubber tube. She lacked, consequently, nourishment and was obsessed by an inordinate desire for food, even when her stomach was filled with food; but she felt no hunger pangs with a full stomach. Longet in 1852 argued that hunger is need, like the

need for sleep or sexual need. In 1897 Roux said that hunger arises from every cell in the body, for it is a sign of starvation, whereas the stomachic sensations are merely a consequence of such hunger. Bardier, in his sophisticated review of the theories of hunger in 1903, supported a similar theory, stating, however, that this general hunger is unpleasant, whereas appetite is pleasant. Even as late as 1915, L. R. Müller insisted that hunger arises, not from the stomach alone, but from all the organs of the body, which determine a condition of the blood that has an effect in the brain. After that the work of Cannon and Carlson was sufficient to make the distinction between sensory pangs and motivated desire fairly clear, although some confusion still arises because the sense-physiologists use the word *hunger* to mean the stomachic pangs and the animal psychologists use the same word for the hunger drive, which is the modern successor of the general sensation.

The theory that hunger is of central origin, a 'brain sensation,' cannot be separated from the theory of hunger as a general sensation. Magendie, thinking that there must be a center for hunger in the brain, and having, of course, to say how this center is excited, appealed, as we have seen, to the action of the nerves (and perhaps also of the depleted blood) upon the center. Others later held similar views, like L. R. Müller, who in 1915 echoed Magendie's theory, pointing to giddiness and weakness in starvation as evidence of the widespread origin of hunger and noting that the hunger contractions of the stomach may thus be caused by the excitation of a center in the brain.

This theory of hunger as a general sensation with appropriate central excitation has never been refuted, except as modern motivational psychology has made it clear that *sensation* is a poor word to apply to a drive. How could it be refuted? Hunger as a drive is known to exist without the stomachic pangs; animal psychologists make full use of such hunger as an incentive. The present book, however, is concerned with the nature of the pangs, and to those theories we now turn.

There have been five important theories of hunger as a specific stomachic sensation, and we may consider them in what seems to be the order of their antiquity.

(a) *Hunger is caused by the contractions of the stomach which stimulate the gastric mucosa mechanically.* This was Haller's view in 1747 and 1764. The proximate cause of hunger is "the grinding

or rubbing of the delicate and villoid folds of the gastric mucosa against each other, through a motion or contraction inherent in the stomach, aided by the diaphragm and the abdominal muscles." To this statement he added the remarks that he considered the facts already demonstrated, that the empty stomach is so contracted that no lumen exists in it any longer, that its normal state may be restored by congestion of the blood, and that hunger is lessened in prolonged fast because the contractions are diminished. He noted further that in hunger "the naked villi of the nerves on the one side [of the stomach] grate against those of the other, after a manner almost intolerable. Thus we are effectually admonished of the danger ensuing from too long abstinence or fasting, and excited to procure food or nourishment by labor and industry." It is important to note that the earliest theory of the hunger pangs, like the latest, ascribes them to stomachic contractions.

(b) *Hunger is caused by the gastric juice which stimulates the mucosa chemically.* Haller himself suggested that gastric secretion may be an auxiliary stimulus to hunger, but it was Soemmerring in 1801 who argued that this action is the sole cause of the hunger pangs. This theory is wrong in the sense that the hunger contractions of Cannon and Carlson, giving rise to hunger pangs, occur without gastric secretion; yet there is a speck of truth in Soemmerring's view because hyperacidity may, especially when there is a gastric ulcer, give rise to pains that some patients have occasionally mistaken for hunger. Once a man complained that, not being always able to tell hunger from indigestion, he could not be sure whether to eat or fast.

(c) *Hunger is caused by the quiescence of the normally active stomach.* This is a logical, if incorrect, view—that the stomach should relax when it has nothing to do: Erasmus Darwin first made the statement in 1801. Johannes Müller formulated it again in 1838: "the aliment is an 'adequate' or 'homogeneous' stimulus to the digestive organs; when this stimulus is wanting, the state of the organ is made known to the sensorium by the nerves." Müller has been said to have equated hunger to a "negative sensation," the absence of the perception of the normal aliment, but that criticism has no more validity than the argument that black is not a sensation because it is due to the absence of light. The trouble with Müller's theory was only that it turned out to be wrong.

(d) *Hunger is caused by the turgescence of the gastric mucosa.*

This is Beaumont's vascular theory of 1833. He argued against Haller's friction theory, holding that the hunger pangs—but not appetite—would be caused by the distension of the vascular vessels, or possibly of the gastric glands, in the stomach.

(e) *Hunger is caused by certain slow rhythmical contractions of the stomach which, having a specific temporal pattern, can be identified as hunger contractions.* This is Cannon's discovery of 1912, which turned out to be right. We need therefore to examine its development in some detail.

The theory had been anticipated, in a way, by Haller and E. H. Weber. Haller, as we have seen, held that the hungry stomach is not quiescent but in continuous contraction, a fact that he regarded as fully demonstrated in 1764. He was wrong, however, in supposing that the pangs arise from the rubbing of the folds and villi upon one another. The pangs are muscular, not tactual, sensations; contraction, not contact, is their adequate stimulus. Weber said in 1846 that "the sensation which we call hunger" is due to "strong contraction of the entirely empty stomach, which completely obliterates the gastric cavity," but he did not detail his evidence.

In 1892 Nicolai showed that hunger is allayed by the introduction of all sorts of substances into the stomach: food, water, saline solution, indigestible material or a stomach tube. It is also inhibited by the act of swallowing. He knew nothing about the contractions, however.

Boldireff, a pupil of Pavlov's using Pavlov's methods, studied in 1905 the gastric contractions of starving dogs. He introduced a rubber balloon into the dog's stomach so that the contractions could be recorded on a kymograph—the first use of this technique. His paper stressed the periodicity of all alimentary functions, describing the hunger contractions, but without indicating that they are related to the periodic hunger pangs. He noted further that the contractions are abolished by gastric juice in the stomach.

In 1910 Cannon and Lieb verified Nicolai's observation that swallowing inhibits the stomachic contractions.

Then in 1912 Cannon with A. L. Washburn, using Boldireff's technique on Washburn as a subject, established the correlation between the hunger pangs and these slow contractions. Fig. 89 illustrates their method. With a balloon in the stomach its con-

tractions were recorded on the kymograph, while Washburn pressed a key to indicate on the same record the appearance and disappearance of the pangs. Another tracing recorded the movement of the abdominal muscles to show that they are not the cause of the contractions or the pangs.

Immediately Carlson verified this discovery and amplified it. He had available a patient with a gastric fistula whose stomach could be reached directly through an opening in the abdominal

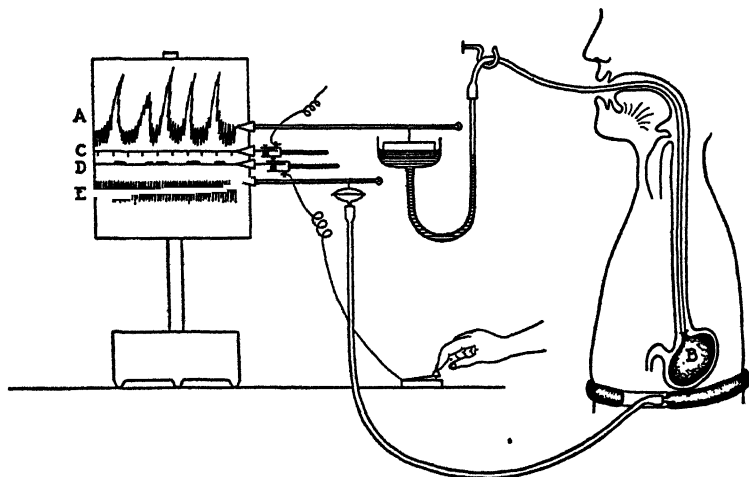


FIG. 89. HUNGER CONTRACTIONS OF THE STOMACH: CANNON AND WASHBURN (1912)

A: kymograph record of volume of stomachic balloon, B. C: time record in minutes. D: record of subject's signals, showing when he felt hunger pangs. E: record of contractions of abdominal muscles from pneumograph about the waist, showing that the hunger contractions are not contractions of the abdominal muscles. From Cannon, in *Foundations of Experimental Psychology*, 1929, 437.

wall. Carlson noted that the synchronism of hunger with contractions did not quite prove that it is the contractions that cause hunger. If the hunger had been of central origin it might have been the cause of the contractions. Carlson, however, supplementing Nicolai's finding, made out a list of inhibitors of the contractions: sapid substances in the mouth, chewing, smoking, swallowing, and the ingestion of warm water, cold water, coffee, tea, beer or wine. When these events occur, the contractions stop and the

pangs disappear. Hence the contractions are the cause of the hunger sensation.

Since then there have been many verifications of these facts, most of them by Carlson and his pupils. Rogers and Martin in 1926 also verified them, using X-ray projections to observe the contractions instead of the balloon. There is now no doubt that the contractions cause the pangs. Do they also cause the hunger drive?

In respect of this point Wada showed in 1922 that restlessness and activity tend to accompany the hunger contractions. A subject sleeping with a balloon in his stomach tends to move and toss when the contractions occur. For the waking subject the balloon indicates that the hunger contractions are apt to be accompanied by maximal effort in muscular performance, and maximal achievement in intellectual tests. Richter in 1927 found that rats, accustomed to being fed at two-hour intervals, are maximally active at these times even when no food is given them. He drew the conclusion that the hunger contractions are therefore the seat of the hunger drive as well as of the hunger sensation. Others have made the same assumption, but such a broad conclusion seems to ignore the history of the knowledge of hunger as an ambiguous concept. If the hunger drive depends upon hunger contractions, then most food-seeking must be under the direction of some other drive. It is also not clear that hunger pangs act differently from the other mild pains or potential distractors which are known to spur attention and effort.

Nowadays hunger is slipping back to its alternative meaning, to the denotation of what Beaumont and Cannon and Carlson called *appetite*. The change from *sensation* to *drive* has come about gradually in animal psychology. At first, around 1900, hunger was simply the incentive used to get animals to solve a problem. In 1908 M. F. Washburn in her *The Animal Mind* listed hunger as a "physiological condition" under which behavior changes, and her account in 1917 was similar. Watson classified hunger in 1914 as a "stimulus to activity." Miss Washburn in 1926 called it a drive or incentive, and in 1936 included it in a chapter on drives and incentives. It was always obvious that the degree of hunger can be measured by the time for which an animal is deprived of food, and Tolman and Honzik in 1930 used this quantitative definition to specify the strength of the hunger drive. In 1934 Elliott meas-

ured degree of hunger inversely by the amount of the ration fed the animal, and Skinner determined the strength of hunger by the rate at which the animal eats. In all this work on animals hunger is what Beaumont called *appetite*. Food deprivation determines and strengthens it. Rate of eating measures it. Whether the stomach contracts or not is largely irrelevant. An animal with insufficient food can be trusted to try to get food even though his hunger contractions are inhibited by any of the means which Carlson listed, just as a man can be trusted to go on with a dinner after the soup has stopped his hunger contractions.

Thirst

The history of the psychophysiology of thirst has, indeed, little to teach us that we have not already learned from the study of the psychophysiology of hunger. Thirst as a 'sensation' has always been described as dryness of the mouth and throat. It is not present when the mouth is wet, but it is not relieved for any length of time unless water is introduced into the body. Loss of water from the body by sweating or excessive secretion induces thirst. Introduction of water into the intestines or the veins without wetting the mouth reduces thirst. Hence there have been those who argued that thirst is a general sensation, in spite of the localization of the sensation in the mouth. Nevertheless, anesthetization of the oral and pharyngeal membranes implies a local 'seat,' since it abolishes thirst without water. The confusion here is exactly analogous to the similar dilemma in the case of hunger. Cannon has, however, advanced the problem somewhat farther than anyone has yet done for hunger, for he has shown why the mouth becomes dry when the water content of the body is diminished, whereas the cause of the hunger contractions is still uncertain.

Much less has been written about thirst than about hunger; yet the standard sources all discuss it, at least briefly. Haller, in 1747, characterized thirst as dryness of the tongue, fauces, esophagus and stomach. He said that it would be relieved by an increase of the water in the blood which would restore the moisture and secretion to the mouth. He noted how perspiration increases thirst, how acid liquors relieve it more than plain water because they provoke secretion of the humours.

Dupuytren and Orfila (both ca. 1821) showed that introduction

of water into the veins of thirsty dogs abolishes the desire of the animals to drink. The great Claude Bernard in 1856 described an experiment on a dog with a gastric fistula. When this dog, after being deprived of water, was permitted to drink and the water was allowed to run out of the stomach through the fistula instead of into the intestine, the animal kept on drinking and drinking until (apparently) too tired to continue, whereas, when the water was allowed to pass into the intestine, the dog soon satisfied himself. Another important case was described by Lepidi-Chioti and Fubini in 1885, who told about a boy with polyuria who passed great quantities of urine. Unless he was allowed to drink constantly, he became tormented by a severe thirst. In 1931 Gregersen verified experimentally what Haller had remarked from common observation, that profuse sweating increases thirst. He also showed (1932) that animals do most of their drinking right after eating, explaining this phenomenon as due to the loss of water in the digestive secretions. The water is secreted in the various digestive juices, is mixed with the food, and is presumably not entirely reassimilated in the intestines. There seems not to have been for a century at least much doubt that the primary cause of thirst is lack of water in the bodily system.

There can be no doubt that the thirst drive is more closely associated with the thirst 'sensation' than the hunger drive is associated with the hunger pangs. Lepidi-Chioti and Fubini showed in 1885 that the thirst of their patient was immediately relieved by painting his throat with cocaine. Valenti found in 1910 that dogs, deprived of water for several days, refused to drink when the backs of their mouths had been cocaineized. Nevertheless, there is an appetitive thirst. The present author discovered in 1915 that mild thirst is consciously little more than "going to get a drink." The thirsty person finds himself with his hand on the faucet or on the knob of the door that leads to the faucet, when he remembers that he is performing an experiment and is not allowed to drink. Winsor discovered in 1930 that a man, deprived of water, may presently develop a strong craving for liquids, even though dryness of the throat and thus the thirst 'sensation' are prevented by the salivation due to the constant chewing of tasteless gum. He called the oral sensory thirst "false thirst," and the craving that develops with a moist mouth and dehydrated system "true thirst." The distinction here between 'false' and 'true' is analogous to the

question as to whether hunger is the pangs of the gastric contractions or the appetite of desire for food.

The saliva furnishes the link between the water content of the body and the moistness of the mouth. In 1852, Bidder and Schmidt, after tying the salivary ducts in a dog, found that he was always ready to drink unless his mouth was kept moist by being kept closed. A dry mouth stimulates salivary flow. Zebrowski showed in 1905 that more saliva is secreted when the mouth is open than when it is closed, and Pavlov in 1902 that salivary secretion is greater for dry food than for moist.

Cannon took up the problem of thirst in 1918. He measured salivary secretion by collecting the saliva when tasteless gum is chewed for five minutes. By this method he found that deprivation of water increases salivary flow and thirst very little in the first four hours, but that thereafter salivation diminishes and thirst increases. When the organism in this state ingests water, the original satisfied condition is rapidly restored. He also verified the fact that increased perspiration—the body was wrapped in warm blankets—reduces the flow of saliva and increases the thirst. When the saliva was almost stopped by a subcutaneous injection of atropine, thirst became intense although the water content of the body remained, of course, unchanged. Strong emotion too may inhibit salivary flow and create a thirsty mouth. In fear the tongue cleaves to the roof of the mouth because the saliva is diminished and sticky. In the ancient ordeal of rice, the fearful person, who, because of his dry mouth could not swallow the rice, was adjudged guilty. Aeneas in fear, could not speak: "*Vox haesit in faucibus.*" He needed more saliva.

There have been various confirmations of Cannon's conclusion about thirst since he first made it, but they have not changed the essential picture.

As to the theories of thirst there have been, of course, those who contended that thirst is a general sensation. Schiff (1867) was one of the first to make this argument as he made it also about hunger. He held that the pharyngeal sensations are no more of the essence of thirst than are the sensations of heavy eyelids of the essence of sleepiness. Mayer in 1900 reasoned that deprivation of water produces thirst by increasing the osmotic pressure of the blood, but Wettendorff (1901) pointed out that osmotic pressure is raised much more slowly than the thirst comes on. L. R. Müller

(1924), who also held that hunger is of central origin, proposed that thirst must depend on the direct stimulation of a brain center by the state of the blood. These theories may be correct as of appetitive thirst which occurs when the oral cavity remains moist, but Cannon's view would still seem to be correct for strong thirst with the sensory accompaniment that occurs when the oral membranes are too dry.

In general, then, it would appear that the need of the body for water is more often accompanied by the oral sensations of thirst than is the need of the body for food by the stomachic hunger pangs. Although many people are so well fed as never to feel the hunger pangs, few are so well watered as never to experience thirst. On the other hand, most ordinary drinking under the conditions of civilization is appetitive with a fairly moist mouth. A man may get himself a drink with little consciousness of a dry mouth; nor did Cannon find much diminution of salivary flow in the first four hours of his experiment.

Organic Sensory Patterns

There were two characteristics of the thought of the nineteenth-century psychologists which affected their attitudes toward the problem of organic sensation. The first was that they were looking, with one eye on the chemists' periodic table of elements, for new sensory qualities. Touch was resolving itself into pressure, pain, warmth and cold. Might not hunger or dizziness or sex, they wondered, reveal still other qualities, new 'sensations'? The second characteristic was that they expected conscious needs to be represented by specific sensations. If the organism consciously seeks food, it must know—that was the lesson of British empiricism—that it needs food, and what more natural than that an important special need should have its special sensation, just as pain, as they thought, gives knowledge of danger? Thus they held to a poorly recognized isomorphism between need and sensation, between function and quality, and it seemed to them as if salt-hunger or sex-hunger must somehow have each its own peculiar quality, such that the mind, apprehending, as it were, a specific nerve energy, would know what it had to get its organism to do.

A great many of the physiological states and needs were recognizedly conscious: thirst, hunger, nausea, the calls to defecation

and urination and the processes themselves, sex, dizziness, and suffocation. Other perceptions—not new qualities—were added from time to time to the common list.

Meumann in 1909, on the basis of his own introspection and an examination of the clinical literature, argued that, besides hunger, both emptiness and repletion are recognizable stomachic states. To them he added oppression as a cardiac sensation and suffocation as a pulmonary sensation, but it is true that he was not primarily concerned with the question of quality so much as with the discriminability of these perceptions. The present author in 1915 sought to analyze thirst, hunger, nausea, and the excretory needs and processes by arranging the proper situations and obtaining introspective descriptions for them. Those results revealed various unique spatial-temporal patterns made up of the familiar qualities—deep pressures, deep pains and sometimes warmth and cold.

In 1915 Carlson formed the opinion that appetite has its peculiar sensory basis. He and Braafladt, demonstrating the varied sensibility of the gastric mucosa, noted that the introduction into a hungry stomach of “moderately cold water, beer, wine, weak acids or weak alcohol” not only stopped the unpleasant hunger contractions but also gave rise to a pleasant “characteristic sensation,” which was “the exact opposite of that caused by the hunger contraction” and which stood out from the hunger pangs by “successive contrast.”

Miss Luce and the present author tried to confirm this finding of Carlson's in 1917. They set hungry subjects before meals, inhibited their hunger contractions with a little food, and bade them introspect and eat and introspect again. Some of their subjects, it turned out, were pleasantly aware of the food when it reached the stomach, thus confirming Carlson on the sensitivity of the gastric mucosa. The bright pressures involved in these perceptions were, however, neither unique nor universal. The chief characteristic of appetite seemed to be a serial behavioral pattern. The need for food expressed itself first in a restlessness of the subject's hand to take the food; then, if the hand took the food, in its tendency to move it toward the mouth. Thereafter the other events of ingestion ran off smoothly. With the food at the mouth the saliva flows and the lips open, the food goes in, mastication ensues, the food passes to the back of the mouth, deglutition follows. When

Carlson's 'sensation' of appetite was felt at all, it came merely as the final perception in this chain of events. In other words, the experiment revealed a need, which was satisfied by a series of actions, most of which came to consciousness as perceptual patterns of the usual somesthetic qualities. Just as mild thirst is going to get a drink, so appetite is merely taking and ingesting food.

Such is the story of one defeat of nineteenth-century elementism. Its atoms—at least what it hoped might prove to be atoms—turned out to be complex molecules; the plausible qualities of organic sensibility were unmasked as needs. In the twentieth century it eventually became apparent that the organism behaves first and feels afterward—feels its behavior, just as James, speaking of emotion, said it does.

Although recapitulations are tedious, perhaps decency requires that we should now write epitaphs for eleven organic qualities. Here they are:

(1) *Thirst*, as we have seen, is the perception of the dryness of membranes at the back of the mouth (Cannon, 1918) and consists of a pattern of pressures and warmth, with pain added when the thirst is extreme (Boring, 1915). A mild thirst is less conscious than behavioral, being primarily the automatic act of getting a drink (Boring, 1915). There may also be in extreme dehydration a behavioral 'thirst,' even though the mouth be wet (Winsor, 1930).

(2) *Hunger* is the perception of the hunger contractions (Cannon, Carlson, 1912) and consists of a pattern of muscular pressure and muscular pain (Boring, 1915). Behavioral hunger without the pangs is in these pages called *appetite*.

(3) *Appetite* is the perception of the normal process of getting food and ingesting it (Boring and Luce, 1917), and occasionally this process ends in pleasant bright pressures from the gastric mucosa (Carlson and Braafladt, 1915).

(4) *Emptiness* of the stomach can be perceived (Meumann, 1909), but the pattern is most easily recognized when it includes other factors, like general muscular weakness.

(5) *Repletion* can also be perceived (Meumann, 1909).

(6) *Nausea* can be perceived, and includes gastric pains, which may in exceptional circumstances be confused with hunger, and, when extreme, the pressures and aches of incipient or actual vomiting; but it is also attended, when strong, by dizziness, blurring of

vision, muscular weakness, circulatory disturbances, sweating, goose-flesh and cold (Boring, 1915). Any one of these secondary phenomena may come by itself to mean nausea to the person who thinks much about his own digestive processes.

(7) The *excretory needs and processes* are all perceived as patterns of pressures and sometimes pain (Boring, 1915).

(8) *Sexual needs and processes* have not come under controlled experimental introspection, but the evidence is that they too are perceptions like the others (Titchener, 1910), perhaps with pleasant pain more involved in the male process than has usually been supposed. The histological search for lust receptors has not led to certain results.

(9) *Dizziness*, as we have seen, is the kinesthetic perception of reflex processes in the eyes, neck, arms, viscera and vascular system induced, it would seem, by stimulation of the ampullar organs of the semicircular canals (Griffith, 1920).

(10) That *cardiac oppression* is perceived is the evidence of clinical observation (Meumann, 1909), but there is no reason to suspect a novel quality here, and the location of the receptors is not established.

(11) *Suffocation* is the perception of the behavior of the organism in its effort to get more air (Meumann, 1909).

So died many extra conscious qualities. If to Aristotle's five we had added three more for the skin, and some others for proprioception, then these eleven might have been, let us say, the twelfth to the twenty-second 'senses'—had they lived. Instead they underwent such metamorphosis that we are now pretty sure that twentieth-century psychology is not like nineteenth-century chemistry.

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Notes

In general see the accounts of internal (mostly kinesthetic) sensibility in the handbooks: C. S. Sherrington, The muscular sense, in E. A. Schäfer's *Text-Book of Physiology*, 1900, II, 1002–1025; W. Nagel, Die Lage-, Bewegungs- und Widerstandsempfindungen, in his own *Handbuch der Physiologie des Menschen*, III, 1905, 734–806; L. Luciani, Sensibility of the internal or-

gans, in his *Human Physiology*, IV, trans. 1917, 57–125; E. von Skramlik, Psychophysiologie der Tastsinne, *Arch. ges. Psychol.*, Ergbd. 4, 1937, 511–898, which is especially full for the muscle sense and the perception of posture. All these accounts are well documented with references to the literature. To them must be added the excellent review by V. Henri, *Revue générale sur le sens*

musculaire, *Année psychol.*, 5, 1898, 399-557, which contains an annotated bibliography of 391 titles.

Deep Sensibility

If this term is limited, as it was by Head, to the sensibility of the *tela subcutanea*, thus excluding the muscles and articular mechanism, then the proper reference is H. Head, W. H. R. Rivers and J. Sherren, The afferent nervous system from a new aspect, *Brain*, 28, 1905, 99-115; Rivers and Head, A human experiment in nerve division, *ibid.*, 31, 1908, 323-450, esp. 355-367; both reprinted in Head, *Studies in Neurology*, 1920, I, 55-65, 225-329, esp. 246-256.

Muscle Sense

On the history of the muscle sense, see William Hamilton, *Works of Thomas Reid with Notes and Dissertations*, 1 ed., 1846, 867-869, or same pp. in 3 ed., 1852; V. Henri, *op. cit.*, 1898, 408-413; Luciani, *op. cit.*, 88-100.

Explicit proponents of muscular sensibility before Bell were J. G. Steinbuch, *Beitrag zur Physiologie der Sinne*, 1811, [n.v.]; M. F. X. Bichat, *Anatomie générale appliquée à la physiologie et à la médecine*, 2 ed., 1812, II, art. 3, sect. 2, pp. 263-266, [n.v.], and T. Brown, *Lectures on the Philosophy of the Human Mind*, 1820, I, 496-503 *et passim*. The citation of Bichat is due to Henri: and the passage does not occur in Bichat's 1 ed., 1801.

Charles Bell's most important paper on this topic is On the nervous circle which connects the voluntary muscles with the brain, *Phil. Trans.*, 1826 (ii), 163-173. See also his *The Hand: Its Mechanism and Vital Endowments as Evincing Design*, 1833, [n.v.], which is Bridgewater Treatise No. IV on the Power, Wisdom and Goodness of God as Manifested in

the Creation. In 2 ed., 1835, the muscular sense is treated on pp. 145-156, a section which gives the illustration of the mother with the anesthetic arm, mentioned in the text. On Bell and the significance of his contribution, see L. Carmichael, Sir Charles Bell: a contribution to the history of physiological psychology, *Psychol. Rev.*, 33, 1926, 188-217, esp. 204-209. On the Bell-Magendie law, see Carmichael, *op. cit.*, 191-198; E. G. Boring, *A History of Experimental Psychology*, 1929, 35-38, 44.

On the *Muskelsinn* after Bell, see J. Müller, *Handbuch der Physiologie des Menschen*, 1838, Bk. 5, sect. 5 (pp. 1329-1331 in Eng. trans.); E. H. Weber, Der Tastsinn und das Gemeingefühl, in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 580-584 (pp. 144-150 in the 1905 reprint).

On the description of the muscle spindles by W. Kühne in 1863, *vide infra* under Kinesthesia.

Sensations of Innervation

On the history of sensations of innervation, see William Hamilton, *op. cit.*, 1846 or 1852, 864-867; H. C. Bastian, *The Brain as an Organ of the Mind*, 1880, 691-700. For a general discussion, see W. James, *Principles of Psychology*, 1890, II, 492-522; Luciani, *op. cit.*, 100-106.

Henri said that the doctrine of sensations of innervation was begun by Steinbuch, *op. cit.*, 1811, 30-76, [n.v.], quoting him at length, but it seems from these quotations as if Steinbuch's *Bewegungsempfindungen* resembled more nearly the ideas of ideomotor action than sensations of innervation. Others who believed in the sensations of innervation, or at least entertained the hypothesis favorably, were: J. Müller, *Handbuch der Physiologie des Menschen*, Bk. 5, 1838, sect. 5 (Eng. trans., II, 1829 f.); Hamilton, *loc. cit.*; C. F.

W. Ludwig, *Lehrbuch der Physiologie des Menschen*, I, 1852, 359–362, esp. 361; W. Wundt, *Vorlesungen über die Menschen- und Thierseele*, 1863, I, 221 f.; *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, 276, 316; 6 ed., II, 1910, 30; A. Bain, *The Senses and the Intellect*, 2 ed., 1864, 92–95 (not in 1 ed., 1855); H. Helmholtz, *Handbuch der physiologischen Optik*, III, 1866, 598 f., 605 (3 ed., III, 1910, 203 f., 209; Eng. trans., III, 1925, 244 f., 250); G. H. Lewes, *Motor-feelings and the muscular sense*, *Brain*, I, 1878, 14–28 (he also argued against the Bell-Magendie law and invented the concept of *recurrent impulse* in order to get sensations from motor nerves); E. Mach, *Analyse der Empfindungen*, 1886, 57 f., 65–69 (somewhat modified, however, in later eds.).

As finding sensations of innervation an unnecessary addition to the muscle sense, see O. Landry, *Traité complet des paralysies*, I, 1859, 213–316; H. C. Bastian, On the “muscular sense” and the physiology of thinking, *Brit. med. J.*, 1869 (1), 394–396, 437–439, 461–463, 509–512; *The Brain as an Organ of the Mind*, 1880, 541–547, 691–700; D. Ferrier, *Functions of the Brain*, 1876, 219–229; G. E. Müller and F. Schumann, Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, *Arch. ges. Physiol.*, 45, 1889, 37–112, esp. 80–91; James, *loc. cit.*; E. B. Delabarre, *Ueber Bewegungsempfindungen*, 1891, 10–31.

Kinesthesia

On the substitution of the word *kinesthesia* for *muscular sense*, see H. C. Bastian, *The Brain as an Organ of the Mind*, 1880, 540–544, esp. 543.

J. G. Lehmann, *De consensu partium corporis humani*, 1741, [n.v.], is said to have described the dis-

covery of the Vater-Pacini corpuscles (still sometimes called *Vater'sche Körper*) by Abraham Vater (1684–1751). I do not know whether Vater had published his findings earlier or not. Filippo Pacini (1812–1883) gave the full account of his rediscovery in his *Nuovi organi scoperti nel corpo umano*, 1840, [n.v.]. See, on the contributions of the two men, G. Herbst, *Die Pacini'schen Körper und ihre Bedeutung*, 1848, [n.v.]. On the localization of these corpuscles at the joints, see A. Rauber, *Vater'sche Körper der Bänder- und Periostnerven und ihre Beziehung zum sogenannten Muskelsinne*, 1865, [n.v.]; *Untersuchungen über das Vorkommen und Bedeutung der Vater'schen Körper*, 1867, [n.v.].

For A. Kölliker's description of the *Nervenknopschen*, which he thought were growth centers, see his Ueber die Endigungen der Nerven in den Muskel des Frosches, *Z. wiss. Zool.*, 12, 1863 (the paper was read Sept. 5, 1862), 149–164. For W. Kühne's further description of the buds, the assignment of the name *muscle spindles* to them, and his correct guess as to their function, see his Ueber die Endigung der Nerven in den Muskeln, *Arch. pathol. Anat. Physiol. klin. Med.*, 27, 1863, 508–533, esp. 526–529; Die Muskelspindeln, *ibid.*, 28, 1863, 528–538. The second paper is the complete account. A. Ruffini proved the spindles neural in Sulla terminazione nervosa nei fusi muscolari e sul loro significato fisiologico, *Atti Accad. Lincei*, Cl. sci., 5 ser., 1, 1892, sem. 2, 31–38; French trans., *Arch. ital. Biol.*, 18, 1893, 106–114. C. S. Sherrington proved them sensory in On the anatomical constitution of nerves of skeletal muscles with remarks on the recurrent fibres in the ventral spinal nerve-root, *J. Physiol.*, 17, 1894, 211–258, esp. 237–248. For the recent evidence on the functioning of these spindles as stretch-receptors, see J. F. Fulton and J. Pi-

Suñer, A note concerning the probable function of various afferent end-organs in skeletal muscle, *Amer. J. Physiol.*, 83, 1928, 554-562; B. H. C. Matthews, The response of a single end organ, *J. Physiol.*, 71, 1931, 64-110; The response of a muscle spindle during active contraction of a muscle, *ibid.*, 72, 1931, 153-174. On the history of this discovery, see A. Kolliker, *Handbuch der Gewebelehre des Menschen*, 6 ed., I, 1889, 394-406; Sherrington, *op. cit.*, 238 f.; J. F. Fulton, *Physiology of the Nervous System*, 1938, 7-18, esp. 7-9.

On the discovery of the Golgi spindles in tendon, see A. Rollett, Ueber einen Nervenplexus und Nervenendigungen in einer Sehne, *Sitzungsber. Akad. Wiss. Wien, math.-naturw. Cl.*, 73 (3), 1876, 34-55; C. Golgi, Sui nervi dei tendini: un nuovo organo nervoso terminale musculo-tendineo, *Mem. Accad. Sci. Torino*, 32, 1880, 359-385; G. C. Huber and L. M. DeWitt, A contribution on nerve terminations in the neuro-tendinous nerve-organs, *J. comp. Neurol.*, 10, 1900, 159-208.

The five texts on kinesthesia and movement, published in 1887 to 1903, are: E. Kröner, *Das körperliche Gefühl*, 1887; H. Beaunis, *Les sensations internes*, 1889; E. B. Delabarre, *Ueber Bewegungsempfindungen*, 1891; A. Goldscheider, *Physiologie des Muskelsinnes*, which is his *Gesammelte Abhandlungen*, II, 1898; R. S. Woodworth, *Le mouvement*, 1903. There is an excellent summary of the status of psychology in respect of kinesthesia in E. B. Titchener, *A Text-Book of Psychology*, 1910, 160-171.

For A. Goldscheider's work, see his *Ges. Abhandl., op. cit.* The three most important papers, all reprinted there, are: Ueber den Muskelsinn und die Theorie der Ataxie, *Z. klin. Med.*, 15, 1888, 82-161 (*Ges. Abhandl.*, II, 9-89); Untersuchungen über den Muskelsinn, *Arch. Physiol.*

Leipzig, 1889, 369-502 (*Ges. Abhandl.*, II, 97-200); *idem, ibid.*, Suppl., 1889, 141-218 (*Ges. Abhandl.*, II, 201-281). The second reference deals with passive movement and includes the limens for the joints; the third paper considers weight and resistance, position and active movement.

G. A. B. Duchenne de Boulogne was the prolific author of much work on the electrical stimulation of muscle, the electrical therapy of muscular pathology, and *la conscience musculaire*, as he called it, meaning the capacity of the patient to move accurately without muscle sensations-or vision, and thus presumably in terms of sensations not in the muscles (at the joint?). See his *Paralysie de la conscience musculaire ou de l'aptitude motrice indépendante de la vue*, 1853, [n.v.], for the early evidence and the case cited in the text, and also in general his *De l'électrisation localisée et de son application à la physiologie, à la pathologie et à la thérapeutique*, 1855, [n.v.], or 2 ed., 1861, chap. 14. Also on the relation of muscular to articular sensibility in the perception of movement, see W. B. Pillsbury, Does the sensation of movement originate in the joint?, *Amer. J. Psychol.*, 12, 1901, 346-353; A. Strümpell, Ueber die Störungen der Bewegung bei fast vollständiger Anästhesie eines Armes durch Stickverletzung des Rückenmarks, *Deutsch. Z. Nervenheilk.*, 23, 1902, 1-38.

For G. E. Müller and F. Schumann on the motor *Einstellung*, see their Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, *Arch. ges. Physiol.*, 45, 1889, 37-112, esp. 42-63.

For W. Wundt's introduction of strain and relaxation into the theory of feeling, see his *Grundriss der Psychologie*, 1896 or Eng. trans., sect. 12-13; *Grundzüge der physiologischen Psychologie*, 5 ed., II,

1902, 263-369, or 6 ed., II, 1910, 274-283.

On conscious attitudes see, first, E. B. Titchener, *Lectures on the Experimental Psychology of the Thought Processes*, 1909, 157-194. Then, for the study cited in the text from Titchener's laboratory, H. M. Clarke, Conscious attitudes, *Amer. J. Psychol.*, 22, 1911, 214-249, esp. 218-221. Other representative introspective studies from Cornell and Clark Universities, all showing the importance of kinesthesia in sensationistic introspection, are T. Okabe, An experimental study of belief, *ibid.*, 21, 1910, 563-596; E. Jacobson, On meaning and understanding, *ibid.*, 22, 1911, 553-577; E. L. Woods, An experimental analysis of the process of recognizing, *ibid.*, 26, 1915, 313-387; S. C. Fisher, The process of generalizing abstraction and its product the general concept, *Psychol. Monogr.*, 21, 1916, no. 2.

Proprioception

On Sherrington and proprioception, see F. W. Mott and C. S. Sherrington, Experiments upon the influence of sensory nerves upon movement and nutrition of the limbs, *Proc. roy. Soc.*, 57, 1895, 481-488; Sherrington, On the proprioceptive system especially in its reflex aspect, *Brain*, 29, 1906, 467-482; *The Integrative Action of the Nervous System*, 1906, esp. 129-132, 336-349; J. F. Fulton, *Physiology of the Nervous System*, 1938, 7-11, 156-163.

Vestibular Sensibility

A full and detailed account of vestibular sensibility is C. R. Griffith, An historical survey of vestibular equilibration, *Bull. Univ. Illinois*, 20, 1922, no. 5, which gives a bibliography of 1701 titles from 1575 to 1921. It is an invaluable source although not always accurate as to details. See also J. G. Dusser

de Barenne, The labyrinthine and postural mechanisms, *Handbook of General Experimental Psychology*, 1934, 204-246.

Erasmus Darwin's account of vertigo is in his *Zoonomia, the Laws of Organic Life*, I, 1794, 227-239; but the fuller account of rotation and the negative apparent after-effect does not appear until the 3 ed., I, 1801, 327-356, esp. 340-347.

J. E. Purkinje's contributions to the subject of vertigo are not easily accessible. The first paper is Beiträge zur näheren Kenntniss des Schwindels aus heautognostischen [introspective] Daten, *Med. Jarhb. Oesterr. Staates*, 6, 1820, 79-125, [n.v.]. Then in 1825-1826 he had three papers read to the Breslau *Gesellschaft* and reported by its secretary: *Bull. Schles. Gesell. vaterland. Kultur, naturw. Sect.*, no. 4, 1825; no. 10, 1825; no. 2, 1826; which are also respectively *Neue Breslauer Zeitung*, ausserordentliche Beilage, no. 86, 1825; no. 8, 1826; no. 43, 1826. None of these originals have I seen, but they are all reprinted in order with these references in Y. Delage and H. Aubert, *Physiologische Studien über die Orientierung*, 1888, 116-122. The first paper is also summarized in *Jarhesber. Schles. Gesell. vaterland. Kultur*, 1825 (1826), 32 f. The fifth paper is Ueber die physiologische Bedeutung des Schwindels und die Beziehung desselben zu den neuesten Versuchen über die Hirnfunctionen, *Mag. ges. Heilk.*, 23, 1827, 284-310.

M. P. J. Flourens' discovery of the function of the semicircular canals is found in the following places. *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés*, 1824, says nothing about the canals, but gives, esp. 36-42, Flourens' account of the functions of the cerebellum, which supplements L.

Rolando's work in 1809. (On Rolando, see E. G. Boring, *History of Experimental Psychology*, 1929, 59 f., 74.) Flourens' first experiment is in his *Expériences sur le système nerveux*, 1825, 44 f., [n.v.], which is abstracted (or reprinted?) in *op. cit. infra.*, 1842, 452 f. The full accounts of the experiments performed in 1828 are *Expériences sur les canaux semi-circulaires de l'oreille dans les oiseaux (pigeons and other birds)*, *Mém. Acad. Sci. Paris*, 9, 1830, 455-466 (reprinted in *op. cit. infra.*, 1842, 454-468); *Expériences sur les canaux semi-circulaires de l'oreille dans les mammifères (rabbits)*, *ibid.*, 467-477 (reprinted in *op. cit. infra.*, 1842, 469-482). Thus one finds the full account in the 2 ed., *op. cit. supra*; *Recherches expérimentales sur les propriétés et les fonctions du système nerveux*, etc., 2 ed., 1842, 438-501.

There were other early papers on equilibration but no important contribution. One of the most complete, following Purkinje and not mentioning Flourens, was M. H. Romberg, *Zur Lehre von dem Schwindel*, *Wochenschr. ges. Heilk.*, 2, 1833, 1057-1070.

C. Harless reported verifying Flourens' experiments in R. Wagner's *Handwörterbuch der Physiologie*, IV, 1853, 422 f. C. E. Brown-Séquard described his experiments with frogs in his *Experimental Researches Applied to Physiology and Pathology*, 1853, 18-23, 99 f. J. Toynee reported the cessation of vertigo with the extrusion of the entire inner ear of a human subject in Vestibule, cochlea and semicircular canals extruded during life, *Trans. path. Soc. London*, 17, 1866, 272 f.

F. Goltz's most important paper is *Ueber die physiologische Bedeutung der Bogengänge des Ohrlabyrinths*, *Arch. ges. Physiol.*, 3, 1870, 172-192, esp. 187-190. E. Hitzig's paper on electrical stimulation is *Ueber die beim Galvanisiren des*

Kopfes entstehend Störungen der Muskelinnervation und der Vorstellung vom Verhalten im Raume, *Arch. Physiol. Leipzig*, 1871, 716-770. A. Lowenberg's evidence that section of the canals is excitatory in function is in his *Ueber die nach Durchschneidung der Bogengänge des Ohrlabyrinths auftretenden Bewegungsstörungen*, *Arch. Augen. Ohr-heilk.*, 3 (1), 1873, 1-12.

For E. Mach's contributions, see his *Physikalische Versuche über den Gleichgewichtssinn des Menschen*, *Sitzungsber. Akad. Wiss. Wien, math.-naturw. Cl.*, 68 (3), 1873, 124-140; 69 (2), 1874, 121-135; *Grundlinien der Lehre von den Bewegungsempfindungen* (one of the classics of experimental psychology in the nineteenth century), 1875, esp. 22-54, 97-124. Later Mach reviewed the whole problem and described his apparatus for the visual observation of small animals during their rapid rotation in his *Analyse der Empfindungen*, 5 ed., 1907, or Eng. trans., 1914, chap. 7.

J. Breuer's papers are *Ueber die Bogengänge des Labyrinths*, *Allg. Wien. med. Zeitung*, 18, 1873, 598, 606 (two short notes); *Ueber die Function der Bogengänge des Ohrlabyrinthes*, *Med. Jahrb. Wien*, 1874, 72-124, esp. 120-124; *Beiträge zur Lehre vom statischen Sinne (Gleichgewichtorgan, Vestibularapparat des Ohrlabyrinths)* (he had to define his new term, the *static sense*, in his title), *ibid.*, 1875, 87-156.

A. Crum Brown's contributions are in *On the sense of rotation and the anatomy and physiology of the semicircular canals of the internal ear*, *J. Anat. Physiol.*, 8, 1874, 327-331; Cyon's researches on the ear, *Nature*, 18, 1878, 633-635, 657-659.

The other two important theorists of the 1870's, both men who perfected the surgical method of stimulating or eliminating the canals, are E. Cyon, *Ueber die Function der halb-cirkelförmigen Canäle*, *Arch. ges.*

Physiol., 8, 1873, 306-327; *Recherches expérimentales sur les fonctions des canaux semi-circulaires et sur leur rôle dans les formations de la notion de l'espace*, 1878, esp. 38-44; and A. Böttcher, Ueber die Durchschneidung der Bogengänge des Gehörabyrinths und die sich daran knüpfenden Hypothesen, *Arch. Ohrenheilk.*, 9, 1874, 1-71.

For the early history of the belief that the canals function for hearing, see pp. 401-403; for Jaensch's belief that the vestibule functions for noise, see p. 364.

On dizziness in deaf mutes, see W. James, The sense of dizziness in deaf-mutes, *Amer. J. Otol.*, 4, 1882, 239-254; A. Kreidl, Beiträge zur Physiologie des Ohrlabyrinths auf Grund von Versuchen an Taubstummen, *Arch. ges. Physiol.*, 51, 1891, 119-150; J. Pollak, Ueber die "galvanischen Schwindel" bei Taubstummen und seine Beziehung zur Function des Ohrlabyrinths *ibid.*, 54, 1893, 188-208.

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Chapter 15

THE PERCEPTION OF TIME AND MOVEMENT

THE physiologists have always classified sensations in respect of the five sense-organs and their related neural systems—sight, hearing, smell, taste, touch. Wundt started the fashion in psychology of classification in respect of sensory dimensions—quality, intensity, space and time. He himself regarded quality and intensity as the two attributes of sensation, whereas space and time represented for him modes of complex perceptual organization. Thus the main chapter headings for about half of experimental psychology were sensory quality (the five senses), sensory intensity (psychophysics and the Weber-Fechner functions), space perception (the organization of perceived space on the basis of various sensory data), and temporal perception (a simple analogue of space perception). Modern psychology, however, has reverted to the physiologists' schema because it is no longer possible to keep the problems of quality, intensity and space separate. Hue, brightness and saturation, for instance, turn out to be interdependent variables, each differently related to the same dimensions of the stimulus. Hue and brightness are also related to visual size; pitch and loudness are not independent of tonal volume. Phenomenal intensity, moreover, nearly always appears as the intensity of a quality. Weber found space (the *Ortsinn*) fundamental to the understanding of the cutaneous mechanism. Recently auditory theory has had to become adequate to intensity (loudness) and space (localization) as well as quality (pitch). In short, modern psychology, being physiologically oriented, has to put all the sensory attributes together and to give up Wundt's distinction between sensation (quality and intensity) and perception (space and time). This book has, therefore, dealt separately with the senses, rather than with the attributes of sensation.

There are, however, many problems of the interrelation of the senses: the facilitation or inhibition of one sensory impression by

another from a different modality, the comparison of two extensions or intensities or brightnesses that occur in different modalities, the cooperation of sense-data in attention, memory, imagination and thought. These topics are omitted here. Attributive comparison across the modalities has already been considered (p. 27), and many of the other topics belong in a book on the history of the 'higher' or 'complex' mental processes.

Two general sensory problems, nevertheless, remain, each of too great historical importance to permit of omission. The one is the perception of time, the *Zeitsinn*; the other, the perception of movement. Although most of the temporal intervals investigated have been heard, and most of the movements seen, both perceptions have been considered to be, in their essentials, independent of the properties of the particular organs of sense and thus almost wholly dependent upon the action of the central nervous system. Whatever their destinies, they are still general sensory problems, and to them we address ourselves in the present chapter.

Perception of Time

Into philosophers' many theories of temporal perception we need not enter in detail. Nichols in 1891 considered more than three score of their opinions, extending his survey from Plato to William James. He made it clear that the early philosophers had little of importance to say. Time was taken by them for granted. British empiricism, however—the school that made philosophy psychological—had to meet the problem of temporal perception. They said (Hobbes in 1655, Locke in 1690, Berkeley in 1710, Hume in 1739) that the idea of time arises from the succession of ideas. The mind has a temporal dimension which gets to be known after the manner in which its other properties are known. There was, however, an error in this view. A succession of ideas is not equivalent to an idea of succession. Reid (1785) and Thomas Brown (1820) pointed out that the perception of time must, therefore, depend upon memory, that the early terms of a series must be held over if they are to unite with the later terms to form the composite of a perceived succession. James Mill (1829) made the same point, and William James (1890) observed that "our consciousness never shrinks to the dimensions of a glow-worm spark," "illuminating the point it immediately covered, but leaving all beyond

in total darkness." "The knowledge of some other part of the stream [of consciousness], past or future, near or remote, is always mixed in with our knowledge of the present thing." Thus Wundt (1874) also came to recognize a temporal range of consciousness, measured by the duration of the rhythm that remains recognizable as a unique pattern without being counted or reflected upon.

This difficulty about the introspection of a duration arose because consciousness was conceived to be 'immediately given,' that is to say, introspection was not believed to be a process that takes time: to have a perception is in itself to know that one has it—that was accepted doctrine. But how then can a duration that takes time be immediately known, since the duration, not being instantaneous, is itself not all immediate? At what time in a time does one perceive that time? That was the problem and there have been three solutions for it.

The first is the solution of the conscious present. E. R. Clay, writing in 1882, noted that the present is specious. It is merely a point without duration, where the past meets the future, and time consists consequently "of three nonentities—the past, which does not exist, the future, which does not exist, and their conterminus, the present," which, having no duration, also does not exist. Yet the "sensible present," as James called it, citing Clay, has duration, specious though that duration be. Wundt was right. A certain duration is held together as a conscious present, which is known immediately as soon as it is complete in actuality or in anticipation. This solution is satisfactory as long as one does not press too vigorously for refinement in the dating of events.

The second solution lay in the denial of the validity of such temporal analysis. Mach originated this view, when in 1866 he regarded time as a sensation, and when later, in his *Analyse der Empfindungen* (1886), he discussed both space and time as if they were sensations. In this innovation he was indirectly aided by Kant's authority which had established the analogy between time and space as a priori intuitions, with the result that whatever could be said of space might be presumed to be applicable to time too. A succession, when it is perceived as a duration, is not a string of sensation-beads any more than a perceived line is a row of perceptions. That was essentially Mach's doctrine, but it is doubtful if it ever became clear until modern Gestalt psychology effectively did away with the analytical attitude toward the prob-

lems of both extension and duration. For Gestalt psychology an extension is an extension, not a row of sensations. Similarly an intensity is an intensity, not a sum of quanta, and a duration is a duration, not a series of events. Extensity, intensity and protensity are all given just as directly as quality. This was also Titchener's final view (1915) after he had turned from sensation to attribute for his ultimate datum.

In modern positivistic psychology this problem has disappeared, and that is the third solution. The substitution of differential response for the old-fashioned dated existential consciousness does away with immediate experience. Any conscious event, for the positivist, is a relationship that occurs in time, a form of discrimination. Durations, although not instantaneous, can be discriminated, a long from a short, and as such are conscious. The process of discrimination can also be dated as accurately as any other temporal process; yet from this point of view dating is no longer very important.

The Time-Sense

In 1857 Czermak suggested that there is for duration a "general sense," a *Zeitsinn* analogous to Weber's *Raumsinn* for space. He made his point by laying down a program for the investigation of the time-sense, a program which included the determination of the shortest perceivable intervals in each of the other senses and of the perceivable rates of visual and tactual movement. He did not experiment; he only set the general problem.

Experiments were, however, easy to get under way in those days when Fechner's new psychophysics provided so many new problems in sensory physiology; and it was Mach who undertook the first experimental work, starting in 1860 and publishing in 1865. His particular project was to test the application of Weber's law to time. He had, of course, to determine the j.n.d. of duration for intervals of different lengths, and he used intervals ranging from 0.016 to 8.0 seconds, filling them with beats—that is to say, the observer heard first one series of beats and then another, being required to say whether the second series was longer or shorter than the first or of the same duration. Mach got the beats for the short intervals from a metronome, for the longer intervals from a special apparatus, and for the longest by striking an anvil by hand with a hammer while he noted the time on a watch. He found

the greatest sensitivity for an interval of 0.375 seconds, where the Weber fraction proved to be about five per cent. Since the fraction was not constant but greater for both shorter and longer intervals, he concluded that Weber's law does not apply to the perception of time.

After Mach came Vierordt and the *indifference point*. The first publication in this new work actually anticipated Mach's. It was a paper in 1864 by Höring, one of Vierordt's pupils. Höring used empty intervals, each of them bounded by two beats of a metronome, and the method of right and wrong cases, which Vierordt had invented in 1852. He employed intervals ranging from 0.3 to 1.4 seconds, finding that the constant error was least in the interval between 0.36 and 0.45 seconds, that is to say, at about 0.4 seconds. The shorter times, as he put it, were estimated as too long and the longer times as too short. The indifference point is the duration for which there is no constant error, the point where the positive errors change to negative. In this particular experiment it is not—as Woodrow pointed out much later—quite correct to say that the short times are judged too long and the long times too short, because Höring was judging the relation between two successive durations. Actually he was determining the psychophysical time-error for the judgment of successive times. He found that, for short intervals, the second duration, seeming shorter, must be physically longer than the first if the two are to appear equal, and, conversely for the long intervals, the second, seeming longer, must be shorter than the first if the two are to appear equal. Thus the first short interval is 'overestimated' as measured by the second, although it is just as true that the second is 'underestimated' as measured by the first. Höring's result was, however, interpreted in this way because such a statement is consistent with the findings for the reproduction of intervals, a method which came next.

In 1868 Vierordt himself published his *Der Zeitsinn*, the classic in this field. He used Höring's apparatus and method, but he also studied the reproduction of durations. For this purpose he arranged a metal lever with a writing point that traced a line on a kymograph. The experimenter gave first the standard interval in auditory terms by striking the lever twice with a brass rod, thus presenting the interval and also recording its duration on the kymograph. Then the subject duplicated the interval, as well as he could, by depressing the lever twice so as to mark off a second

interval on the record. Vierordt also used other variants of method, but these two procedures were the most important.

Vierordt found that the place of the indifference point varies from person to person, from time to time, and from the ear to the eye. Its total range was from 1.4 to 3.5 seconds, but the shorter times are most often cited, because they come closer to three-fourths of a second, a value which was later accepted as correct.

In respect of Weber's law, Vierordt discovered that the Weber fraction varies continuously from about 18 per cent with a positive

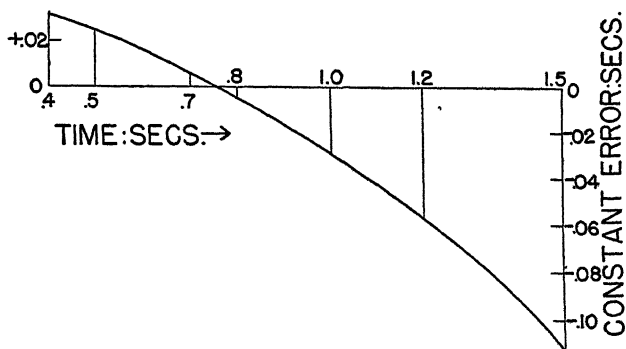


FIG. 90. POSITIVE AND NEGATIVE CONSTANT ERRORS IN THE JUDGMENT OF DURATION: KOLLERT (1882)

The indifference point (zero constant error) lies at 0.755 sec., with positive errors ("overestimation") for shorter durations and negative ("underestimation") for longer. The results are smoothed in accordance with a mathematical function. Adapted from Kollert.

error for durations under 0.5 seconds, through a minimum of about 3 per cent with little error between 1.0 and 1.5 seconds, up to a maximum of about 41 per cent in the region of 5 to 8 seconds. This result was taken as confirming Mach's conclusion that Weber's law does not hold for time.

After Wundt's new laboratory got under way in 1879, the time-sense was one of the subjects investigated there. There were important papers in Wundt's *Philosophische Studien* by Kollert, Estel and Mehner, all of which Fechner criticized. One point was that Wundt wanted the method of least perceptible differences used instead of Vierordt's method of right and wrong cases.

For this reason Kollert introduced the method of least perceptible differences in 1882. He had two metronomes, one for the

standard interval, and the other for the comparison. He used standards from 0.4 to 1.8 seconds. His intervals were empty, being each the duration between two beats of a metronome. After excluding certain anomalous results, he found an indifference point at 0.755 seconds. Fig. 90 shows the way in which his errors of 'overestimation' and 'underestimation' ran.

Then Estel in 1884 with a new apparatus elaborated and confirmed Kollert's findings. He got the indifference point at 0.75 seconds, but he also interpreted his findings as showing that the error of estimation is reduced at even multiples of 0.75 seconds—at 1.50, 2.25, 3.00, . . . seconds. His curve looks like Kollert's in Fig. 90 with a periodic curve superimposed upon it. He argued that a law of contrast is operating, that a short first standard interval makes the second comparison intervals longer by contrast, and conversely. This relationship would seem, however, to be more one of assimilation than contrast. A short first interval should make the second seem short by assimilation, so that the second has to be lengthened physically in order to seem equal to the first.

In 1885 Mehner repeated Estel's work, found an indifference point at 0.71 seconds, and interpreted his results as indicating that the error is minimal at odd multiples of 0.71 and maximal at even multiples. Glass, however, criticized in 1887 both Estel's conclusions and Mehner's, failing to confirm either kind of periodicity and showing that the evidence for periodicity was not significant as against the great variability of the data. L. T. Stevens in 1886 also failed to find periodicity and got great variability of the indifference point, although his average was close to the others (0.71 sec.).

What these men in Wundt's laboratory were hoping to find was a psychological unit of time. May there not be, they wondered, some absolute duration, near 0.7 seconds, which is always available to the mind as a standard, which is most accurately judged both in respect of estimation, being neither overestimated nor underestimated, and in respect of variability, being the point where the Weber fraction is least? James later regarded this interval as a measure of the sensible present, and nearly everyone took it to be the right interval for the warning signal before maximal attention is needed for a stimulation or a reaction. The discovery of a physiologically absolute duration was a reasonable aspiration in the days of the new psychology when the mental chronometry

of the reaction times seemed also to be providing natural durations for apperception, discrimination, cognition and choice. Nevertheless, these men were wrong. The indifference point is variable and has no regular periodicity.

Münsterberg was nearer the truth in 1889. He said that it was futile to look for fixed mental units of duration, that what we must seek is the physiological measures of the time-sense. Thus he de-

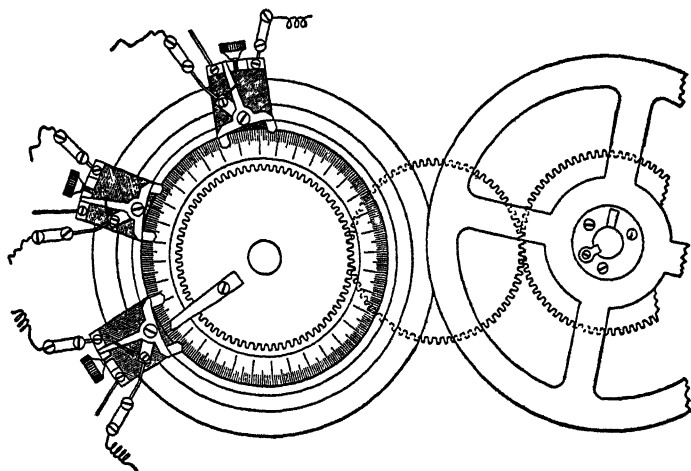


FIG. 91. APPARATUS FOR THE TIME-SENSE: MEUMANN (1894)

A kymograph (right) drives the graduated disk (left), on which the projecting finger trips the electrical contacts to operate the stimuli (sounders, lights) that limit the judged intervals.

scribed the manner in which the perception of duration depends upon the perception of the sensory tensions (*Spannungsempfindungen*) that fill the conscious interval, the tensions of the rhythm of breathing and of many other strains beside. Münsterberg was thus one of the first to emphasize the importance of kinesthesia in the conscious life (cf. pp. 533 f.).

After this came Meumann's extensive study of the time-sense (1892-1896). His papers have always been regarded as the second classic in this field after Vierordt, but the fact is that he contributed no generalization as basic as Vierordt's on the indifference point. He invented an important apparatus (Fig. 91), a disk that can be geared to the Ludwig-Baltzer kymograph (then the most accurately controlled 'clock') in such a way as to make elec-

trical contacts for any desired interval according as the contact points are adjusted on the disk. He studied the relation of the estimates of times to the intensities of the limiting sounds, to rhythm, to the difference between sound and light, and to the difference between filled and empty intervals.

We may omit consideration of the numerous investigations of the time-sense in the present century and content ourselves with noting the final outcome in Woodrow's experiments. Woodrow showed conclusively (1930) that there is nothing absolute about the indifference point. The individual differences are too large. Some of his subjects overestimated almost all the intervals that he used (from 0.2 to 30. seconds); others underestimated them. On the average, it is true that an indifference point comes out at about 0.6 seconds with his data (1934); there may be some underlying tendency of the sort that Vierordt asserted. One factor that determines the estimates—so Woodrow found—is attitude, a parameter that had not been controlled in the early experiments. He set up in 1933 two instructions. In one, the "auditory instruction," the subject was to keep his attention solely on the sound, to forget about his reacting finger, to remain unanalytical. In the other, the "strain instruction," the subject was to attend carefully to the interval between the sounds and to his reacting finger. The auditory instruction gave him almost entirely underestimation, the strain instruction almost entirely overestimation. It was as if the second instruction had filled the interval with strain and expanded it in perception, in the way that filled visual extents seem longer than empty. Thus the finding of an indifference point may mean merely that the attitude of observation tends to change with the length of the duration judged.

While it is disappointing to realize that the hopes of Vierordt and the Wundtians were unfulfilled, it is no news to remark that the discriminating organism seldom operates simply and would hardly carry around with it a temporal unit as great as three-fourths of a second. If the indifference point had been of the order of a physiological constant like the refractory period of nervous tissue, that would have been a different matter.

Rhythm

Rhythm has played a larger role in psychology than in experimentation, principally because it is involved in the problem of esthetic form, although also somewhat because there is an atmosphere of mystery about natural physiological rhythms. Of the 714 items in Ruckmick's bibliography of rhythm we need to mention less than a score here.

There is a story that G. Stanley Hall, entrepreneur and spell-binder among psychologists, travelled about the United States, shortly after Bolton had finished his experiments on rhythm at Clark in 1894, lecturing to Sigma Xi audiences on rhythm and fascinating them with the magic of his tongue. When one looks at Bolton's dull account of how men group a uniform series of clicks into natural patterns, one wonders what it was that Hall had to say, but when one examines Bolton's introduction, done in the true spirit of Hall's "synthetic psychology," one begins to understand. There Bolton discussed cosmic rhythms, physiological rhythms, the periodicity of attention and of speech, the emotional effects of rhythm on savages and children, the place of rhythm in poetry and music. Rhythm is a large subject.

In experimental psychology the investigation of rhythm has been undertaken in connection with three important problems: (1) the determination of the temporal range of consciousness, (2) the establishment of the subjective nature of auditory rhythm and its objective conditions, and (3) the decision as to the extent to which kinesthesia is required to fix a rhythm. Let us consider these matters in order.

Wundt, when he first formulated his system in 1874, conceived of consciousness as having a given spread over what he called the *Blickfeld* (focus + margin), containing within it the *Blickpunkt* (focus) of clear attention. The span of the *Blickpunkt* constituted the range of apperception, which Wundt later measured by the amount of differentiated material correctly reproducible after tachistoscopic presentation. The span of the *Blickfeld* was the range of consciousness, which Wundt thought was measured by the content of a perception which can be identified as a whole without specification of its parts. Later he employed rhythmical groups as representative of this sort of perceptual material. Six unrelated beats constitute, he believed, the maximal range of con-

consciousness, for six such beats can be perceived as different from five or seven even though they have not been counted. Grouping increases the number of beats within the range to sixteen, Wundt thought—eight pairs, or four pairs of pairs.

In 1885 Dietze's experiment in Wundt's laboratory was instituted in the interests of this theory. He provided subjects with a large number of rates of uniform beats, rates ranging from four beats per second to four seconds per beat, and tabulated the subjective rhythms that the subjects perceived. The largest group was 40 beats, five groups of eight each at the rate of four per second, a perception that makes the range of consciousness in that case 10 seconds. His longest group in respect of time was 12, three fours at the rate of 3 seconds per beat, a perception that took altogether 36 seconds.

Bolton's experiment in 1894 was similar to Dietze's, but he got no unitary group of more than eight beats. With rapid rates he found a range of consciousness of about one second; with slower rates the range averaged about 1.6 seconds. Koffka, in a very thorough study of rhythm in 1909, obtained durations of the rhythmical groups varying from 0.65 to 5.6 seconds with a mode between 1.1 and 1.6 seconds. It seems probable that Bolton's and Koffka's subjects may have adopted more rigorous criteria of unitariness than Dietze's; but the matter is not likely to come to final experimental determination, because the problem of the range of consciousness lost its motivational support when the very concept of consciousness was seen to be ambiguous and its usefulness as a systematic tool became dubious.

The *objective conditions* of rhythm have been worked out by a number of investigators. The most important conditions are relative intensity of the members of the group (accent), relative duration of the members and relative duration of the intervals between members.

Meumann in 1894, contributing a pioneer paper in this field, varied the relative loudness of sounds and concluded that a loud sound tends to lessen the subjective interval preceding it, to increase the subjective interval following it, and thus in general to conclude the rhythmic foot. A series of sounds at equal objective intervals would thus be heard as an iambic, not a trochaic rhythm, but Meumann's conclusion was not confirmed by subsequent workers.

Bolton, also in 1894, observed that any regularly recurrent contrasting members of a series tend to determine rhythmical feet, that loud sounds tend to begin the group and weak to follow, that sounds of long duration tend to end the group and weak to precede. He also found that all rhythmical feet are separated by intervals subjectively longer than the intervals within the feet, even though objectively all the intervals are equal.

Then, in 1903, Robert MacDougall found that a loud sound tends to increase the apparent duration of the interval preceding it and to decrease the apparent duration of the interval succeeding it, thus showing that Bolton's first finding depends on his second. MacDougall was able to measure the strength of this effect by reducing the objective duration of an interval immediately preceding a loud sound until he had just destroyed the rhythm—had found, as he put it, the "indifference point." If all intervals are equal, then the interval before the accented members seems longer than the interval after it; thus the accented member initiates the foot. If the interval before the accented member is made shorter than the other intervals, however, a point can be found—the indifference point—where it seems equal to the other intervals in duration. At the indifference point the rhythm is destroyed.

Miner, also in 1903, worked with visual rhythms and verified the fact that the interval between feet is always perceived as longest. He noted in addition that the duration of a light increases its apparent intensity, but that may have been in part a retinal phenomenon.

Finally Woodrow in 1909 brought this whole matter to a satisfactory conclusion. He found, in the first place, that of two-rhythms the trochee is more natural than the iambus, thus confirming Bolton and MacDougall in respect of the fact that the accented member tends to begin the foot and that the apparent interval between feet is longest.

(1) Objective ' - - ' - - ' - - becomes subjective ' - - ' - - ' - -
On the other hand, the member of longest duration tends to end the foot and thus to be followed by an interval of great apparent duration; thus:

(2) Objective - - - - - becomes subjective - - - - -
— or even ' - - ' - - ' - -

Similarly the dactyl is the natural three-rhythm and the same rules apply:

(3) Objective - ' - - - ' - - - ' - - becomes subjective - ' - - - ' - - - ' - -

(4) Objective - - - - - becomes subjective - - - - -

The lengthening of the objective interval in the middle of a trochee may, however, change it into an iambus; thus:

(5) Objective - ' - - ' - - is heard subjectively as - - ' - - ' - -

(6) Objective - - - - - is heard subjectively as - - - - -

This finding led Woodrow to measure these effects by the determination of indifference points after the method of MacDougall. For instance, if the objective interval after an accent is increased, the rhythm may fail or be reversed:

(7) Objective - - ' - - ' - - ' may become subjective - - ' - - ' - -

The third historically important problem about rhythm is the question as to whether all rhythmization demands *kinesthesia*. Again we must recall that the atmosphere of the late nineteenth century favored motor theories—motor theories of the perception of space and, by analogy, motor theories of the perception of time. Ever since Lotze (1852) there had been the suspicion that all visual extent might be given in terms of eye-movement. Wundt made kinesthesia—the “great god Kinesthesia”—basic to all space perception. Münsterberg made it basic to the perception of duration. A discovery that rhythm depends upon, or even reduces to, kinesthesia could have outraged no one. Rhythm is, moreover, a good subject for motorization. Both Wundt and Stanley Hall thought that the bilateral symmetry of the human locomotor organism provided, especially in walking, the genetic basis for rhythmical perception, making the two-rhythm more ‘natural’ than the three-rhythm.

All investigators discovered that auditory rhythms tend to be accompanied by kinesthetic accentuation—Bolton, MacDougall, Stetson, Miner, Koffka. The head, the toe or the finger tends to keep time. Titchener, after reserving judgment as to the existence of purely auditory rhythms, finally concluded (1910) that, when other kinesthesia is lacking, strains in the ear (kinesthesia from the tensor tympani, he thought) provide the accentuation. Titchener also helped the kinesthetic theory of rhythm by introducing at this

time (1909) his context theory of perception, the theory that every sensory "core" of a perception is given its meaning by the addition to it of a sensory or imaginal "context." Rhythm formed for this theory the example *par excellence*. The auditory series is the core which is marked off into groups by the addition of the recurrent kinesthetic context.

The frontal attack on this problem was undertaken by Ruckmick in 1913. He found, like his predecessors, plenty of kinesthesia. He was inclined to conclude, moreover, that some sort of kinesthesia is the invariable accompaniment of both auditory and visual temporal rhythms in the initial stage of their formation, but that the kinesthesia in an established rhythm may nevertheless drop out and the perceptual pattern continue in purely auditory or visual terms. In some cases it is a visual context that fixes the pattern for an auditory core. Thus Ruckmick helped to diminish the prevalence of motor theory in the explanation of temporal and spatial perceptions, at a time when motor theory was already being deserted and the alternative phenomenological theory of Gestalt psychology was about to be brought forward.

After all, the question of the universality of kinesthetic context for rhythm might be only a question of definition. Many perceptions are grouped by concomitant kinesthesia. Many are not. Tachistoscopic presentation of Schumann's checkerboard (Fig. 35A, p. 248) or Rubin's figures (Fig. 36, p. 251) may show grouping without eye-movement. The unitary nature of a trill that occurs in a melody does not depend upon kinesthesia of the large muscles or even of the tensor tympani. There are perceptual patterns—even recurrent temporal auditory patterns—which occur without kinesthetic context. Are they rhythms? One could define rhythm as the kinesthetic determination of visual or auditory grouping, just as readily as one could define emotion as an affective experience which includes prominent organic processes. Such a definition of rhythm would, however, narrow the concept unreasonably and require the dropping off of kinesthesia in an habituated rhythm (Ruckmick) to be interpreted as a disappearance of the rhythm. In general, the broader definition has seemed better: kinesthesia is not a *sine qua non* of rhythm.

Perceived Movement

At first the perception of movement did not seem to be a special problem. If one can localize visual and cutaneous impressions and if the localization changes continuously in time, why then one must perceive movement—or so it seemed. If a circular excitation on the retina gives rise to a seen circle, must not a moving excitation result in a seen movement? In Johannes Müller's time, and much later too, perception raised no problem—except in the illusions where excitation and experience do not exactly agree. Only then had something to be explained. It was for this reason that interest in perceived movement began with the discovery of the illusions of movement—stroboscopic movement where a discrete displacement of the stimulus gives rise to the perception of a single continuously moving object, and the negative after-sensation of movement where watching one movement makes stationary objects seem afterward to move the other way.

This history starts in the 1820's with Purkinje's phenomenological description of seen and felt movement in giddiness (see pp. 535 f.). The rotated giddy subject feels himself revolving in one direction and sees his environment spinning in the opposite direction, continuing to spin after he has stopped. Did Purkinje thus discover the negative after-sensation of seen movement? Perhaps. Such movement is, of course, complicated by the negative after-images of felt movement and by the nystagmic eye-movements.

The period under discussion—about 1825–1850—was, however, an era of popular scientific interest in the magic of illusions, the period of the invention of the stroboscope, the stereoscope, the kaleidoscope and numerous other trick instruments for exciting wonder by deceiving the eye, instruments which the scientists described and which then found their way into the parlors of the Victorian intellectuals (see pp. 266, 268–270, 285–287, 307).

Thus it came about, in the same year that Purkinje was discussing giddiness (1825), that Roget, the man who wrote the *The-saurus*, published a description of the appearance of a moving carriage wheel seen through the vertical apertures of a paling fence or palisade. The wheel may seem to move horizontally without rotation and with the spokes curved, convex downward on both sides. Roget published a picture of the illusion and an explanation. He noted how the successive appearance of the different parts of

a spoke, as the wheel moves along and the spoke moves up or down, would combine to give a total impression of a curved spoke, a curve for which he wrote the formula. This is not a simple stroboscopic phenomenon, but the observation was an appropriate introduction to it.

In 1831 Michael Faraday published a paper on what is more nearly a true stroboscope. He cut from white cardboard two

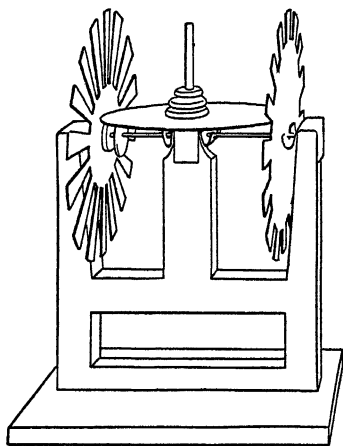


FIG. 92. FARADAY'S WHEELS
(1831)

The two white cardboard wheels rotate in opposite directions and the spokes are viewed against a dark background with the line of regard parallel to the axis of the wheels. One sees a stationary "spectral" wheel with double the number of spokes and spaces. The illusion anticipates the stroboscope.

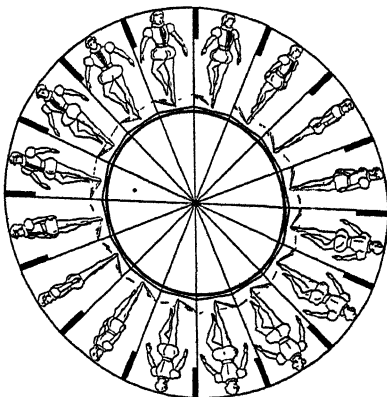


FIG. 93. PLATEAU'S STROBOSCOPE
(1833)

The disk is mounted so that it can be spun around its center. It is turned to face a mirror, and the observer behind it views an image of a dancer in the mirror through the rectangular slots (black in figure). As the slots successively expose the successive pictures, the dancer is seen to execute a turn. Plateau called this instrument a phenakistoscope.

wheels, each with rectangular teeth or spokes and with spaces between the spokes equal in size and shape to the spokes. (Fig. 92.) He mounted them on the same axis but on different axles, arranging for them to be rotated in opposite directions. Then he sped them up until the white teeth, seen casually at an angle, fused as a gray blur. Sighting the spokes along a line parallel with the axis in such a way that one wheel was directly behind the other, he found, however, that he could see a 'spectral' stationary wheel with

double the number of ill-defined teeth and with as many blurred spaces between them. His explanation was not wholly clear, but Helmholtz later gave this account. When the spokes coincide, with a front spoke obscuring a back spoke, the total field is relatively dark, half black (the background) and half white; when the spokes are staggered, the total field is light, all white for an instant. The lighter field prevails over the darker. The spokes of both wheels are seen, though blurred, near the time of maximal illumination. Then this impression is reenforced at the next moment of maximal illumination when the wheels have rotated—the one with respect to the other—through the distance between spokes. This explanation makes the phenomenon truly stroboscopic.

In November, 1830, Plateau, the Belgian scientist who contributed so much to visual science, constructed an instrument which consisted of the disk pictured in Fig. 93. One holds the disk, mounted so that it can be spun around, up to a mirror, with the face shown in the figure toward the mirror, and one stands behind it, looking, while the disk is spun, at the mirrored image through the slots cut in the disk. He sees, of course, successive instantaneous images of the dancer depicted on the face of the disk, with the dancer revolving through one sixteenth of a complete turn in each successive image. The image appears to move continuously, and Plateau, imbued with the nineteenth-century belief that the normal function of the senses is to tell the mind the truth about the external world, called the machine an 'eye-deceiver,' a *phenakistoscope*. Plateau gave his first model to Quetelet (the Belgian who invented statistics and applied the normal law of error to the distribution of human characteristics) to give to Faraday, because he believed that the instrument threw light on the explanation of Faraday's wheel-and-spoke illusion. Later—in January, 1833—he published an account of the instrument and of the phenomenon.

Meanwhile Stampfer in 1832 had invented a similar device which he described in a brochure whose preface is dated July, 1833. He called the instrument a 'visual whirler' or *stroboscope*, of which he is often considered to be the inventor. It is true that he conceived the principle without knowledge of Plateau and made up the name which was finally adopted, but Plateau has priority for the idea.

In 1834 Horner invented the cylindrical stroboscope, similar to

the one in Fig. 94. A strip of paper, bearing pictures showing the successive positions of some object, is slipped inside the cylinder around its wall at the bottom and is viewed from above down through the slits at the top. Horner thought the elimination of the mirror a great advantage and called his machine a *daedaleum*, after the mythical Daedalus to whom the Greeks attributed the invention of many mechanical wonders from the minotaur's labyrinth in Crete to the wings with which he flew away from Crete.

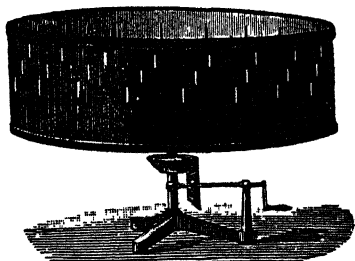


FIG. 94. HORNER'S STROBOSCOPE

In this way the stroboscopic principle that the discrete displacement of the stimulus-object can give rise to the perception of continuous movement was fully established. The word *stroboscopic* has nowadays this meaning in spite of its different etymology. The next event was the discovery of the negative after-sensation of seen movement.

The successive pictures are on a strip of paper inside the cylinder at the bottom and are viewed obliquely through the slots above them as the cylinder rotates. Cf. Fig. 93. No mirror is required. Horner called this instrument a *daedaleum* (1834) and published no picture. This figure, with three sets of slots, is reproduced from Helmholtz (1896).

In 1834 Addams published "An Account of a Peculiar Optical Phaenomenon Seen after Having Looked at a Moving 'Body'"—the water-fall illusion. If one gazes at the moving water of a fall and then looks away at rocks beside the water, the rocks appear to move up. The deck of a boat seems to move forward if one turns his regard to it after inspecting the water over the side moving aft. Addams sought to explain these illusions in terms of the persistence of compensatory eye-movements: the eyes move with the moving stimulus and, having reached a limit, shift back and move forward again. The backward shifts continue, he thought, when regard is changed to a stationary object.

In 1850 Plateau described what happens when one views a slowly rotating disk that has an Archimedes (arithmetical) spiral drawn upon it (Fig. 95). Mainly he was interested in the rings of color generated by a white spiral on a black ground, but he also noted that such a disk tends to expand or contract, according to its

direction of rotation, that the contrary movement is seen if the disk is stopped, and that this contrary expansion or contraction is transferred to any other stationary object—like the experimenter's head—to which the subject turns his gaze. In 1856 Oppel studied

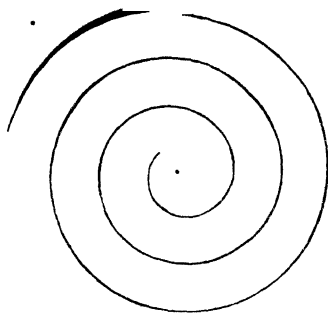


FIG. 95. PLATEAU'S SPIRAL (1850)

The disk is seen to expand as it rotates clockwise, or to contract as it rotates counterclockwise. The reverse motion is perceived when the disk is stopped or when other objects are fixated, after the moving disk has been observed. Cf. Fig. 96. The device is sometimes attributed to Mach, who used a black, broader spiral. Plateau's disk may also induce colored rings.

the phenomena of movement obtained with Plateau's disk, and then Mach took up the problem, putting his student, Dvorak, on it in 1870, and later discussing it in his classical monograph on *Bewegungsempfindungen* in 1875. H. P. Bowditch and Stanley Hall used the disk in 1882.

Meanwhile Helmholtz had discussed the water-fall problem in 1866. He pointed out that the same illusion occurs in a train: after viewing the moving landscape through the car window, the aisle of the car is seen to move in the opposite direction when the eyes are turned upon it. He espoused Addams' theory of compensatory eye-movement, and Wundt, who always turned to kinesthesia for the explanation of spatial perception, lent the

weight of his prestige to this theory. Bowditch and Hall even described a mechanical 'water-fall' (Fig. 96), to which James' name has often been attached because he reprinted a sketch of it in his *Principles* (1890). (Or did James build it first?)

Plateau's spiral, however, told against the eye-movement theory. The eye cannot execute movements of expansion or contraction. Thus Dvorak, writing with Mach's approval, argued against Helmholtz's view; and Bowditch and Hall, describing a number of illusory rotatory movements, as well as the expansion-contraction of the spiral, made the same point vigorously. But, if the perception of movement when nothing moves—in the stroboscope, in negative after-sensation—is not due to compensatory eye-movement, to what is it due? Dvorak said it must be a special retinal process.

Vierordt (1876) appealed to relativity and contrast. When the current in mid-stream is swift, he said, the water near shore may appear to move upstream. The moon can be seen scurrying behind seemingly still clouds. Often when the background moves, we think ourselves moving, as when in a railroad car we see another train start. If the forefinger, Vierordt noted, is held against the forehead and is kept still while the head is moved from side to side, then the head is perceived as moving one way and the stationary finger the other. Thus could Silvanus Thompson, citing numerous other instances of this sort in 1880, lay down a general law of "subjective complementary motion." The retina, he said, adapts itself to long continued movement; then the field surrounding the movement, or the field viewed after the movement, tends to be perceived as moving in the opposite direction by "contrast"—a principle not unlike that being invoked at the same time by Helmholtz and Hering to account for simultaneous color contrast.

There were other theories. Wundt appealed to fusion as well as eye-movement in order to explain the perceived continuity of stroboscopic movement. Marbe explained the fusion in terms of persistent after-images. The school of *Gestaltqualität* (von Ehrenfels, 1890) held that perceived movement is a form-quality, founded on the change of place in time—like a melodic movement which is founded on change of pitch in time. These arguments and opinions got nowhere, but there was another that brought forth fruit.

Dvorak (speaking, it would seem, for Mach in 1870) held that

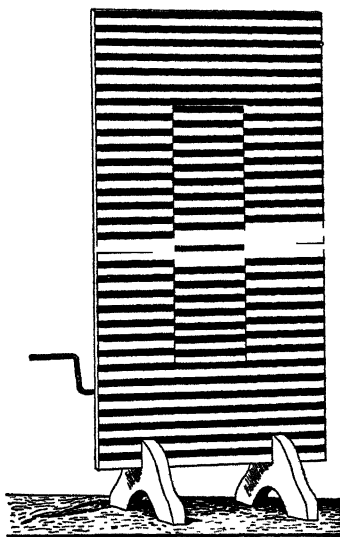


FIG. 96. WATER-FALL ILLUSION:
BOWDITCH AND HALL (1882)

The crank moves a striped band, seen through the aperture, down (or up). The subject sees a reverse movement when the band is stopped, if he looks at the background or if he looks at another object. This instrument is usually attributed to James. (Did he build it first without describing it in print?)

These arguments and opinions got nowhere, but there was another that brought forth fruit.

movement depends upon a simple retinal process and must be understood in its own right and not as a space-time complex. Vierordt presented a similar view when he argued in 1876 that movement is nativistic and primary, ending his paper with: "Nihil est in intellectu, quod non antea fuerit in sensu." Movement is a sensation in itself, Vierordt said—like time in the *Zeitsinn*.

Exner had meanwhile performed experiments which led him to a similar belief. He had presented the subject with two spatially separated, successive, electric sparks and had found that the time-order of the sparks can be correctly perceived (on the average) when the interval between them is not less than 0.045 seconds. Then he put the sparks closer together in space and got, not succession, but the stroboscopic appearance of the movement of a single spot from the earlier position to the later. The threshold for the correct perception of the direction of this moving spark was only 0.014 seconds. In other words, movement caused by the displacement of position in time can be sensed correctly as to the temporal order of the stimuli when the time-interval itself is too small to be perceived as such. Movement must, therefore, involve a special process, and Exner argued that it is thus a sensation and not complex like a perception.

Against this systematic background G. H. Schneider in 1878 asked the question: Why are objects so much more easily noticed when they move a little than when they are at rest? He found that a shadow, too faint to be perceived at rest, is noticeable when it moves. The ratio of the intensity of the just perceptible moving shadow to the intensity of the just perceptible still shadow he determined as of the order of 2:1 and 3:1. Similarly, the finger's shadow cast by the sun on the closed eyelid is invisible until the finger is moved. Schneider discussed at length the biology of movement—the way in which animals become invisible in 'death-shamming,' the manner in which the stalking animal avoids alarming his prey by remaining quiet, how birds and squirrels will approach an immobile person and flies light on a stuffed bird or frog, and, conversely, how a kitten cannot avoid attention to a moving ball and how a person will attract attention by jumping up and down. In other words, the organism does not necessarily perceive a thing and then see it move, if it moves; it can perceive a faint or small object only if it moves. Biologically movement is prior to the other conditions of sensation.

It may therefore be said that the thought of the 1870's and 1880's pretty well established the fact that seen movement is not due solely to eye-movement, but is an immediate sensory phenomenon, in no wise dependent upon an inference based upon change of place in time. Thereafter, the adequacy of stroboscopic movement was amply confirmed by Edison's invention of the kinetoscope in 1894 and its subsequent development in moving pictures. The problem of seen movement did not, however, again figure importantly in the psychological laboratories until Wertheimer's paper in 1912, the paper that begot modern Gestalt psychology.

Wertheimer simplified the observational situation. Instead of the continuously occurring discrete displacements of the stroboscope or the kinetoscope, he arranged, with a tachistoscope, for a single discrete displacement of a simple geometrical figure, a line or a curve. The first member presented he designated *a*, the second *b*. When the time-interval between *a* and *b* was relatively long (above 200 millisecon.) the subject perceived succession, first *a*, then *b*. When the interval was very short (less than 30 millisecon.), the perception was one of simultaneity, *a* and *b* together. In between successivity and simultaneity he got movement, the optimal interval for which was about 60 millisecon. For times between the movement-optimum and successivity the subject perceived various kinds of partial movement. For instance, as the time-interval is increased above the optimum, the seen movement tends to break up into a dual movement in which each part moves with a lack of continuity, or into a singular movement in which one part moves and the other is stationary. In these cases, instead of seeing a single object move, the subject sees two successive objects with one or both of them moving. Within this interval there is also the case of pure movement, which Wertheimer named ϕ , movement which connects the objects and has direction between them but seems not in itself to be an object. The series of phenomenal impressions for increasing time-intervals is therefore something like this: simultaneity—optimal movement—partial movement—pure movement (ϕ)—succession.

Wertheimer pointed out—essentially—that this finding shows that movement is movement. The succession *a*—*b* is not essential to it. It can occur as ϕ without being an object. For optimal movement one sees a single object moving, not an *a* turning into a *b*. In this contention Wertheimer was following out the tradition of

Mach and Exner, but he went further. He insisted on the validity of movement as an immediate experience without reference to basic constituents, on the 'givenness' of ϕ and its irreducibility to terms of space and time. Out of such an intransigent phenomenology arose Gestalt psychology.

There is one other item of importance in Wertheimer's paper. He suggested that seen movement may be the consequence of a "physiological short-circuit" in the brain. Given exactly the right time-interval, the excitation at one point may be drawn over to become the excitation at the other, the process being—not a retinal process, as Dvorak had thought—but a cortical process which is the physiological substrate of apparent movement. This form of psychophysical parallelism follows the axioms of Mach and G. E. Müller, and anticipates the isomorphism of Köhler which has become so nearly an indispensable for Gestalt psychology (see pp. 88–90).

Wertheimer's paper, supported presently by the enthusiasm of the growing school of Gestalt psychology, was a great success, for it was the starting point of well over a hundred papers on apparent movement during the next thirty years. At first there were but a few studies by Gestalt psychologists in Germany under the influence of Koffka and Köhler, but the Americans took up the topic in the 1920's with considerable zeal. The best that we can do with this large literature is to select seven important topics, considering each of them briefly.

1. *Kinds of movement*. Immediately after Wertheimer's paper, Kenkel (1913), working under Koffka's direction, described three different kinds of apparent movement which he named α , β , and γ . A little later Korte (1915), another student of Koffka's, added δ -movement, and Benussi distinguished bow movement in his study of apparent tactual movements in 1916. To these may be added DeSilva's split movement (1926). The meanings of these terms are as follows:

ϕ -movement (Wertheimer, 1912) is pure movement, the movement that is seen without a moving object and the basis for the claim that movement is as primary as any other sensory phenomenon.

β -movement (Kenkel, 1913) is the movement of an object from one position to another, the kind of movement that is most

frequently investigated and the kind that is usually indicated by the phrase *optimal movement*.

α -movement (Kenkel, 1913) is the change of size of an object under successive presentation. Kenkel was working with the lengthening and shortening of the central line of the Müller-Lyer illusion (Fig. 33, p. 243) when its two forms are presented successively.

γ -movement (Kenkel, 1913) is the expansion or contraction of an object as the illumination is respectively increased or decreased. While γ -movement, like α -movement, seems to represent a change of size, the experiences are quite different. In γ -movement the object swells out in all directions or closes up upon itself.

δ -movement (Korte, 1915) is reversed movement which occurs when the later stimulus is much brighter than the earlier. Then the movement is in the direction opposite to the order of presentation.

Bow movement (Benussi, 1916) is a curved movement which does not follow the shortest distance from the first stimulus to the second. In vision an obstructing stimulus between the two successive stimuli may cause the apparent movement to curve around the obstruction, often out of the plane in which the stimuli lie.

Split movement (DeSilva, 1926) sometimes occurs in ambiguous cases, as when a vertical line, followed by a horizontal line perpendicular to it at its base, is seen to divide and rotate both to the right and left to form the long horizontal line.

2. *Nature of movement.* Systematic positions die hard. Titchener in 1915 had surrendered his belief in elementary sensations, and was holding that the conscious ultimates are the sensory dimensions (attributes; see pp. 23-25). Under his influence perceptions were being studied as integrations of these dimensions (see pp. 509-512). In 1920, from Titchener's laboratory, Dimmick published an analytic-phenomenological (a contradiction of terms from the Gestalt point of view) study of seen movement. He concluded that ϕ is a specific integration of quality, duration and extent—a peculiar gray flash between the two members. Dimmick reported that the quality of ϕ is always gray; Higginson denied this generality in 1926, obtaining colored ϕ 's for colored stimuli. Later

experiments indicate the quality of seen movement for colored stimuli to be sometimes gray and sometimes colored.

Several studies at this time showed satisfactorily that optimal apparent movement, obtained from successive discrete stimuli, may be indistinguishable from real movement. One of the first of these was by Dimmick and Scahill (1925).

3. *Laws of movement.* Korte in 1915 worked out the well-known laws of optimal movement that bear his name. They show how optimal movement depends upon the distance between the stimuli (s), the time between the stimuli (t) and the intensity of the stimuli (i). They are:

- | | |
|----------------------------------------------------------|----------------------------------------------------|
| I. $s_{\text{opt}} \sim i$, if $t = \text{const.}$; | $i_{\text{opt}} \sim s$, if $t = \text{const.}$ |
| II. $i_{\text{opt}} \sim 1/t$, if $s = \text{const.}$; | $t_{\text{opt}} \sim 1/i$, if $s = \text{const.}$ |
| III. $t_{\text{opt}} \sim s$, if $i = \text{const.}$; | $s_{\text{opt}} \sim t$, if $i = \text{const.}$ |

The first formula is read: if the time-interval is constant, the optimal distance for apparent movement varies directly with the intensity. If the spatial separation is increased, the intensity must be increased, or the optimal movement breaks down. Any two relationships in either of the columns above determine the other four. A simpler way of representing this relationship is to suppose that s , t and i are rectilinear coordinates in tridimensional space. Then, in such a system, the optimal conditions for apparent movement are represented by the three coordinates of every point that lies in a particular surface which cuts this space. On one side of the surface lie the conditions for the perception of simultaneity of the members; on the other side, the conditions for successivity.

Korte's laws hold under his special conditions, but exceptions have been found, for there are many other determinants of optimal movement.

4. *Determinants of optimal movement.* Almost every study of apparent movement has something to say about its determinants, but there have been few large generalizations. Higginson (1926) and DeSilva (1926, 1928) have established the largest numbers of conditions. DeSilva made it clear that attitudes affect the perception, that the analytical attitude is unfavorable to it, that a common-sense, passive attitude is favorable. Thus he found that meaningful objects move more readily than simple lines: the arm of a sketched man will move up to his forehead in a salute more readily than one line will rotate toward another through the same angle.

Guilford and Helson (1929), by photographing eye-movements, proved (if proof were still needed) that eye-movements are not determinants of apparent movement.

Perhaps the broadest generalization was made by Brown and Voth (1937), working in the recent Gestalt tradition with field forces. As evidence that all phenomena in the space-time field are related vectorially, tending in general to attract one another, they presented numerous examples of special fields in which particular movements were predicted on the basis of such dynamic hypotheses, and then they exhibited the confirmations of their predictions. A simple instance is that of the stimulus-object which actually rotates in a circle. Brown and Voth predicted that the attraction among the excitatory traces of the object would tend to pull the circle together, decreasing its apparent size, and that the shrinkage would increase with the speed of rotation, since the faster rates would allow less time for the dissipation of the traces. True enough: the faster the stimulus rotates, the smaller is its apparent circle of rotation. This paper came, however, late in the period which we are discussing, when Gestalt psychology was already giving place to general 'field theory.'

5. *Physiology of movement.* So well does Wertheimer's cortical short circuit fit the isomorphism of Gestalt psychology that Köhler elaborated and modified the theory in 1923, shortly after he had laid down his general principles for isomorphic brain fields in his *Physische Gestalten* of 1920. The Americans, however, have always stuck more closely to the conventional neurology of the neurologists, and brain fields have not been popular with many of them. Higginson in 1926 published nine instances which he claimed contradicted Wertheimer's theory. For instance, he was able to get simultaneous movement in different directions in the same region of the field, a situation that could hardly occur if localized excitatory potentials are moving in that region so as to achieve equilibrium. The whole question must remain unsettled until we know whether the brain is constrained to act under the all-or-none law of conduction by way of connections formed among insulated fibers, as the neurological facts indicate, or whether it is free to act by the equilibration of excitatory fields, as the phenomenological facts indicate.

6. *Tactual movement.* In 1885 Stanley Hall and Henry Donaldson determined the capacity of observers to discriminate cutane-

ous movement, inventing for this purpose the kinesimeter, an instrument for dragging a light pressure-stimulus along the skin at various uniform speeds. They found that the spatial threshold for the perception of the direction of tactual movement is about one-fourth the size of the threshold for the simultaneous discrimination of two points, a result which makes the threshold for direction about equal to the threshold for the discrimination of two points successively applied (cf. p. 477). More important than this, however, was their discovery that movement can be perceived correctly as to whether it occurs or does not occur for distances too small to give a correct perception of the direction. That conclusion, of course, fitted in with Exner's and Schneider's: movement is primary and can be correctly appreciated even when its direction and extent remain unknown.

Von Frey and Metzner, in their work on the successive two-point limen in 1902, noted that the discrimination of the two points often takes the form of being a perceived movement—a "shifting" or a "stroking" (cf. p. 479). It was not until Wertheimer's paper, however, that research in apparent tactual movement was stimulated. Then Benussi (1913, 1916) undertook to duplicate Wertheimer's visual experiment for touch. He got "kinohaptic" movement—shiftings, strokings, bows—but it was not "objectless," always an "Etwas" moving. He concluded, therefore, that there is no pure ϕ in touch. Some of his movements were over great distances, as from a finger of one hand to a finger of the other with the arms outstretched, a distance of 140 centimeters. He, an act psychologist, attributed the perception of movement to an act of judgment, and one would hardly expect to get a pure ϕ for these huge distances.

Burt in 1917 was able to confirm Korte's three laws for touch. Whitchurch (1921) showed that the conditions for apparent tactual movement are less compulsory than they are for visual movement. Her optimal conditions yielded movement only about 60 per cent of the time. Andrews (1922) showed that Benussi's bow movement does not occur except under an attitude favorable to it; the same objective conditions can always yield a straight movement. Hulin (1927) gave the fullest account of conditions and confirmed Whitchurch on the non-compulsory nature of the conditions, obtaining apparent movement in 64 per cent of the cases with optimal conditions. His finding that "bewilderment" charac-

terized the attitude of the subjects in observing apparent tactual movement accords with Benussi's conclusion that the perception is mediated by judgment and is not an immediate ϕ .

7. *Auditory movement.* If tones can be localized, they ought to move when the localization changes. Such auditory movement has been recognized ever since it was discovered that a continuous change of dichotic phase gives rise to a continuous change of localization (see pp. 387-391). When phase is changed in this manner the sound—often called a “phantom”—moves around the head from one side to the other and, when the shift of phase is more than a wave-length, either “moves” or “jumps” through the head to the other side and starts to move around the head again. It is more probable that the shift “through” the head is a judgment rather than an immediately phenomenal movement, for the sound sometimes appears on one side before it has left the other.

Burt in 1917 undertook to produce apparent auditory movement by the discrete displacements of sounds. He obtained results similar to Wertheimer's and Korte's but decided against Wertheimer's short-circuit theory because he thought that difference of localization would not mean difference of spatial projection in the brain. Later auditory theory is, however, against him on this point. Kester, on the other hand, decided in 1926 that there is a pure auditory ϕ , in which there is movement when no “Etwas” moves. Then Mathiesen in 1931 indicated that apparent auditory movement is even more adventitious than tactual, for she found no conditions which she could regard as compulsory, getting apparent movement in only about four per cent of her 6000 trials. Those who, like von Hornbostel, are interested in demonstrating the “unity of the senses” (p. 27), have naturally wanted to see auditory apparent movement turn out to be like visual and tactual. Burt's objection, however, will continue to be of significance until we know more about the relation of auditory localization to brain localization.

Such is the history of perceived movement. Beginning with the notion that there is no special problem, that movement on the retina or skin is a movement of neural excitation which can, of course, be perceived as such, sense-physiology found a problem when the illusions were first described. Movement can be perceived—the scientists then realized—when nothing moves. It can

be perceived for discrete displacement—in the stroboscope—or for no displacement at all—in a negative after-sensation. Still, except for the eye-movement theory, explanation remained anchored for the most part to the logical analysis that the cause of movement is nothing more than displacement of the stimulus in space and time. Ultimately it was Wertheimer's insight of 1912 that dismissed this conventional explanation as a false tautology, to show how the phenomenological approach can set the problem and also begin to solve it. He who would understand the nature of the positive contribution of Gestalt theory to psychology needs no better tutor than this history of research and thought on the perception of apparent movement. Problems cannot be solved until they are discovered.

Notes

On the interrelation of the senses in respect of mutual facilitation and inhibition, see the excellent review by G. M. Gilbert, Inter-sensory facilitation and inhibition, *J. general Psychol.*, 24, 1941, 381–407. On other general matters of sensation, see Chapters 1 and 2. On the Weber-Fechner intensive functions and on space perception, see the chapters for the special senses.

Perception of Time

In general, see H. Nichols, The psychology of time, *Amer. J. Psychol.*, 3, 1891, 453–529; 4, 1891, 60–112; V. Benussi, *Psychologie der Zeitauffassung*, 1913; E. G. Boring, *The Physical Dimensions of Consciousness*, 1933, 127–149.

For a resumé of the views of seventy-two philosophers, see Nichols, *op. cit.*, 453–502. On the perception of time as dependent upon the succession of ideas, see Thos. Hobbes, *De corpore*, 1655, or Eng. trans., Chap. 3, esp. sect. 3; John Locke, *An Essay Concerning Human Understanding*, 1690, Chap. 14; George Berkeley, *Principles of Human Knowledge*, 1710, sect. 98; David Hume, *Treatise on Human*

Nature, 1739, Pt. 3, esp. sect. 3. On the notion that memory or some other integrative process is necessary if the succession is to be perceived as a unitary duration, see Thos. Reid, *Essays on the Intellectual Powers of Man*, 1785, III, Chap. 3; Thos. Brown, *Lectures on the Philosophy of the Human Mind*, 1820, I, 293–298; James Mill, *Analysis of the Human Mind*, 1829, Chap. 10, esp. the third paragraph; W. Wundt, *Grundzüge der physiologischen Psychologie*, 1874, 680–683, and more elaborately in later editions; *Einführung in die Psychologie*, 1911, Eng. trans., 1912, esp. Chap. 1; Wm. James, *Principles of Psychology*, 1890, I, 605–627.

On the conscious though specious present, see E. R. Clay, *The Alternative*, 1882, esp. 167f., who published this book anonymously, invented the term *specious present*, and is cited at some length by James, *op. cit.*, 609. All other writers seem to cite James for him, probably because they learn about him from James and fail to pierce the anonymity of his book. See also James, *op. cit.*, 608–610; L. W. Stern, *Psychische Präsenzzeit*, *Z. Psychol.*, 13, 1897, 325–349.

On duration as a simple sensation or an immediate experience, see E. Mach, *Untersuchungen über den Zeitsinn des Ohres*, *Sitzungsber. Akad. Wiss. Wien, math.-naturw. Cl.*, 51 (2), 1865, 133-150, esp. 150, in which the point of view is implicit; *Beiträge zur Analyse der Empfindungen*, 1886 or later eds., chap. on *Zeitempfindung* (103-112 in 1 ed.), in which the point of view is explicit; E. B. Titchener, *Sensation and system*, *Amer. J. Psychol.*, 26, 1915, 258-267; Boring, *op. cit.*, 1933, 127-149, esp. 127-129, 133-137. That Gestalt psychology accepts all phenomenological givens as valid direct experience, including extension and duration, is too well known to require documentation here. To choose one of the possible score of important references would be invidious.

On the attitude of the positivist toward duration, see E. G. Boring, *Temporal perception and operationalism*, *Amer. J. Psychol.*, 48, 1936, 519-522.

Time-Sense

In general, on the time-sense, see Nichols, *op. cit.*, esp. 503-529; W. James, *Principles of Psychology*, 1890, I, 611-619; E. B. Titchener, *Experimental Psychology*, II, i, 196-198; ii, 393-404; J. Fröbes, *Lehrbuch der experimentellen Psychologie*, I, 1923, 379-395; H. Woodrow, *The temporal indifference interval determined by the method of mean error*, *J. exp. Psychol.*, 17, 1934, 167-188, esp. 167-171.

The experiments and papers cited in the text are as follows: J. N. Czermak, *Ideen zur einer Lehre von Zeitsinn*, *Sitzungsber. Akad. Wiss. Wien, math.-naturw. Cl.*, 24, 1857, 231-236; reprinted in *Untersuch. Naturlehre des Menschen und der Thiere*, 5, 1858, 65-71; E. Mach, *Untersuchungen über den Zeitsinn des Ohres*, *Sitzungsber. Akad. Wiss.*

Wien, math.-naturw. Cl., 51 (2), 1865, 133-150; reprinted in *Untersuch. Naturlehre des Menschen und der Thiere*, 10, 1870, 181-200; A. Hering, *Versuche über das Unterscheidungsvermögen des Hörsinnes für Zeitgrößen*, 1864; K. Vierordt, *Der Zeitsinn*, 1868; J. Kollert, *Untersuchungen über den Zeitsinn*, *Phil. Stud.*, 1, 1882, 78-89; V. Estel, *Neue Versuche über den Zeitsinn*, *ibid.*, 2, 1884, 37-65; M. Mechner, *Zur Lehre vom Zeitsinn*, *ibid.*, 2, 1885, 546-602; R. Glass, *Kritisches und Experimentelles über den Zeitsinn*, *ibid.*, 4, 1887, 423-456; L. T. Stevens, *On the time-sense*, *Mind*, 11, 1886, 393-404; H. Münsterberg, *Beiträge zur experimentellen Psychologie*, esp. II, 1889, 1-68; also IV, 1892, 89-120; E. Meumann, *Beiträge zur Psychologie des Zeitsinns*, *Phil. Stud.*, 8, 1892, 431-509; 9, 1893, 264-306; *Beiträge zur Psychologie des Zeitbewusstseins*, *ibid.*, 12, 1896, 127-254; *Berichtigung*, *Z. Psychol.*, 10, 1896, 158-160; H. Woodrow, *The reproduction of temporal intervals*, *J. exp. Psychol.*, 13, 1930, 473-499; *Individual differences in the reproduction of temporal intervals*, *Amer. J. Psychol.*, 45, 1933, 271-281; *The temporal indifference interval determined by the method of mean error*, *J. exp. Psychol.*, 17, 1934, 167-188.

On the estimations of long times—minutes, hours, days, years—there has not been much experimental work. On voluntary waking after a predetermined interval of sleep and on the physiological 'clock' in general, see L. D. Boring and E. G. Boring, *Temporal judgments after sleep*, *Studies in Psychology* (Titchener Commemorative Volume) 1917, 255-279; E. N. Brush, *Observations on temporal judgment during sleep*, *Amer. J. Psychol.*, 42, 1930, 408-411; H. Hoagland, *The physiological control of judgments of duration: evidence for a chemical clock*, *J. gen. Physiol.*, 9, 1933, 267-287;

R. B. McLeod and M. R. Roff, An experiment in temporal disorientation, *Acta psychol.*, 1, 1936, 381-423 (the subjects were placed in camera, deprived of cues from the outside world for long periods of time).

Rhythm

C. A. Ruckmick's bibliography of 714 titles on rhythm is A bibliography of rhythm, *Amer. J. Psychol.*, 24, 1913, 508-519; 26, 1915, 457-459; 29, 1918, 214-218; 35, 1924, 407-413. There is no thorough review of the literature of rhythm, but see R. H. Stetson, Rhythm and rhyme, *Psychol. Monogr.*, 4, 1903, no. 17, 413-466, esp. 413-416 for references to J. F. Herbart (1812), R. H. Lotze (1868), K. Vierordt (1868) and E. W. von Brücke (1871); H. Woodrow, A quantitative study of rhythm: the effect of variation in intensity, rate and duration, *Arch. Psychol. N.Y.*, 2, 1909, no. 14, esp. 1-11 for the work on the objective conditions of rhythm; Ruckmick, The rôle of kinaesthesia in the perception of rhythm, *Amer. J. Psychol.*, 24, 1913, 305-359, esp. 306-311 for the history of opinion on the rôle of kinesthesia in rhythm.

On rhythmical grouping as indicating the range of consciousness, see W. Wundt, *Grundzüge der physiologischen Psychologie*, 1 ed., 1874, 717-719, which begins the doctrine by defining *Blickfeld* and *Blickpunkt*; 6 ed., III, 1911, 330-345, which gives the final development of the theory; G. Dietze, Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinander folgenden Schalleindrücken, *Phil. Stud.*, 2, 1885, 362-393; T. L. Bolton, Rhythm, *Amer. J. Psychol.*, 6, 1894, 145-258; K. Koffka, Experimental-Untersuchungen zur Lehre vom Rhythmus, *Z. Psychol.*, 52, 1909, 1-109, esp. 35 f. As the modern evidence for the ambiguity of

the concept of consciousness, see J. G. Miller, *Unconsciousness*, 1942.

On the objective conditions of subjective rhythms, see E. Meumann, Untersuchungen zur Psychologie und Aesthetik des Rhythmus, *Phil. Stud.*, 10, 1894, 249-322, 393-430; T. L. Bolton, *op. cit.*, 1894; R. MacDougall, The structure of simple rhythm forms, *Psychol. Monogr.*, 4, 1903, no. 17, 309-412; J. B. Miner, Motor, visual and applied rhythms, *ibid.*, 5, 1903, no. 21; H. Woodrow, *op. cit.*, 1909.

On the rôle of kinesthesia in rhythm, see Bolton, MacDougall, Stetson, Miner, Koffka, *opp. cit.*; E. B. Titchener, *A Text-Book of Psychology*, 1910, 344 f. (rhythm), 367-371 (context theory); and esp. C. A. Ruckmick, *op. cit.*, *Amer. J. Psychol.*, 24, 1913, 305-359; Visual rhythm, *Studies in Psychology* (Titchener Commemorative Volume), 1917, 231-254.

Perceived Movement

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Chapter 16

CONCERNING SCIENTIFIC PROGRESS

WE come at last to the conclusion of this book. Does the history of science, having no end, have, nevertheless, a conclusion? Does it teach something? Yes; in exhibiting the nature of scientific progress, it provides an insight into the present.

Science is thought: It exists in the minds of scientists. At any moment it consists of what scientists believe to be true, and the best established facts are, in general, the oldest ones. Not being philosophy or art, science is controlled by nature, by the empirical methods of observation and experiment. Observation and experiment are, however, merely the tools of insight—insight into nature, we say, having regard to the empirical character of observation. *Scire* is, thus, something more than *esse*. Science is not only objective, but also personal, depending, as it does, on human thought, being limited by the laws of thought. The psychologist, consequently, has a special advantage when he studies the history of his own science, for he is able to perceive how its progress has been wrought out of thinking, has been achieved, one might almost say, in spite of the limitations of the human mind.

Let us, therefore, seize this opportunity—referring for the most part to the contents of this book, and less often to the materials of its predecessor of 1929—to realize how scientific thinking gets on and what keeps it back—especially what keeps it back. Much can be said for an effort to understand the nature of scientific progress by an examination of its inhibitors. Someone once remarked that to comprehend the nature of insight, of correct thinking, one needs at least to know what prevents it. It would be so easy to be right if one were never wrong! Could Kepler have discovered that white is a mixture of colors? If not, why not? Could Haller have discovered that the nervous impulse is an electrical wave of negativity? Why not? Why could not Charles Bell have formulated the resonance theory of hearing? Or could he?

The complete answer to such questions would be another book. Its author would have to examine all the psychological determinants of scientific thinking on the positive side—the occasions for discovery, the insights, the motives—and on the negative side—the inauspicious moments, the blindnesses, the prejudices. He would then discover that he was really writing about every kind of thinking, and that thinking in general can be understood only when the total personality is understood—its motives and drives, its wishes and prejudices, its needs and interests. To no undertaking of such proportions can we here address ourselves; yet it lies within reason that we should conclude this book with a simple statement of some of the more important reasons as to why science progresses no faster than it does.

1. *Scientific progress at any point waits on the discovery of instruments and techniques.* This limitation, not being psychological, is understood by all. Newton would never have discovered color mixture without a prism. He went to the Stourbridge Fair, bought a prism, and presently knew that white is a mixture of colors. Helmholtz would never have formulated the resonance theory of hearing had not the compound microscope been improved enough for Corti to describe the structure of the inner ear. It is true that Haller made a guess about resonance before the microscope was improved, but his guess was wholly wrong. Lacking a good microscope, he lacked facts. Galvani and Volta could not have told us much about the action potentials in nerves because they had no galvanometers; du Bois-Reymond did better with poor galvanometers, Lucas still better with a capillary electrometer, Adrian best of all with the means for amplifying potentials that the radio provided. The scientific workman must have tools.

2. *Discovery is serial; it presupposes other knowledge.* This limitation on thought is also well known. Only because Newton and Helmholtz had established the laws of color mixture could König show that color blindness is best described, without the hazards of introspection, in terms of dichromatic and trichromatic vision. Bernstein would never have described the nerve impulse as a wave of electrical negativity had not Galvani first discovered what he took to be animal electricity and had not Volta then corrected Galvani. Helmholtz, in conceiving the resonance theory of hearing, depended entirely upon Galileo's discovery of reso-

nance; even Haller could guess about resonance only because Galileo had preceded him. How in the world could Wertheimer ever have described the *phi*-phenomenon had not Plateau and the others found in the stroboscope that seen movement can be created by discrete displacement of the stimulus? Thought has to build on something. Ignorance may constrain its form but will not serve for a foundation.

3. *Insight conforms to the Zeitgeist; only rarely does it depart widely from contemporary thought.* Thought, in civilization as in the individual, is sensibly continuous. While isolated steps of insight may themselves follow an all-or-none law, such quanta are seldom large. The inertia of tradition is too great for the course of progress often to be deflected widely and suddenly. It is true that convention is not so absolute as ignorance in preventing wisdom; nevertheless thought is never free from its habits. Consider, for example, the theory of sensory quality. Empedocles believed that eidola of objects are transmitted by the nerves to the Mind so that it may perceive the objects by their images. Later there arose the notion that there are animal spirits in the nerves to conduct the eidola. Then, under the influence of materialism, the animal spirits came to be regarded as a *vis viva* or a *vis nervosa*. Presently Johannes Müller, seeing that every sensory nerve always produces its own quality, substituted for the *vis nervosa* five specific nerve energies. Soon it was seen, however, that Müller's alternative hypothesis, the notion that the specific energies may lie in the central terminations of the nerves, meant nothing more than that the nerves are specific as to their termini. Thus arose the concept of sensory centers. Now we are beginning to realize that the important thing about a center is that it can make certain specific connections and not others. Was there anything to prevent Galen or Haller from conceiving of sensory neural specificities? Only the habits of thought of their times. Why did Müller prefer to think of the energies as more likely to be in the nerves than in the brain, and why did he call the specificities of the nerves *energies*? Because energy is something like a *vis nervosa* which in turn resembles animal spirits. They are all substantial forces in nerves. It is in the nature of thought to proceed slowly.

There are, of course, notable exceptions, but they are few. The most original discovery described in this book is Newton's finding that white light is a mixture of colors. That idea was simply too

silly to seem true, and Newton at first was made to suffer for his temerity—though the experimental demonstration with the prisms was so simple and so easy. Not easily do men prefer logic to approbation.

4. *Individual thinking also shows its inertia; men do not readily perceive the obvious when it contradicts their habits of thought.* These biases—for they are biases—may be conscious or unconscious. On the unconscious side, we have the persistent failure to appreciate the function of binocular parallax in the perception of depth. Kepler in 1604 fixed upon the scientists the notion that vision is the perception of the retinal image—a theory that was comfortably consistent with the ancient image theory of perception. The definition of the horopter in 1613 established the concept of identical retinal points as the reason for singleness of binocular vision. After those events retinal disparity was described more than once and explained away by various ingenious logics. No one was quite able to transcend the established fact of identical points enough to see that retinal disparity *is* the reason for binocular perception of depth—not for two centuries. Then Wheatstone, aided perhaps by current interest in optical magic (the stroboscope, the kaleidoscope), invented the stereoscope. Even he did not at once see its full significance. Or consider, in this connection, the paradox of Gestalt psychology in pressing its claim that perception does not mirror the stimulus. The fact is that, for seventy years before Wertheimer introduced Gestalt psychology to the world, psychologists had been investigating the differences between perception and the stimulus—in the illusions, the constants of psychophysics, Weber's law, indeed, in everything experimental about perception. Of course, this aspect of the Gestalt concept was not new. Yet Gestalt psychology had to fight to get it accepted, all because the simpler views—the views that derive from the old image theory of perception, which Johannes Müller too had fought—persisted along with facts that contradicted them. Contradiction does not seriously menace strong habits of thought when their character is not consciously recognized.

On the conscious side, there are all the clearly held systematic views. Titchener thought analytically. Under pressure from Gestalt psychology he shifted from sensations to dimensions for his ultimates, but he was still making an analysis. He even sought to determine the color of the *phi*-phenomenon. Today the positivists

claim to have the formulae for reducing consciousness to discriminatory response, and sometimes they do it. Yet the dualism of mind and body, of phenomenal experience and brain, is so well ingrained in conventional thought that not many follow them, though few refute them. These inertias are conscious and voluntary. Most of the protagonists understand both sides, yet each holds to his own.

5. *Personal attitudes constrain or divert thought.* First there are egoism and the need for prestige. They tend to close the mind in controversy, a phenomenon which is illustrated in any thoroughgoing scientific quarrel. Hering quarreled with Helmholtz about the horopter; and, if Helmholtz came off the better, it was only because he was able to see both sides. Wundt quarreled with Stumpf about the bisection of tonal distances, Wundt roaring through his majesty and Stumpf wrapping his robe of musical sophistication firmly about him. Even the nativists quarreled with the empiricists, although, as we have seen, every nativist was something of an empiricist, every empiricist something of a nativist.

Not distinct from egoism is the need for self-consistency. Every quarrel shows that too. When a man takes up a position, his pride prevents retraction. Galvani had said that the frogs' legs move by animal electricity and was in no frame of mind to accept Volta's belief that the leg was simply the cell of a battery. Psychology, moreover, grew up in Germany where every *Gelehrter* was fully entitled to defend his past commitments. Sometimes it is not respectable to be right if you have to change your mind out loud.

6. *Social attitudes also constrain thought.* Here we have the influence of the schools, the need for men to stand together. The in-group magnify their agreements and rise to repel, or at least to depreciate, the out-group. Gall must have prevented the exact localization of brain function for years because everyone among the 'proper' scientists was against *his* exact localizations. Similarly the psychologists of 1880-1910 wasted no end of time in proving they were scientific, all because they were fighting philosophy. Within the school agreement is facilitated. Wundt's students confirmed the tridimensional theory of feeling; others did not. Würzburg never found images for thought; Cornell did. Is feeling a sensation or not? Laboratory atmosphere largely determined what would be found in answer to that question, and laboratory atmosphere often extended from a parent laboratory to its offspring.

Psychical acts belong to southern Germany; contents to northern; whereas mind that is of use to the organism is American. These matters need to be worked out more thoroughly, but there can be no doubt that within the *Zeitgeist* there are local *Geister* which determine what theory you shall apply to your experimental findings or even how you shall record your data.

It would be easy now to end with a moral, to suggest that science would get along faster if the scientists knew their history and could, by that insight, overcome their inertias, egoisms, prejudices and needs for self-consistency. Such a conclusion would, however, be much too simple. Even if these constraints to progress could be removed by the mere act of insight into their nature, the result might not be a quickening of the scientific pace. Science has perpetually to contend against its own ignorance by honest hard work. The psychological forces that block or misdirect its progress also drive it; and all these negatives have their positives. The obverse of the *Zeitgeist* as inhibitor is knowledge of contemporary research. The investigator's power is increased if he works with the times. The obverse of individual inertia is personal integrity. We have a right to expect that a colleague will not think one thing on Monday and another on Wednesday—at least not every Monday and every Wednesday. The obverse of egoism is pride—pride of technique, pride of observation, pride of logic, pride of insight, pride of achievement. What other guarantee have we of integrity? The obverse of social constraint is loyalty—loyalty to colleagues, capacity for cooperation, generosity of private information. Before we take science away from men and put it in the hands of impersonal robots, we had better know what sort of personalities we want to give the robots.

Yet a knowledge of the history of science is important. It gives perspective. It aids humility. It exhibits the kinds of errors that can be avoided. It even saves the trouble of discovering the same fact twice. If scientists need to compute errors of observation and to determine the constants of their instruments, then they should remember further that the primary instrument of all science, the human mind, also has its errors and its constants of which they need to be aware—and to beware. The history of science can teach them that.

INDEX OF NAMES

The important items are given first. Incidental mentions of a name are labelled "inc." and given second. Citations of the literature are labelled "ref." and given third. When there are more than four important subjects, they are keyed by subject. No "inc." item is included when there is an important subject-item or a "ref." item for the same page.

- Abbott, E., 498, 500; ref., 519
 Abney, W. deW., ref., 196
 Abrahams, H., ref., 461
 Addams, R., 591; ref., 605
 Ades, H. W., ref., 95
 Adrian, E. D., refractory period, 59 f.;
 all or none, 60 f.; action poten-
 tials, 65 f.; frequency theory, 68;
 cochlear response, 422
 inc., 214, 389, 419, 421, 609
 ref., 91-3, 95, 435
 Agatharcos, inc., 265
 Aguilonius, F., 79, 226, 228, 271, 284;
 inc., 157; ref., 257, 305, 308
 Aitken, J., inc., 164; ref., 191
 Albinus, *see* J. P. Lobé
 Alcmaeon, inc., 400
 Alexander, G., inc., 543; ref., 570
 Alhazen, 98, 222; inc., 155, 165, 192,
 267; ref., 121
 Alrutz, S., 504-9, 511; inc., 493; ref.,
 520
 Alspach, E. M., ref., 151
 Alston, J. H., 508; ref., 521
 Anaxagoras, inc., 265
 Andrews, W. A., 600; ref., 606
 Angelico, Fra, 265
 Angell, F., 343; ref., 350
 Ansbacher, H., ref., 262
 Appunn, A., 328, 334 f.
 Arago, D. F. J., inc., 137; ref., 152
 Archimedes, inc., 86
 Aristotle, 321, 465, 525; inc., 5, 8, 122,
 155 f., 165, 192, 308, 313, 449,
 463
 Aristoxenos, 315, 318
 Arnheim, F., ref., 398
 Aronsohn, E., 441, 443; ref., 459
 Arrer, M., 281
 Arrhenius, S. A., 455
 Asher, L., ref., 51, 460
 Aubert, H., 139, 160-3, 172, 174, 207;
 inc., 178, 193, 204 f., 240, 484;
 ref., 153, 191, 193-5, 218, 260,
 309, 517, 568
 Auerbach, F., 244, 384; ref., 261,
 396 f.
 Bach, J. S., 318
 Bacon, R., 98
 Bain, A., 8, 86, 382 f., 545; inc., 528;
 ref., 95, 566, 570
 Baird, J. W., 281 f.; inc., 174 f.; ref.,
 193, 305, 307
 Baley, S., 364; ref., 394
 Bappert, J., ref., 307
 Bardier, E., 553; ref., 571
 Barnholdt, S. E., 499; ref., 519
 Barrow, J., inc., 101
 Barth, W., inc., 484, 486; ref., 517
 Bartley, S. H., ref., 605 f.
 Bastian, C., 525, 535; inc., 528; ref.,
 565 f.
 Bates, W. H., ref., 306
 Beaumont, W., 551, 555, 557 f.; ref.,
 572
 Beaunis, H., ref., 567
 Becher, E., 548; ref., 571
 Beguelin, N. de, inc., 156
 Békésy, G. v., 365, 429-31; ref., 394,
 436
 Bell, C., Bell-Magendie law, 63; spe-
 cific energies, 70; cochlea, 402 f.,
 405; taste organs, 451; muscle
 sense, 525-7
 inc., 3, 8, 93, 439, 450, 531, 608
 ref., 46, 92, 94, 432, 461, 565

- Bentley, M., 130, 499, 509-11, inc., 307; ref., 151, 519, 521
- Benussi, V., 596 f., 600 f.; ref., 602, 606
- Berkeley, G., empiricism, 5 f.; perception, 15; apparent size, 223 f.; retinal inversion, 224 f.; perception as inference, 263; aerial perspective, 267; convergence, 272; accommodation, 273; kinesthesia, 524
- inc., 19, 28, 122, 298, 382, 526, 575
ref., 45, 257, 305, 602
- Bernard, C., 559; ref., 572
- Bernstein, J., 58 f., 62, 80-2; inc., 60; ref., 92, 95
- Berschansky, I., 510; ref., 521
- Beryl, F., 297; ref., 310
- Bezold, W. v., 132, 333, 410; ref., 151, 433
- Bichat, M. F. X., 526; ref., 565
- Bidder, F., 560; inc., 439, 450; ref., 458, 460, 573
- Biddulph, R., 340-2, 345; ref., 349
- Biedermann, W., 495 f.; ref., 518
- Bigelow, J., ref., 191
- Biot, J. B., 333 f.; ref., 348
- Birch, T., ref., 123
- Bishop, G. H., ref., 95
- Blake, C. J., inc., 335
- Blanchard, J., inc., 139; ref., 153
- Blix, M., 467 f.; inc., 463; ref., 513 f.
- Bloch, E., 385, 387; ref., 398
- Blumenfeld, W., 296; ref., 308, 310
- Boerhaave, H., ref., 91
- Böttcher, A., 537, 541; ref., 570
- Bois-Reymond, E. du, 57 f.; inc., 56, 78, 609; ref., 91 f.
- Boldireff, W. N., 555; ref., 572
- Boll, F., 205; ref., 218
- Bolton, T. L., 583-6; ref., 604
- Boltzmann, L., 336; ref., 348
- Bonnet, C., 7; ref., 45
- Borelli, G. A., 54, 322; ref., 91
- Boring, E. G., attributes, 26; massed pitch, 364; tonal brightness, 378; auditory localization, 390; Head's theory, 474 f.; visceral sensation, 549 f., 562-4; thirst, 559, 563
- inc., 66
ref., 45-52, 91-6, 123 f., 152, 261 f., 310 f., 349-51, 394, 396, 398, 433-5, 515-7, 565, 571, 573, 602 f.
- Boring, L. D., ref., 603
- Bouguer, P., 136 f.; inc., 34, 38, 290; ref., 152, 309
- Bourdon, B., ref., 604 f.
- Bowditch, H. P., 60, 592 f.; inc., 419; ref., 92, 605
- Boyle, R., 322; inc., 155 f., 158; ref., 121 f., 347
- Braafladt, L. H., 562 f.; ref., 573
- Bravo, J., 453; ref., 461
- Bray, C. W., 419-23; inc., 65 f.; ref., 93, 435
- Brentano, F., 130 f., 206, 245, 378, 380; inc., 16, 29, 47; ref., 151, 261, 396
- Brett, G. S., ref., 49
- Breuer, J., 387, 537, 540-2; inc., 463, 535; ref., 398, 569
- Brewster, D., 150, 286 f.; inc., 131, 199, 268; ref., 122, 150, 190, 307 f.
- Bridgman, P. W., 12 f.; ref., 46
- Broca, C., 75; ref., 94
- Brodhun, E., 139; inc., 145; ref., 153 f.
- Bronk, D. W., 422; ref., 435
- Brown, A. Crum, 537, 540-2; inc., 463, 535; ref., 569
- Brown, J. F., 599; ref., 606
- Brown, T., 7, 14, 526, 575; inc., 28, 30; ref., 46 f., 565, 602
- Brown, Warner, 40; ref., 51
- Brown-Séquard, C. E., 537; ref., 569
- Brücke, E., 132, 287; ref., 151, 308
- Brückner, A., 499; ref., 519
- Brues, A. M., 362; ref., 394
- Brunn, A. v., 440; ref., 458
- Brunot, C., 244; ref., 261
- Brunswick, E., 256, 298 f., 301, 303; ref., 262, 311
- Brush, E. N., ref., 603
- Bryan, W. L., inc., 23
- Bühler, K., inc., 135; ref., 152
- Buffon, G. L. L., 155 f., 158 f.; inc., 165, 192; ref., 190
- Buitendijk, F. J. J., 65, 420; ref., 93, 435
- Bull, O., 174 f.; ref., 194
- Burch, G. J., 59, 164; ref., 92, 191
- Burmester, E., ref., 260
- Burnett, N. C., 508; ref., 521

- Burney, C., ref., 346
 Burow, C. A., 274; inc., 119; ref., 124, 306
 Burtt, H. E., 600 f.; ref., 606 f.
 Busch, W., 551 f.; ref., 571

 Caesalpinus of Arezzo, 526
 Cagniard de la Tour, C., 328; ref., 347
 Cajori, F., ref., 122, 346
 Calkins, M. W., 22 f.; ref., 48
 Camerer, W., 479-81, 485; ref., 516
 Campbell, C. J., inc., 65; ref., 93
 Cannon, W. B., 552-8, 560 f., 563; inc., 463; ref., 571-3
 Carlson, A. J., 552-7, 562 f.; ref., 571-3
 Carmichael, L., ref., 92, 565
 Carr, H. A., ref., 258, 303
 Cartwright, D., ref., 51
 Casserio, G., 400; ref., 431
 Cattell, J. M., 40; ref., 51
 Cattell, McK., ref., 93
 Cermak, P., ref., 606
 Chaffee, E. L., ref., 348, 394
 Chamberlain, E. M., ref., 194
 Chapman, D. W., 24; ref., 49
 Charcot, J. M., inc., 387
 Charles II, inc., 100
 Charpentier, A., 204 f.; ref., 217
 Chevreul, M. E., 167; ref., 192
 Chladni, E. F. F., 333 f.; inc., 407, 412; ref., 348
 Churcher, B. G., inc., 44; ref., 52
 Clarke, H. M., 532; ref., 568
 Clay, E. R., 576; ref., 602
 Cloquet, H., 438 f.; ref., 458
 Cobbey, L. W., inc., 511; ref., 521
 Coblentz, W. W., ref., 194, 196
 Cohen, N. E., 27; ref., 49
 Cohn, G., 456 f.; ref., 461
 Cole, R. V., ref., 303
 Collins, M., ref., 196, 216
 Collins, R., 385; ref., 397 f.
 Condillac, E. B. de, 7; ref., 45
 Cook, F., 548 f.; ref., 571
 • Cooper, A., inc., 401; ref., 431
 Cornelius, H., 17; ref., 47
 Corti, A., inc., 404, 406, 609; ref., 432
 Cramer, A., 277; ref., 306 f.
 Crew, H., ref., 122 f.
 Crowe, S. J., 425 f.; ref., 436

 Crozier, W. J., inc., 440, 450; ref., 458, 460
 Culler, E. A., 82, 426; ref., 95, 436
 Cutolo, F., 507 f.; ref., 521
 Cuvier, G. L. C. F. D., inc., 74
 Cyon, E., 537, 541 f.; inc., 544; ref., 569
 Czermak, J. N., 477, 479 f., 577; ref., 515, 517, 603

 Daedalus, inc., 591
 Dallenbach, K. M., 507 f.; inc., 471; ref., 461, 513 f., 521
 Dalton, J., 108, 184 f.; ref., 196
 Darwin, C., inc., 159, 535
 Darwin, E., 535, 554; inc., 159; ref., 568, 571
 Darwin, R. W., 159, 165 f.; inc., 156, 160; ref., 191
 Davies, H., inc., 44; ref., 52
 Davies, H. M., 474; ref., 515
 Davis, H., action potentials, 65; combination tones, 356; cochlear response, 422; auditory action potentials, 423 f.; stimulation lesions, 425-9; theory of hearing, 430 f.
 ref., 52, 93, 95, 347-50, 393-5, 397, 432, 434-6
 DeForest, L., 332
 Delabarre, E. B., ref., 566 f.
 Delage, Y., ref., 568
 Delboeuf, J. R. L., 42, 244; inc., 37, 45 f.; ref., 50 f., 261
 Delezenne, C. É. J., 339 f.; inc., 38; ref., 349
 Democritos, 4 f.
 Derbyshire, A. J., 66, 423 f.; ref., 93, 435 f.
 Descartes, R., sensationism, 7; animal spirits, 54; interactionism, 85; light, 98 f.; color, 100; retinal image, 100, 223; convergence, 271 f.; accommodation, 273, 277; harmonics, 324; kinesthesia, 524
 inc., 28, 63, 95, 101, 221, 272, 526
 ref., 45, 91, 121, 257, 305 f.
 DeSilva, H. R., 596-8; ref., 606
 Despretz, C., 333 f.
 Dessoir, M., 502; ref., 520
 Dewey, J., inc., 534
 DeWitt, L. M., 531; ref., 567
 Didymos, 316-8,

- Dieterici, C., 130, 187, 201 f.; inc., 142; ref., 151, 154, 197, 217
 Dietze, G., 584; ref., 604
 Dimmick, F. L., 134, 503, 597 f.; ref., 152, 460, 520, 606
 Dixon, E. T., ref., 307
 Dobrowolsky, W., 129 f.; inc., 178; ref., 151, 195
 Dodge, R., 496 f., 543 f.; inc., 535; ref., 519, 570
 Donaldson, H. H., 467, 471, 599; ref., 514, 606
 Donders, F. C., primary yellow, 129; color blindness, 203; color evolution, 211; central color processes, 212-4; Donders' law, 230-2; accommodation, 275; vowels, 370-2
 inc., 173, 210, 216, 220, 442
 ref., 96, 150, 217, 219, 258, 306, 395
 Dove, H. W., 288; inc., 178; ref., 195, 304, 308
 Drobisch, M. W., inc., 397
 Duchenne de Boulogne, G. A. B., 532; ref., 567
 Duncan, J., ref., 347
 Dupuytren, G., 558; ref., 572
 Dusser de Barenne, J. G., ref., 568
 Duval, 453; ref., 461
 Duverney, G. J., inc., 400; ref., 431
 Dvorak, V., 592 f.; inc., 596; ref., 605 f.
 Ebbinghaus, H., 148 f., 417 f.; inc., 130, 150; ref., 151, 153 f., 219, 434
 Ebert, H., ref., 195
 Edelmann, M. T., inc., 335
 Edison, T. A., 331; inc., 330, 369, 395, 595
 Edmonds, E. M., 362; ref., 394
 Edridge-Green, F. W., inc., 210, 216; ref., 219
 Ehrenfels, C. v., 16 f., 246, 254; inc., 593; ref., 47, 605
 Einthoven, W., 244; ref., 261
 Eisler, R., ref., 95
 Eissler, K., ref., 262
 Ek Dahl, A. C., 364; ref., 394
 Elliott, J., inc., 308
 Elliott, M. H., ref., 572
 Ellis, A. J., 320; ref., 346
 Ellis, W. D., ref., 261 f.
 Emerson, W. B., ref., 194, 196
 Emmert, E., 292; ref., 310
 Empedocles, 4, 84, 400; inc., 610
 Epicuros, 5
 Erasistratos, 53 f.; inc., 63
 Erb, W. H., 467
 Erlanger, J., 77; ref., 95
 Estel, V., 579 f.; ref., 603
 Euclid, optics, 97; apparent size, 2 convergence, 271; binocularallax, 282; visual angle, 290; musical scale, 316
 inc., 268
 ref., 307, 309
 Euler, L., inc., 267
 Eustachio, B., 400; ref., 431
 Ewald, E., ref., 218
 Ewald, J. R., 413; inc., 407; ref., 434
 Ewert, P. H., 237; ref., 259
 Exner, S., 163 f., 363, 594, 600; inc., 596; ref., 191 f., 394, 605 f.
 Faraday, M., 589 f.; inc., 56; ref., 605
 Fearing, F., ref., 91
 Fechner, G. T., psychophysics, 34-45; parallelism, 87 f.; experimentalism, 117; cloud experiment, 137; after-image, 159 f.; size and distance, 291; two-point limen, 480; thermal j.n.d., 497 f.; kinesthesia, 529
 inc., 10, 55, 68, 89, 95, 116, 129, 132, 136, 169, 220, 342, 350 f., 501, 577
 ref., 50 f., 153, 191, 309, 350, 519
 Fernberger, S. W., 40; ref., 51
 Ferrall, S. C., 507; ref., 521
 Ferree, C. E., 175 f., 385; ref., 193 f., 397 f.
 Ferrier, D., 75; inc., 528; ref., 95, 566
 Fick, A., ref., 513
 Finch, G., ref., 436
 Findley, A. E., ref., 460
 Firestone, F. A., inc., 44; ref., 52
 Fischer, E., 442; ref., 459
 Fisher, S. C., ref., 568
 Fleming, J. A., 331
 Fletcher, H., auditory sensitivity, 337 f.; aural harmonics, 359; thermal noise, 365; masking, 367; vowels, 375; pitch in cochlea, 428

- inc., 44, 430
 ref., 52, 348 f., 393-6, 436
 Flourens, M. J. P., 74-6, 403, 405,
 , 536 f.; inc., 80, 386, 535, 540;
 , ref., 94, 432, 568 f.
 Foerster, R., ref., 309
 Folio, C., 400; ref., 431
 Forbes, A., 65-7, 420; inc., 214, 419,
 421, 423; ref., 91, 93, 435
 Foster, M., ref., 91
 Foucault, M., 481; inc., 480; ref.,
 515 f.
 Fourcroy, A. F. de, 444; ref., 459
 Fourier, J. B. J., 325 f.; inc., 327 f.;
 ref., 347
 Francesca, P. della, 265
 Frank, H., ref., 309 f.
 Franke, G., 440; ref., 458
 Franklin, B., 55, 158; ref., 190
 Franz, S. I., 76; inc., 94
 Fraunhofer, J. v., 108, 128, 177 f.;
 inc., 34, 127; ref., 150, 194
 Freeman, E., ref., 310
 Freeman, G. L., ref., 521
 Fresne, R. du, ref., 308
 Frey, M. v., tactual senses, 468-71,
 475; Head's theory, 474 f.; two-
 point limen, 479, 483 f.; pressure
 stimulus, 487-90; thermal stimu-
 lation, 492 f., 504 f.; pain stimu-
 lation, 493 f.; heat, 505, 511;
 tactual movement, 600
 inc., 142, 463, 496, 499, 501 f.,
 504, 515, 524
 ref., 154, 514, 516-20, 606
 Friedline, C. L., 482; ref., 516
 Fritsch, G., 75; ref., 94
 Frobes, J., ref., 348, 603
 Fry, H. S., 388; ref., 398
 Fubini, S., 559; ref., 572 f.
 Fullerton, G. S., 40; ref., 51
 Fulton, J. F., 531; ref., 91 f., 95,
 566-8
 Funke, O., 466 f., 502; ref., 46, 513
 Gad, J., inc., 502; ref., 514, 520
 Gaglio, G., inc., 543; ref., 570
 Galen, C., animal spirits, 53 f.;
 chiasma, 79; optic decussation,
 227; binocular parallax, 283;
 labyrinth, 400
 inc., 63, 272, 610
 ref., 257, 307, 431
 Galileo Galilei, 98, 323 f., 378; inc.,
 26, 97 f., 319, 322, 328, 332, 347,
 409, 609 f.; ref., 346
 Gall, F. J., 74 f.; inc., 257, 612; ref.,
 94
 Galton, F., 335 f.; inc., 460; ref.,
 348
 Galvani, L., 55-7; inc., 64, 609, 612;
 ref., 91
 Gamble, E. A. McC., 443; ref., 459
 Cassendi, P., 322; ref., 347 f.
 Gasser, H. S., 77; ref., 95
 Gates, E. J., 134, 481; inc., 21; ref.,
 152, 516
 Gatti, A., 496 f.; ref., 519
 Geiger, P. H., inc., 44; ref., 52
 Gellé, 387; ref., 398
 George I, inc., 319
 Gertz, E., 500; ref., 519
 Gibson, J. J., 238; ref., 260
 Gilbert, G. M., ref., 602
 Gilbert, W., 55
 Glass, R., 580; ref., 603
 Glisson, F., 54 f.; ref., 91
 Godart, de, inc., 156, 158
 Göbel, O., ref., 434
 Goethe, J. W., 112-6, 128; inc., 28,
 32, 117-9, 122, 125, 131, 159,
 166, 184, 199, 206, 208, 361;
 ref., 123 f.
 Götz, W., 296; ref., 310
 Goldscheider, A., skin spots, 467-71;
 two-point limen, 479; pain
 as summation, 490; after-
 sensations, 502 f.; paradoxical
 warmth, 506 f.; kinesthesia, 530-
 2
 inc., 463, 494, 523
 ref., 514 f., 520 f., 567
 Golgi, C., 531; ref., 567
 Goltz, F. L., 75 f., 537-41; ref., 95,
 569
 Goodale, H., ref., 518 f.
 Gotch, F., 59; inc., 64; ref., 92
 Grailich, J., inc., 178; ref., 195
 Grassmann, H., 87 f., 141-3, 200; inc.,
 89; ref., 96, 154
 Gray, A. A., 406-9; inc., 411, 431;
 ref., 432-4
 Gregersen, M. I., ref., 573
 Gregg, A., 67; inc., 66, 214, 419, 423;
 ref., 93, 435
 Griesbach, H., 481; ref., 516

- Griffith, C. R., 543 f., 564; inc., 535;
ref., 568, 570, 573
- Grimaldi, F. M., 98, 101; ref., 121
- Guericke, O. v., 55, 321; ref., 347
- Guild, S. R., 425 f.; ref., 436
- Guilford, J. P., 599; inc., 37; ref., 51,
606
- Guillaume, P., ref., 262
- Guthrie, F., 330
- Hacker, F., inc., 506; ref., 521
- Haggqvist, G., inc., 471; ref., 514
- Hallström, G. G., 353 f.; inc., 357;
ref., 392 f.
- Hanig, D. P., 452; inc., 454; ref., 461
- Hall, G. S., 583, 586, 592 f., 599; inc.,
514; ref., 50, 605 f.
- Haller, A. v., velocity of nerve, 58;
animal spirits, 62 f.; natural stim-
ulus, 71; accommodation, 275;
anatomy of ear, 400-2; smell
classes, 444 f.; taste, 449 f.; taste
qualities, 453; internal sensibili-
ty, 545; hunger, 551, 553-5;
thirst, 558
inc., 8, 91, 405, 439, 608-10
ref., 46, 94, 306, 431, 458-61, 570-2
- Halley, E., inc., 165, 192
- Halverson, H. M., 25, 379; ref., 396
- Ham, L. B., inc., 43; ref., 52
- Hamilton, W., 526, 528, 545; ref., 46,
565, 570
- Hardy, J. D., 494, 499; ref., 518 f.
- Harless, C., inc., 537; ref., 569
- Harris, J., 285; ref., 308
- Hartley, D., 6, 85, 227; inc., 8 f., 95;
ref., 45, 257
- Hartmann, G. W., ref., 49, 261
- Hartridge, H., inc., 411; ref., 434
- Hartshorne, C., ref., 49
- Haupt, I. A., ref., 196 f.
- Hawkins, J., ref., 346
- Haycraft, J. B., 441, 447 f., 455; inc.,
439, 450; ref., 458, 460 f.
- Hayes, M. H. S., 503; ref., 520
- Hayes, S. P., inc., 189; ref., 197
- Head, H., 471-4, 484, 524, 548; inc.,
463, 476, 480, 494, 496; ref.,
515, 565, 571
- Hecht, S., 181 f., 215 f.; ref., 196, 217
- Heermann, inc., 119; ref., 124
- Hegel, G. W. F., inc., 248
- Hegelmaier, F., inc., 40; ref., 51
- Hegg, E., inc., 174 f.; ref., 194
- Heiderich, F., ref., 461
- Helmholtz, H., modality, 10; empir-
icism, 31, 233 f., 237; velocity of
nerve, 58; specific energies, 73;
experimentalism, 117, 120; vi-
sion, 119; primary colors, 128;
Bezold-Brücke phenomenon,
131 f.; Weber's law, 137 f.;
color mixture, 140-5; color tri-
angle, 147; after-image, 159;
contrast, 167; color zones, 173 f.;
color blindness, 183, 185 f.; color
theory, 200-3; apparent size,
224; horopter, 229 f.; illusion,
240 f., 243; constancy, 255; per-
ception as inference, 264; paral-
lax of movement, 267 f.; ac-
commodation, 273, 275-9; stere-
oscopy, 287; perception as judg-
ment, 289; harmonics, 325;
tuning-forks, 330 f.; limits of
hearing, 334; number of pitches,
341 f.; combination tones, 354-
6; beats, 355 f.; timbre, 358;
aural harmonics, 359; conso-
nance, 359 f.; noise, 363 f.; vow-
els, 367-73; perception of phase,
382, 388 f.; theory of hearing,
404-11; sensations of innerva-
tion, 525; after-sensation of
movement, 592
inc., 9, 28, 64, 67, 78, 87 f., 110,
112 f., 120, 132 f., 139, 157, 160,
163, 165, 170 f., 185, 187-9, 191,
193, 199, 207, 210 f., 230, 264,
272, 297, 302, 312, 320, 327,
329, 345, 353, 357, 381, 391,
399-401, 417 f., 437, 449, 454,
467, 528, 590, 609, 612
ref., 46, 49 f., 92, 94, 123 f., 150 f.,
153 f., 190, 192, 216 f., 257-62,
304-8, 346-9, 393-5, 432, 434,
566, 604 f.
- Helson, H., 253, 599; ref., 262, 606
- Henning, H., smell stimulation, 444;
smell classes, 445 f.; smell chem-
istry, 448 f.; taste qualities, 455;
taste chemistry, 457
inc., 439
ref., 458-61
- Henri, V., 479-81, 484, 486; ref., 258,
515-7, 564 f.

- Hensen, V., 406; inc., 404 f.; ref., 394, 432, 434
 Heraclitos, 4
 Herbart, J. F., 35; inc., 16, 258, 604; ref., 50
 Herbst, G., ref., 566
 Hering, E., nativism, 29, 233, 236 f.; phenomenology, 33, 117, 120; adequate stimulus, 71; isomorphism, 88 f.; primary colors, 129; light sense, 132 f.; after-image, 159; adaptation, 161 f., 164; contrast, 167 f.; color zones, 173-5; Purkinje phenomenon, 181 f.; color blindness, 183; color theory, 208 f.; color evolution, 210 f.; horopter, 229 f.; local signs, 235 f.; illusion, 240-2; constancy, 255; apparent size, 289; size and distance, 291 f.; tactual senses, 466; thermal stimulation, 491-3; thermal adaptation, 500
 inc., 4, 28, 74, 116, 128, 135, 160, 167, 172, 187-9, 207, 214, 216, 221 f., 230, 280, 293 f., 449, 495, 612
 ref., 49, 94, 96, 125, 150-2, 191 f., 195, 218, 258-60, 262, 307, 309, 513, 518 f.
 Herlitzka, A., 454; ref., 461
 Hermanides, J., 443 f.; ref., 459
 Hermann, L., 368, 370, 372 f.; inc., 450; ref., 46, 94, 309, 395, 458, 460 f., 513, 518
 Herschel, J. F. W., 185; inc., 153, 188, 197; ref., 197
 Hertz, A. F., 548 f.; ref., 571
 Herzen, A., 467; ref., 513
 Hess, C., 129, 168 f.; inc., 164, 175; ref., 150, 192-4
 Heuschke, inc., 217
 Heymans, G., 243 f.; ref., 261
 H. F. T., inc., 166; ref., 192
 Higginson, G. D., 597-9; ref., 604, 606
 Hillebrand, F., 280-2, 294 f.; ref., 307, 310
 Hilton, J., 547; ref., 571
 Hippel, A. v., 186; ref., 197
 Hire, P. de la, 274; ref., 305
 Hirschberg, J., ref., 309
 Hitzig, E., 75 f., 538; ref., 94, 569
 Hoagland, H., ref., 93, 153, 603
 Hobbes, T., 5; inc., 28, 575; ref., 45, 602
 Honigschmied, J., 458
 Hóring, A., 578; ref., 603
 Holaday, B. E., 298 f.; ref., 262, 310 f.
 Holland, R. T., 504; ref., 520
 Holmes, O. W., 287; ref., 307
 Holmgren, F., 186; ref., 197
 Holt, E. B., 131; inc., 12; ref., 151
 Holway, A. H., 458; inc., 443; ref., 153, 310, 350, 459, 462, 519
 Home, E., 276; ref., 306
 Honzik, C. H., 557; ref., 572
 Hooke, R., 101, 324, 328; inc., 102; ref., 121
 Horn, W., 451; inc., 450; ref., 461
 Hornbostel, E. M. v., 27, 389 f.; inc., 601; ref., 49, 398
 Horner, W. G., 590 f.; ref., 605
 Huber, G. C., 531; ref., 567
 Huddart, J., 183; ref., 196
 Hulin, W. S., 600 f.; ref., 607
 Humboldt, A. v., inc., 92
 Hume, D., 6, 86; inc., 19, 527, 575; ref., 45, 602
 Hunter, J., 276 f.; ref., 306
 Hunter, W. S., 76; inc., 12
 Hurst, C. H., 416; ref., 434
 Hurvich, L. M., 458; ref., 462
 Husserl, E. G., inc., 248
 Huygens, C., 54, 98, 108; ref., 121
 Hykes, O. V., ref., 124
 Ibn al Haitham, ref., 121
 Ishihara, S., 187
 Jacobson, E., ref., 568
 Jaensch, E. R., 249, 364, 370, 373, 376; inc., 418; ref., 261, 310, 394 f., 434
 James, W., 44, 85, 542, 575 f.; inc., 193, 528 f., 580, 592 f.; ref., 49, 52, 95, 259, 565 f., 570, 602-5
 Jastrow, J., 40; ref., 51
 Jeffries, B. J., ref., 197
 Jenkins, W. L., 493, 500, 508; ref., 518 f., 521
 Jones, A. T., ref., 392
 Jones, L. A., 130; ref., 151
 Joule, J. P., 86; ref., 96
 Jurin, J., 158 f.; ref., 190

- Kämpfe, B., 343; ref., 350
 Kahlenberg, L., 456; ref., 461
 Kames, Lord, inc., 190
 Kammler, A., 484; ref., 517
 Kant, I., 233 f.; inc., 28 f., 221, 248, 576
 Kast, L., 548; ref., 571
 Kato, G., 60
 Katz, D., 135, 249, 255, 512; inc., 251, 296; ref., 152, 261 f., 522
 Keith, A., ref., 432
 Kelvin, Lord, 86; inc., 327, 347; ref., 96
 Kemp, E. H., 425; ref., 433, 436
 Kemp, W., ref., 394
 Kempelen, W. de, 369, 371; ref., 395
 Kenkel, F., 596 f.; ref., 606
 Kepler, J., projection, 80, 227; optics, 98 f.; retinal image, 98-100, 222-4; retinal inversion, 224 f.; accommodation, 273, 276 f.
 inc., 97, 122, 155, 237, 272, 274, 608, 611
 ref., 121, 257, 306
 Kester, P., 601; ref., 607
 Kiesow, F., 452, 454, 487, 490, 496 f.; inc., 450, 501, 506; ref., 460 f., 517-21
 Kincaid, M., 480; ref., 516
 King, A. J., inc., 44; ref., 52
 Kircher, A., 157, 321 f.; ref., 190
 Kirschmann, A., 169 f.; ref., 193
 Klemm, O., 389 f.; ref., 49, 398
 Klimpfänger, S., ref., 262
 Knight, L., 508; inc., 511; ref., 521
 Knudsen, V. O., 344 f.; ref., 349 f.
 Köhler, W., projection, 83; isomorphism, 84, 90, 303; field theory, 252; transposition, 255; size constancy, 296; vowels, 370, 372-4; tonal attributes, 376, 378, 380; theory of hearing, 408; apparent movement, 599
 inc., 13, 250, 253, 596
 ref., 47 f., 96, 260 f., 310 f., 395-7, 433, 606
 Kölliker, A., 203, 530; inc., 404; ref., 217, 566 f.
 König, A., color j.n.d., 130; Weber's law, 138 f., color triangle, 147; luminosity curve, 179-82; color blindness, 187 f.; color theory, 201 f.; visual purple, 204
 inc., 11, 120, 142, 144, 149, 210, 609
 ref., 151, 153 f., 190, 195-7, 217-9
 Koenig, R., 319 f., 328-32, 334 f., 356, 370, 372-4; inc., 357 f.; ref., 347, 392 f., 395
 Koffka, K., 300-2, 584, 596; inc., 13, 152, 170, 250, 252 f.; ref., 47 f., 96, 152, 193, 262, 308, 310, 604, 606
 Kollert, J., 579 f.; ref., 603
 Korte, A., 596-8; inc., 601; ref., 606
 Koster, W., 181; ref., 195
 Kottenkamp, R., 479, 481, 484 f., 502; ref., 516, 520
 Krakauer, D., ref., 461
 Kratzenstein, C. G., inc., 156
 Krause, W., 451; ref., 461
 Kreidl, A., 542; ref., 570
 Kries, J. v., 130, 180 f., 188-90, 203-6, 384, 387; inc., 50, 142, 210, 364; ref., 49, 151, 154, 195, 197, 216, 218, 258, 397 f.
 Kröner, E., ref., 567
 Kronecker, H., 59; ref., 92
 Kühne, W., 205, 530; inc., 527; ref., 196, 218, 565 f.
 Külpe, O., number of sensations, 10; attributes, 11, 20-4, 375; empiricism, 31 f.; psychophysics, 44; number of colors, 130; nativism, 233 f.; local signs, 236; phenomenology, 246; number of pitches, 342; noise, 364
 inc., 3, 17, 23, 28, 30, 38, 47, 87, 133, 193, 379, 533
 ref., 46, 48-50, 151, 258 f., 349, 393, 396
 Kuile, E. ter, 416 f.; ref., 434
 Kundt, A., 320; inc., 240; ref., 260
 Laborde, J. V., 386; ref., 398
 Ladd, G. T., inc., 94, 193; ref., 570
 Ladd-Franklin, C., 129-31, 210 f.; inc., 145, 204; ref., 150 f., 216, 219
 Lagrange, J. L., ref., 392
 Laird, D. A., inc., 43; ref., 52
 Lambert, J. H., 144, 148; ref., 154
 La Mettrie, J. O. de, 7; ref., 45
 Landolt, E., 172; ref., 194
 Landry, O., 528; ref., 566

- Lane, C. E., 359, 366 f., 427-9; inc., 395; ref., 393 f., 436
- Lange, C. G., inc., 570
- Länge, N., 271; ref., 305
- Langenbeck, M. A., ref., 306
- Langford, C. H., ref., 192
- Langley, S. P., 178 f.; inc., 182; ref., 195 f.
- Lashley, K. S., 76 f.; inc., 12; ref., 94
- Láska, W., 244; ref., 261
- Leewenhoek, A. v., inc., 217
- Lehmann, A., 168, 410; inc., 407; ref., 192 f., 433 f.
- Lehmann, J. G., ref., 566
- Leibnitz, G. W., 85; inc., 28, 35, 50, 95
- Lennander, K. G., 547 f.; ref., 571
- Leonardo da Vinci, contrast, 165; colored shadows, 166; eye as camera, 222; light and shade, 266; aerial perspective, 266; binocular parallax, 283 f.
inc., 98 f., 155 f., 192, 206, 208, 268, 303
ref., 304, 308
- Lepidi-Chioti, G., 559; ref., 572 f.
- Lewes, G. H., inc., 528; ref., 566
- Lewy, W., 486; ref., 517
- Lieb, C. W., 555; ref., 572
- Lillie, R. S., 62; ref., 92
- Linke, P., ref., 605
- Linnaeus, K., 444, 453; ref., 459, 461
- Linné, K. v., *see* Linnaeus, K.
- Lipps, T., 245; inc., 258; ref., 261, 434
- Lissajous, J. A., 320, 330
- Listing, J. B., 230-2, 275 f.; ref., 124, 258, 306
- Lobé, J. P., 276; ref., 306
- Locke, J., 5, 69, 238, 490, 500; inc., 19, 28, 84 f., 158, 190, 198, 221, 466, 492, 527, 575; ref., 45, 257, 518, 602
- Löwenberg, A., 538
- Löwit, 495 f.; ref., 518
- Lohmann, W., 162 f.; ref., 191
- Longet, F. A., 551 f.; ref., 571
- Lorenz, G., 344; ref., 350
- Lorry, D., 444 f.; ref., 459
- Lotze, R., sensory continuum, 10; empiricism, 30, 233, 236 f.; isomorphism, 87; eye-movements, 232; local signs, 234-6; illusion, 238; ideomotor action, 529
inc., 9, 19, 28 f., 31-3, 38, 72 f., 221, 288, 381, 386, 483, 515, 526, 586, 604
ref., 46, 50, 96, 259 f., 517, 569
- Lovén, C., 450 f.; ref., 460
- Lucas, K., 59-61; inc., 419, 609; ref., 91 f.
- Luce, A., 562 f.; ref., 573
- Luciani, L., inc., 76, 94; ref., 513, 518, 564 f., 570
- Ludwig, C. F. W., 291; inc., 528; ref., 309, 565 f.
- Luft, E., 340-2, 345; ref., 349
- Lummer, O., inc., 145; ref., 154
- Lurie, M. H., 426-8; ref., 435 f.
- Macdonald, M. K., ref., 460
- MacDougall, R., 585 f.; ref., 604
- Mach, E., positivism, 12; isomorphism, 88 f.; color theory, 206 f.; central color processes, 212-4; form-quality, 255; tonal brightness, 378; theory of hearing, 408; vestibular sense, 537-42; time perception, 576-8; after-sensation of movement, 592; movement sensation, 593
inc., 13, 126, 150, 220, 407, 463, 528, 535 f., 544, 596
ref., 96, 122, 218, 257, 396, 433, 566, 569, 603-5
- MacLeod, R. B., ref., 152, 262, 604
- Magendie, F., 63, 274, 551 f.; inc., 3, 8, 93, 526; ref., 92, 305, 571
- Malebranche, N., inc., 267
- Malmud, R. S., ref., 521
- Mandelstamm, E., 129; ref., 150 f.
- Marbe, K., 271, 593; ref., 305, 604 f.
- Marey, E. J., 59; ref., 92
- Mariotte, E., inc., 155, 171; ref., 121
- Marloye, 319, 329
- Martin, C. L., 557; ref., 572
- Martius, G., 293 f.; inc., 299; ref., 310
- Masson, V., 137 f.; ref., 152
- Mathiesen, A., 601; ref., 607
- Matsumoto, M., 385; ref., 398
- Matteucci, C., 57; ref., 91
- Matthews, B. H. C., 531; inc., 65; ref., 93, 567
- Matthews, R., 85; ref., 93

- Maurolycus, F., 222; ref., 122, 257
 Maxwell, J. C., 144-6, 185 f., 201;
 inc., 139; ref., 154, 197, 217
 Mayer, A., 560; ref., 573
 Mayer, A. M., 365 f.; inc., 395; ref.,
 394
 Mayer, B., 457; ref., 461
 Mayo, H., inc., 286; ref., 308
 McDonaugh, A. R., ref., 48
 McDougall, W., 212, 380, 481 f.; inc.,
 210, 216; ref., 219, 396, 516
 McKeag, A. J., ref., 94, 514
 McKendrick, J. G., 410; ref., 50,
 432 f.
 Meenes, M., 511 f.; ref., 522
 Mehner, M., 579 f.; ref., 603
 Meinong, A., 16, 246, 255; ref., 47
 Meissner, G., 487; ref., 517
 Meltzer, S. J., 548; ref., 571
 Merkel, J., 43, 343 f.; inc., 37; ref.,
 51 f., 350
 Mersenne, M., 322-4; inc., 357, 409;
 ref., 346
 Mettler, F. A., ref., 95
 Metzger, W., ref., 304
 Metzner, R., 479, 483 f., 600; ref.,
 516, 606
 Meumann, E., 548, 562-4, 581 f., 584;
 ref., 571, 573, 603 f.
 Meyer, H., 291; inc., 302; ref., 192,
 309
 Meyer, M., 167, 170, 340, 376, 380,
 413-6; ref., 349, 396, 434
 Mill, Jas., 7, 9, 15, 545, 575; inc., 10,
 382, 527; ref., 46 f., 570, 602
 Mill, J. S., 9, 15, 18; ref., 46 f., 259
 Miller, D. C., 327, 370, 372; ref.,
 346 f., 395 f.
 Miller, J. G., ref., 604
 Miller, R. H., 420; inc., 66; ref., 93,
 435
 Miner, J. B., 585 f.; ref., 604
 Mitchell, J. F., ref., 571
 Modell, J. D., 370, 374; ref., 395
 Molyneux, W., 223-5; ref., 257
 Montgomery, M. F., ref., 573
 Moore, T. V., 47; ref., 47 f.
 More, L. T., 388; ref., 122, 398
 Morgan, C. T., 345; inc., 38; ref., 51,
 95, 350
 Mott, F. W., 534; ref., 568
 Müller, G. E., attributes, 21; psycho-
 physics, 40; isomorphism, 89 f.;
 vision, 120; cortical gray, 133,
 213 f.; kinesthetic set, 532 f.
 inc., 37, 42 f., 87, 135, 162, 210,
 216, 220, 247, 249 f., 596
 ref., 48, 50 f., 96, 125, 152, 216 f.,
 566 f.
 Müller, H., 203; ref., 217
 Müller, Joh., sensation, 8; nativism,
 29, 233 f.; nerve impulse, 57;
 velocity of nerve, 58; specific en-
 ergies, 63, 68, 70-3, 399; experi-
 mentalism, 117; vision, 118;
 retinal receptors, 203; retinal in-
 version, 224 f.; horopter, 228-30;
 eye-movements, 230; accommo-
 dation, 274, 277; auditory stimu-
 lation, 383; ear, 403; taste, 450;
 taste adaptation, 457; pain, 467;
 innervation sensations, 528; hun-
 ger, 554
 inc., 5, 28, 30, 34, 50, 77-80, 84 f.,
 92, 98, 112, 115-7, 119, 159,
 166, 191, 199, 217, 402, 404 f.,
 439, 527, 588, 610 f.
 ref., 46, 49, 91, 94, 124, 190, 257-9,
 305 f., 397, 432, 460, 565, 570,
 572
 Müller, L. R., 553, 560; ref., 571, 573
 Müller-Lyer, F. C., 243-5; ref., 261
 Münsterberg, H., 27, 387, 581, 586;
 inc., 381, 385; ref., 49, 398, 603
 Munk, H., 75 f.; ref., 95
 Munson, W. A., ref., 52
 Murray, E., 501, 503, 510 f.; ref.,
 520 f.
 Musschenbroek, P. v., 144
 Myers, C. S., 389; ref., 398
 Nafe, J. P., 493; ref., 518
 Nagel, W., 181, 188 f.; inc., 187, 191,
 193, 439; ref., 94, 194-7, 458,
 513
 Napoleon, inc., 74
 Natanson, 72; inc., 403; ref., 94
 Necker, L. A., 268 f.; ref., 304
 Neff, W. S., ref., 605
 Neumann, A., ref., 570
 Newman, E. B., 356, 359, 390 f.;
 ref., 52, 393, 398
 Newton, I., chiasma, 79; light, 98,
 108; color, 101-7, 126; physiolo-
 gy of color, 110 f.; Goethe, 112,

- 115; brightness, 132 f.; color mixture, 139-42; color circle, 145 f.; after-image, 158; contrast, 165; brightness of hues, 176 f.; optic decussation, 227 f.; sound, 322
inc., 6, 85-7, 97, 100, 112, 119, 122 f., 135, 142, 147, 155, 178, 187, 198-200, 207, 316, 609-11
ref., 122, 153, 190, 194, 257
Nichols, H., 575; ref., 602 f.
Nicolai, W., 555; ref., 572
Noble, W., 324 f.
Norr, C., 343; ref., 350
Nothnagel, H., 498; ref., 519
Nutting, P. G., ref., 196
- O'Connor, J., 420; inc., 66; ref., 93, 435
Oehrwall, H., 451, 454 f.; inc., 450; ref., 460 f.
Ogden, R. M., 379; ref., 396
Ohm, G. S., 326 f.; inc., 55; ref., 347
Okabe, T., ref., 568
Oppel, G. W., ref., 519
Oppel, J. J., 239, 266, 592; inc., 270; ref., 260, 304, 605
Orbison, W. D., ref., 262
Orfila, P., 558; ref., 572
Ostwald, W., 62; ref., 92
- Pacini, F., 530; ref., 566
Panum, P. L., 288; inc., 291; ref., 308 f.
Parinaud, H., 181, 205; ref., 195 f., 218
Parker, G. H., inc., 439, 450; ref., 458, 460
Parkinson, J. S., inc., 43; ref., 52
Parrish, C. S., 484 f.; ref., 517
Parry, C. H. H., ref., 346
Parsons, J. H., 182; ref., 195 f., 216
Passy, J., 447; inc., 439; ref., 458, 460
Paulsen, E., 440; ref., 458
Pavlov, I. P., inc., 534, 555, 560; ref., 573
Peacock, G., ref., 123, 306
Pearson, K., 12
Peirce, C. S., 40; ref., 51
Peirsec, N. C. F. de, inc., 157
Pendleton, C. R., inc., 471; ref., 514
Penzoldt, F., 442; ref., 459
- Phillips, G., 422; ref., 435
Pierce, A. H., 385 f.; ref., 260, 397 f.
Pigot, T., 325
Pillsbury, W. B., 484 f., 532; ref., 517, 567
Piper, A., 162 f.; ref., 191
Pirrenne, M. H., ref., 196
Pisco, inc., 260
Pi-Suñer, J., 531; ref., 566 f.
Plateau, J. A. F., psychophysics, 42 f.; Talbot-Plateau law, 144 f.; phenomenology of color, 156 f.; stroboscope, 589 f.; after-sensation of movement, 591 f.
inc., 610
ref., 51, 154, 190-2, 605
Plato, inc., 85, 575
Poggendorff, J. C., 241; ref., 260
Pole, W., 186; ref., 197
Politzer, A., 383; ref., 397
Pollak, J., 543; ref., 570
Polvogt, L. M., 425 f.; ref., 436
Ponzo, M., inc., 486; ref., 517
Poppelreuter, W., 296; ref., 310
Porta, G. B., 222, 227; ref., 257
Porterfield, W., 108, 227, 290; inc., 285, ref., 122 f., 305
Pratt, C. C., 361 f.; ref., 52, 153, 350 f., 394, 519
Pretori, H., 168 f.; ref., 193
Preyer, W., limits of hearing, 334 f.; pitch j.n.d., 340; combination tones, 356; auditory localization, 385-7; after-sensation, 502
inc., 357
ref., 348, 393, 398
Priestley, J., 290; inc., 155, 183; ref., 190, 257, 305, 309
Protagoras, 4
Ptolemy, 267, 316-8; inc., 155 f.
Purdy, D. M., 132; ref., 151
Purkinje, J. E., vision, 117 f.; indirect vision, 171; Purkinje phenomenon, 177 f., 181; accommodation, 277; vertigo, 536 f.; seen movement, 588
inc., 115 f., 159, 184, 204, 512, 535, 538, 540
ref., 124, 193 f., 306, 568, 605
Pythagoras, 314 f., 318 f., 323; inc., 312
Quetelet, L. A. J., inc., 590

- Rahn, C., 23; ref., 48
 Ramsay, W., 447; ref., 460
 Rand, G., 175 f.; ref., 193 f.
 Rauber, A., 530; ref., 566
 Rayleigh, Lady, inc., 388
 Rayleigh, Lord, color blindness, 187;
 auditory sensitivity, 336; aural
 harmonics, 359; auditory local-
 ization, 384 f.; binaural beats,
 388
 inc., 381, 390
 ref., 197, 348, 393, 395, 397 f.
 Reboul, J. A., inc., 430; ref., 436
 Reid, T., 7, 13 f., 575; inc., 19, 74;
 ref., 46 f., 602
 Reissner, E., inc., 404; ref., 432
 Rentz, T., 342-4; ref., 350
 Retzius, C., 404; ref., 432
 Reute, C. G. T., ref., 258, 306
 Révész, G., 376, 380, 512; ref., 396,
 522
 Ribot, T., ref., 49, 258 f.
 Rich, G. J., 370, 374, 376, 378-80;
 inc., 21; ref., 395 f.
 Richards, T. W., 456; ref., 461
 Richardson, L. F., inc., 43; ref., 52
 Richey, C., ref., 571
 Richter, C. P., 557; ref., 572
 Richter, I. A., ref., 304, 308
 Richter, J. P., ref., 304, 308
 Riesz, R. R., 344 f.; ref., 350
 Rigaud, J. F., ref., 308
 Rittenhouse, D., 266; ref., 304
 Ritter, C., ref., 571
 Rivers, W. H. R., 471; inc., 548; ref.,
 515, 565, 571
 Roaf, H. E., 406, 409; ref., 433
 Robinson, V., ref., 124
 Römer, O. C., 98; ref., 121
 Roff, M. R., ref., 604
 Rogers, F. T., 557; ref., 572
 Roget, P. M., 588; ref., 605
 Rolando, L., inc., 94; ref., 568 f.
 Rollett, A., 531; ref., 567
 Romberg, M. H., ref., 569
 Romieu, J. B., 353; inc., 357; ref., 392
 Ross, J. S., inc., 43; ref., 52
 Roux, J., 553; ref., 571
 Rubin, E., 249-51, 566 f., 511; inc.,
 253, 587; ref., 261, 520
 Ruckmick, C. A., 587; inc., 583; ref.,
 397, 604
 Ruffini, A., 531; ref., 566
 Rullier, P., ref., 572
 Rumford, Count, 166; ref., 192
 Runge, P. O., 148 f.; ref., 154
 Rutherford, W., 413; inc., 414; ref.,
 434
 Sachs, M., ref., 193
 Salinas, F., 318 f.
 Sander, F., inc., 253
 Sanford, E. C., ref., 260, 309
 Sanson, L. J., 277-9; ref., 306
 Saul, L. J., inc., 65; ref., 93, 435
 Sauveur, J., harmonics, 325; limits of
 hearing, 332-4; pitch j.n.d., 339;
 beats, 352; harmonic fusion, 359
 inc., 38, 357
 ref., 347 f., 392
 Savart, F., 333-5, 402; inc., 324; ref.,
 348, 431
 Scahill, G. H., 598; ref., 606
 Scaliger, J. C., 526
 Schäfer, E. A., inc., 439, 450, 525;
 ref., 94, 432, 458, 460, 518, 564
 Scheibler, J. H., 319 f., 329
 Scheidt, V. P., ref., 196
 Scheiner, C., 223, 273, 275 f.; inc.,
 272, 274; ref., 257, 305 f.
 Schelske, R., inc., 174; ref., 194
 Schenck, F., 212; inc., 210, 216; ref.,
 219
 Scherffer, C., 158; inc., 156, 160; ref.,
 191
 Schiff, J. M., 467, 560; ref., 513, 573
 Schiff, M., 453; ref., 461
 Schirmer, R., ref., 194
 Schlesinger, E. G., 548 f.; ref., 571
 Schlick, M., 12
 Schlosberg, H., ref., 521
 Schmidt, C., 560; ref., 573
 Schneider, G. H., 594, 600; ref., 606
 Scholz, W., ref., 606
 Schopenhauer, A., 115 f.; inc., 199;
 ref., 124
 Schott, G., inc., 347; ref., 257
 Schottky, W., ref., 394
 Schriever, W., ref., 308
 Schröder, H., 266, 269; ref., 304 f.
 Schubotz, F., ref., 310
 Schultze, M., 179, 204, 210, 440;
 inc., 204, 450; ref., 195, 217, 458
 Schumacher, G. A., ref., 518
 Schumann, F., 244 f., 247-9, 532 f.;

- inc., 250, 587; ref., 193, 261, 566 f.
- Schur, E., ref., 310
- Schwalbe, G., 450 f.; ref. 460
- Scripture, E. W., inc., 385
- Sédillot, C. E., 552; ref., 571
- Seebeck, A., 184 f., 319, 329, 340; inc., 178, 188, 190, 196, 324, 405; ref., 195, 347
- Shambaugh, G. E., 409; ref., 433
- Sheehan, M. A., 297; ref., 262, 308, 310
- Sheppard, H., inc., 164; ref., 192
- Sherman, F. D., 181; ref., 195
- Sherren, J., 471; inc., 548; ref., 515, 565, 571
- Sherrington, C. S., 71, 492, 525, 528 f., 534 f.; ref., 94, 513, 518, 564, 566-8
- Shlaer, S., 182; ref., 196
- Shore, J., 319, 323, 329
- Shore, L. E., 452; inc., 454; ref., 461
- Shower, E. G., 340-2, 345; ref., 349
- Sinden, R. H., ref., 154
- Sinsteden, W. J., 270; ref., 305
- Sivian, L. J., ref., 349
- Skinner, B. F., 558; ref., 572
- Skramlik, E. v., inc., 437, 439, 450; ref., 458-60, 513, 564
- Slade, H., inc., 31
- Sloan, L. L., ref., 194
- Smith, A. H., ref., 190
- Smith, M. E., 362; ref., 394
- Smith, R., inc., 285; ref., 190
- Snell, W., 98; ref., 121
- Snow, W. B., 385; ref., 398
- Soemmerring, S. T., 554; ref., 571
- Sorge, G. A., 353; ref., 392
- Soury, J., ref., 94
- Spiro, K., ref., 51, 460
- Spoerl, H. D., ref., 94
- Spurzheim, G., ref., 94
- Stampfer, S., 590; ref., 605
- Starke, P., 344; ref., 350
- Starling, S. G., ref., 347
- Steinach, E., 64; ref., 93
- Steinberg, J. C., 385; ref., 398
- Steinbuch, J. G., 526; inc., 528; ref., 565
- Steinheil, C. A., inc., 153
- Stern, L. W., ref., 349, 602
- Sternberg, W., 456; inc., 454; ref., 461
- Stetson, R. H., inc., 586; ref., 604
- Stevens, L. T., ref., 603
- Stevens, S. S., attributes, 25 f.; tonal quanta, 345; combination tones, 356; aural harmonics, 359; tonal attributes, 376-9, 381; auditory localization, 390 f.; pitch in cochlea, 426-8; theory of hearing, 430 f.
- inc., 38, 44
- ref., 46, 49, 51 f., 95, 152, 347-51, 393-8, 432, 434-6
- Stewart, B., inc., 87; ref., 95
- Stewart, D., 7, 14; ref., 46 f.
- Stich, A., 453; ref., 461
- Stilling, J., 187; ref., 197
- Stirling, W., ref., 92
- Stout, G. F., ref., 151
- Stratton, G. M., 237; ref., 259, 519
- Strauss, H. H., 501; ref., 520
- Strenger, F., 178; ref., 195
- Strümpell, A., 532; ref., 567
- Strutt, J. W., *see* Rayleigh, Lord
- Studnicka, F. K., ref., 124
- Stücker, H., 340; ref., 349
- Stumpf, C., sensory continuum, 10; nativism, 29 f., 233; brightness, 134; local signs, 235; phenomenology, 247, 249; fusion, 360-2; noise, 364; tonal attributes, 376, 378; aural stimulation, 382; tonal gaps, 410
- inc., 21, 23, 28, 33, 38, 340, 381, 394, 444, 612
- ref., 46, 50, 152, 259, 307, 393 f., 396 f., 433
- Sturm, J. C., 275; ref., 306
- Sullivan, A. H., 510-2; ref., 521 f.
- Swammerdam, J., 55; ref., 91
- Szabadföldi, M., 466; ref., 513
- Szokalsky, V., 171; inc., 199; ref., 124, 193
- Talbot, E. B., 22; ref., 48
- Talbot, W. H. F., 144 f.; ref., 154, 308
- Tarchanow, J., 385; ref., 397
- Tartini, G., 352 f.; ref., 392
- Tawney, G. A., 479 f.; ref., 516
- Taylor, E., inc., 43; ref., 52
- Terman, S. W., inc., 189; ref., 197
- Terpander, 314
- Theophrastos, inc., 122
- Thiéry, A., 245; ref., 261

- Thompson, B., *see* Rumford, Count
 Thompson, S. P., 384 f., 387 f., 593; ref., 397, 605
 Thomson, William, *see* Kelvin, Lord
 Thouless, R. H., 256, 297, 299; ref., 262, 310 f.
 Thunberg, T., 503, 506; inc., 504; ref., 513, 520 f.
 Tiédemann, F., 552; ref., 571
 Tischler, E., 343; ref., 350
 Titchener, E. B., number of sensations, 10; context theory, 17 f.; tonal volume, 21; attributes, 23 f.; unity of senses, 27; empiricism, 31 f.; psychophysics, 42 f.; brightness, 134 f.; color mixture, 143; nativism, 233 f.; convergence, 272; limits of hearing, 336; vowels, 374; tonal attributes, 376, 378, 380; two-point limen, 482; touch blends, 510, 512; kinesthetic contents, 533; sexual sensation, 564; rhythm, 586; apparent movement, 597
 inc., 3, 6 f., 11 f., 15, 21, 28, 37 f., 120, 130 f., 136 f., 152, 611
 ref., 46-51, 151 f., 258-61, 305, 307, 309, 348-51, 394, 396 f., 460, 516, 521, 567 f., 573, 603 f.
 Toepler, A., 320, 336; ref., 348
 Tolman, E. C., 13, 18, 47 f., 557; inc., 12; ref., 47 f., 572
 Torricelli, E., 321
 Toulouse, E., ref., 459
 Toynbee, J., 537
 Trendelenberg, W., ref., 196
 Treviranus, G. R., 274 f.; inc., 119, 203, 217; ref., 124, 305 f.
 Trimble, O. C., 390; ref., 397 f.
 Troland, L. T., 66, 164, 418; inc., 215, 423; ref., 93, 125, 153, 192 f., 195, 258, 396, 434 f.
 Trotter, W., 474; ref., 515
 Troxler, D., 171; ref., 193
 Tschermak, A., ref., 191, 217
 Tung, S., 510; ref., 521
 Turberville, D., 183; ref., 196
 Turnbull, L., inc., 335
 Uccello, P., 265
 Uhlmann, R. F., 501; ref., 520
 Ullrich, H., 479, 481, 484 f., 502; ref., 516, 520
 Umrath, K., ref., 93
 Unzer, J. A., 55; ref., 91
 Upton, M., ref., 436
 Urban, F. M., 39; inc., 37; ref., 51
 Urbantschitsch, V., 385, 502 f.; ref., 520
 Valenti, A., 559; ref., 573
 Valentin, G., 453; ref., 461
 Vance, T. F., 340 f.; ref., 349
 Vannod, T., 481; ref., 516
 Vaschide, N., ref., 459
 Vater, A., 530; inc., 566
 Vesalius, A., inc., 91
 Vierordt, K., psychophysics, 40; luminosity curve, 178; two-point limen, 477 f.; thermal stimulus, 490 f.; time-sense, 578 f.; after-sensation of movement, 593; movement sensation, 594
 inc., 342 f., 350, 479, 581, 604
 ref., 51, 195, 515, 518, 603, 605 f.
 Vieth, G. U. A., 229, 353; ref., 257, 392
 Vinci, *see* Leonardo da Vinci
 Vintschgau, M. v., 454, 457; inc., 439, 450; ref., 458, 460-2
 Viviani, V., 322
 Voeste, H., inc., 164; ref., 192
 Volkmann, A. W., 73, 225, 480; inc., 41, 119, 484; ref., 94, 124, 516
 Volkmann, J., 345; inc., 38; ref., 51 f., 350 f.
 Volta, A., 55-7; inc., 609; ref., 91
 Voth, A. C., 599; ref., 606
 Wada, T., 557; ref., 572
 Waetzmänn, E., inc., 411; ref., 432, 434
 Wagner, R., inc., 439, 450; ref., 50, 94, 124, 259, 275, 306, 458, 460, 513, 516, 518, 569-72
 Wagoner, K. S., ref., 518
 Wallach, H., 391 f.; ref., 398
 Waller, A. D., 412; inc., 417; ref., 434
 Wallis, J., 324 f.; inc., 357; ref., 347
 Ward, J., inc., 258
 Warren, H. C., ref., 150
 Washburn, A. L., 555 f.; ref., 572
 Washburn, M. F., 22; inc., 557; ref., 48, 572
 Watson, J. B., 11 f.; inc., 13, 534, 557; ref., 46, 572

- Watson, W., ref., 347
 Watt, H. J., 376, 380, 417; ref., 394, 396, 434
 Weber, E. F., 383; inc., 387; ref., 397
 Weber, E. H., Weber's law, 34 f.; psychophysics, 42; projection, 80; auditory localization, 383; smell stimulus, 440 f.; tactual senses, 465 f.; pain, 467; two-point limen, 475-7, 479, 481; error of localization, 483-5; thermal stimulus, 490-2; pressure j.n.d., 495 f.; thermal summation, 498 f.; thermal adaptation, 500; internal sensibility, 545-7; hunger, 555
 inc., 10, 29 f., 81, 288, 351, 387, 463, 487, 494, 497, 499, 501, 509, 513
 ref., 50 f., 259, 397, 458, 513, 515-9, 565, 570-2
 Weber, W. E., inc., 397
 Wegel, R. L., 337 f., 359, 366 f., 427-9; inc., 395; ref., 348 f., 393 f., 436
 Weinmann, R., ref., 94
 Weiss, A. P., inc., 12
 Weld, H. P., 47 f.; ref., 47
 Werner, H., ref., 431
 Wertheimer, M., 250-2, 389 f., 595 f., 599; inc., 5, 48, 224, 253, 600-2, 610 f.; ref., 605 f.
 Westphal, H., 129; ref., 150
 Wettendorff, H., 560; ref., 573
 Wever, E. G., 419-23; inc., 65 f.; ref., 93, 434 f.
 Wheatstone, C., 285-7, 371; inc., 263, 282, 611; ref., 308, 395
 Wheeler, R. H., ref., 48
 Whewell, W., ref., 122, 346
 Whitchurch, A. K., 600; ref., 606
 White, S. D., ref., 349
 Whytt, R., 55; inc., 63; ref., 91
 Wien, M., 336 f., 344; ref., 348, 350
 Wilcocks, R. W., 24; ref., 49
 Wilkinson, G., 409; inc., 411; ref., 432-4
 Wille, H. H., inc., 43; ref., 52
 Williams, H. B., inc., 65; ref., 93
 Williams, R. E., 181 f.; ref., 196
 Willis, R., 369-71; ref., 395
 Willis, T., 54; ref., 91
 Wilska, A., 339; ref., 349
 Wilson, G., 185 f.; ref., 196 f.
 Wilson, H. A., 389; ref., 398
 Winsor, A. L., 559; ref., 573
 Wirth, W., inc., 605
 Wittmaack, K., 425; ref., 433
 Woinow, M., inc., 174; ref., 194
 Wolf, Abraham, ref., 91, 122, 346
 Wolf, August, 342-4; ref., 350
 Wolff, H. G., ref., 518 f.
 Wollaston, W. H., 79, 227 f., 108, 111, 127 f., 401; ref., 149, 257, 431
 Woodrow, H., 582, 585 f.; inc., 578; ref., 603 f.
 Woods, E. L., ref., 568
 Woodworth, R. S., inc., 94, 193, 440; ref., 303, 309, 458, 567, 570
 Wooster, M., 238; ref., 260
 Wrightson, T., ref., 432
 Wunderli, A., 466; ref., 513
 Wundt, W., associationism, 9; perception, 15 f.; attributes, 19-21, 375; unity of senses, 27; empiricism, 31, 233 f., 236; color cone and sphere, 148; contrast, 170; local signs, 235 f.; illusion, 240-4; convergence and accommodation, 279-81; limits of hearing, 334; fall apparatus for noise, 342; theory of hearing, 416 f.; taste qualities, 454; sensations of innervation, 525; kinesthetic contents, 533; internal sensibility, 546; time perception, 576; rhythm, 583 f.; after-sensation of movement, 592; seen movement, 593
 inc., 3 f., 8 f., 11, 13, 23, 28, 30, 37, 42, 48, 87, 116, 169, 210, 216, 221, 261, 272, 289, 293, 340, 343, 381, 386, 484, 519, 526, 528, 574, 580, 586, 612
 ref., 46 f., 49-51, 154, 193, 218, 258-61, 307, 348, 350 f., 434, 461, 566 f., 570, 602, 604 f.
 Wyss, H. v., 451; ref., 461
 Yokoyama, M., 24; ref., 49
 Yoshii, U., 411, 425; ref., 433 f.
 Young, P. T., ref., 260
 Young, T., specific energies, 70; light, 108; accommodation, 109 f., 275-7; color theory, 110-2,

Young, T., *continued*

199-201; color, 127; primary colors, 128; color triangle, 146, contrast, 167; indirect vision, 171; difference tones, 353

inc., 72, 108, 115, 119, 122, 129, 140, 172, 185, 203, 228

ref., 122 f., 192 f., 216, 306, 392

Zarlino, G., 318

Zebrowski, E. v., 560; ref., 573

Zenneck, 453; ref., 461

Zigler, M. J., 443, 501, 510-2; ref., 459, 517, 520, 522

Zöllner, F., 241; inc., 31; ref., 260

Zoll, P. M., 24; ref., 49

Zotterman, Y., 65; ref., 93

Zurmuhl, G., 324, 378; ref., 396

Zwaardemaker, H., handbook, 438 f.; smell organ, 440; olfactometry, 441-3; smell laws, 443 f.; smell classes, 444-6

inc., 438

ref., 458-60

INDEX OF SUBJECTS

- Abstraction, Kulpe, 23, 48 f.
- Accidental colors, 156, 160
- Accommodation, convergence, 279-82, 307
 - cue to distance, 272-82, 302 f., 305-7
 - function, 109 f., 122 f., 272-4, 302 f.
 - theories, 274-8, 306 f.
 - Young, 109 f., 123
- Acoustics, *see* Sound
- Action potentials, auditory nerve, 423 f., 435 f.
 - sensory, 64-6
- Adaptation, Aubert, 160 f., 191
 - color, 163 f., 191 f.
 - dark, 160-2
 - Hering, 161 f., 164, 191
 - light, 162 f.
 - pain, 501, 520
 - pressure, 500 f., 519 f.
 - smell, 443, 459
 - taste, 457, 461
 - terminology, 191
 - thermal, 498, 500, 519
 - vestibular, 536, 539, 543 f., 570
 - visual, 160-4
- Adequate stimulation, 71 f., 94
- Aerial perspective, 266 f.
- After-images, visual, 157-60, 190 f.
- After-sensations, tactual, 501-4, 520
 - vestibular, 536, 539, 543 f., 570
 - visual movement, 591-3, 605
- Alimentary canal, excretory processes, 564
 - gastric processes, 563 f.
 - hunger, 551-8, 571 f.
 - sensibility, 546-51, 571
 - thirst, 558-61, 572 f.
- Alley problem, 290, 294-6, 310
- All-or-none law, 60-2, 66, 92, 214, 419, 423 f.
- α -movement, 597
- Amplification, electrical, 332
- Ampullar sense, *see* Vestibular sensibility
- Anaglyptoscope, 266
- Animal electricity, 56-8, 91
- Animal spirits, 53 f., 62, 91
- Apparent movement, attitude, 598
 - auditory, 601, 607
 - cortical short-circuit, 596, 599, 606
 - determinants, 598 f.
 - early investigations, 588-92, 605
 - eye movements, 591-3, 599, 605
 - field theory, 599
 - illusions, 588-90, 605
 - Korte's laws, 598
 - negative after-sensation, 591-3, 605
 - optimal movement, 595-8, 606
 - ϕ -phenomenon, 595-8, 606
 - primacy, 594, 605 f.
 - sensation given, 594, 605 f.
 - stroboscopic, 589-91, 605
 - subjective complement, 593
 - tactual, 599-601, 606 f.
 - types, 595-7, 606
 - Wertheimer's experiment, 595 f., 602, 606
- Apparent size, 289 f., 309
 - See also* Size Constancy
- Appetite, 551-3, 557 f., 562 f., 572
 - See also* Hunger
- Appunn lamella, 334
- Articular sensation, 532
- Articulation, 253
- Associationism, 14-6
 - sensation, 3-10, 46
- Attributes, 3 f.
 - actuality, 23 f.
 - attention, 22, 24
 - common, 27
 - criteria, 20 f., 25 f.
 - dimensions, 24
 - duration, 576 f., 603
 - elements, 22
 - Hornbostel, 27
 - Külpe, 20-3

Attributes, *continued*

- learning, 26 f.
- number, 26 f.
- references, 48 f.
- senses, 574
- Stevens, 25 f.
- stimulus, 49, 25 f., 377-81, 396 f.
- Titchener, 22-4
- tones, 21 f., 25 f., 375-81, 396 f.
- unity of senses, 27, 49
- vision, 21
- Wundt, 19 f.
- See also* Sensation
- Aubert-Förster law, 249, 309 f.
- Audibility curve, 336-8, 348 f.
- Audiogram, 337 f., 348 f.
- Audition, *see* Auditory localization, Auditory theory, Ear, Noise, Pitch, Tone, Vowels
- Auditory localization, apparent movement, 601, 607
 - binaural beats, 387 f.
 - binaural ratio, 384 f., 390 f., 397 f.
 - dichotic differential, 382 f., 387-91
 - dichotic intensity, 383-5, 390 f., 397 f.
 - dichotic phase, 384, 387-91, 397 f.
 - dichotic stimulation, 382, 387-91
 - dichotic time, 389-91, 398
 - semicircular canals, 386 f., 398, 541
 - space continuum, 381 f., 386 f.
 - tactual cues, 383, 387, 398
 - tridimensional, 391 f., 398
- Auditory theory
 - action potentials in nerve, 423 f., 435 f.
 - cochlear dynamics, 429-31, 436
 - direct cochlear lesions, 426-8, 436
 - duplicity, 364, 418
 - frequency, 413-8, 434
 - Haller, 401, 431
 - localization of pitch in cochlea, 424-9, 436
 - M. Meyer, 413-6, 434
 - models of ear, 410, 433
 - place, 406-11, 416 f., 432-4
 - platoon volleys, 421-4, 435
 - pressure-pattern, 412, 434
 - resonance, 404-11, 432-4; background, 404 f., 432; beats, 408; bright and dull, 408, 433; combination tones, 408; difficulties, 406 f.; loading, 409, 433; loud-

- ness, 408; Ohm's law, 408; principle of maximal amplitude, 408 f., 431, 433; resonators, 406 f., 432 f.; tectorial membrane, 409 f., 433; tension, 409, 433
- stimulation lesions, 411, 425 f., 433 f., 436
- telephone, 413
- tonal gaps, 410 f., 433
- Aural harmonics, 358 f., 393
- Beats, beat tone, 353, 356, 392
 - binaural, 387 f.
 - masking, 366 f.
 - origin, 352-7, 392
 - theory, 355 f., 408
- Behavioral operationism, *see* Positivism
- Behaviorism, 46
 - behavioristics, 13
 - sensation, 11 f.
- Bell-Magendie law, 63, 92 f.
- Binaural ratio, 384 f., 390 f., 397 f.
- Binocular parallax, 282-5, 307 f.
- β -movement, 596 f.
- Body-mind relation, *see* Mind-body relation, Isomorphism
- Bow movement, 597
- Brain, *see* Isomorphism, Localization of brain function
- Brightness, luminosity curve, 176-82, 194-6
 - tonal, 378, 396
 - See also* Color
- Brightness constancy, 255
- Cerebrum, *see* Isomorphism, Localization of brain function
- Chemical senses, 437 f.
 - See also* Smell, Taste
- Circular reflex, 527 f., 565
- Closure, 253
- Cochlea, *see* Ear
- Cochlear response, 420-3, 435
- Coenesthesia, 545
- Cold, *see* Thermal sensibility
- Color, accidental, 156, 160
 - achromatic series, 133 f.
 - adaptation, 163 f., 191 f.
 - after-images, 157-60, 190 f.; negative, 158 f.; positive, 157 f.

Bezold-Brücke phenomenon, 131 f., 151
 black, 133 f., 151 f.
 brightness, 106 f., 132-6
 central processes, 212-4
 color blindness, 182-90
 complementaries, 139-43, 154
 contrast, 169 f.
 cortical gray, 133 f.
 discriminable hues, 129 f., 150 f.
 double representation, 135 f., 152
 early theories, 100 f., 121 f.
 elementary hues, 127-31, 150 f.
 evolution, 210-2
 Goethe, 112-6, 123 f.
 homogeneous light, 102 f.
 indirect vision, 171-6, 193 f.
 intensity, 132-6
 light sense, 132 f.
 memory, 255 f.
 modes of appearance, 135, 152
 Newton, 101-7, 122
 objects, 107
 phenomenal green, 131, 151
 phenomenology, 113 f., 124, 135 f., 152
 primaries, 104 f., 127-31, 150 f., 202 f.
 principal hues, 129, 150
 refraction, 101-7
 relation to tones, 104 f.
 shadows, 157, 192
 simplicity, 130 f., 151
 spectra, 156, 160
 stimulus, 126-32, 149-51
 theory, all-or-none law, 214; Aubert, 207; brain, 212-4; Donders, 212 f., 219; duplicity, 203-6, 217 f., Ebbinghaus, 219; Edridge-Green, 219; evolutionary, 210-2; excitation curves, 200, 202; G. E. Müller, 213, 219; Goethe, 114 f.; Hecht, 215 f., 220; Helmholtz, 173 f., 200-2; Hering, 173-5, 194, 206-9, 218; indirect vision, 173-6, 194; König, 219; Ladd-Franklin, 210-2, 219; McDougall, 212, 219; Newton, 107, 110 f., 198 f.; references, 216-20; rods and cones, 203-6, 217; Schopenhauer, 115 f.; von Kries, 205 f.; Wundt, 218 f.; Young, 110-2, 123, 199 f.

visual purple, 205, 217 f.
 white light, 103 f.
 zones, 172-6, 194
See also Adaptation, Color blindness, Color diagrams, Color mixture, Contrast, Purkinje phenomenon, Vision
 Color blindness, anomalous, 189
 Dalton, 108 f., 184, 196
 deuteranopia, 188 f.
 dichromatic, 188 f., 197
 early cases, 183 f., 196
 monochromatic, 189
 protanopia, 188 f.
 tests, 186 f.
 types, 184-9, 197
 Color diagrams, Ebbinghaus' pyramid, 148 f.
 Helmholtz's circle, 145
 Helmholtz's triangle, 147, 201
 König's triangle, 147
 Lambert's pyramid, 148
 Maxwell's triangle, 146 f.
 Newton's circle, 139 f.
 references, 154
 Runge's pyramid, 148 f.
 Wundt's cone, 148
 Wundt's sphere, 148
 Color mixture, Grassmann, 141-3
 Helmholtz, 140-5, 153 f.
 Hering theory, 209
 laws, 139-43, 153 f.
 methods, 144 f.
 Newton, 106, 139 f.
 Newton's circle, 139 f.
 Talbot-Plateau law, 145, 154
 Combination tones, distortion, 355-7
 origin, 352-7, 392 f.
 theory, 408
 Common sensibility, *see* Gemeingefühl
 Compass test, 475-7
 Conscious attitudes, 533 f., 567 f.
 Conscious present, 576, 602
 Consonance, 360
 Constancy phenomenon, 254-6, 262, 288-99, 308-11
See also Size constancy
 Context theory, Berkeley, 6
 perception, 17 f., 47 f.
 rhythm, 586 f., 604
 Titchener, 17 f., 47 f.
 Contrast, apparent movement, 593

- Contrast, *continued*
 brain, 170 f., 193
 brightness, 168 f
 color, 169 f.
 contours, 167, 170, 193
 Helmholtz, 167 f., 192
 Leonardo, 166
 visual, 165-71, 192 f.
 Convergence, accommodation, 279-82, 307
 cue to distance, 271 f., 302 f., 305, 307
 Cornea, accommodation, 276, 306
 Corresponding points, disparity, 284 f., 287 f., 308
 Cutaneous sensation, *see* Touch

 Daedaleum, 590 f.
 Deaf mutes, vertigo, 542, 570
 Deep sensibility, 471-3, 523 f., 565
 Density, tonal, 377, 380 f., 397
 Dichotic stimulation, 382, 387-91
 Dichromatic vision, 187-9, 197
 Dichromic vision, 186, 188
 Difference tones, 352-7, 392 f.
 Dimensions of consciousness, *see* Attributes
 Diotic stimulation, 382
 Discrimination, *see* Weber function
 δ -movement, 597
 Donders' law, 230-2, 258
 Double representation, 135 f., 152, 300 f.
 Drive, appetite, 551-3, 557 f., 562 f., 572
 sensation, 561-3
 thirst, 559-61, 563, 573

 Ear, action potentials in nerve, 423 f., 435 f.
 anatomy, early knowledge, 400 f., 431
 cochlea, early theories, 402-4; histology, 404-7, 411, 432
 cochlear dynamics, 429-31, 436
 cochlear response, 420-3, 435
 direct lesions, 426-8, 436
 localization of pitch in cochlea, 424-9, 436
 middle, 401 f., 431 f.
 platoon-valley theory, 421-4, 435
 round window, 402 f.
 sensitivity, 339, 348 f.
 stimulation lesions, 411, 425 f., 433 f., 436
 Wever-Bray effect, 420-3, 435
 Emmert's law, 292 f., 310
 Empiricism, 3-6, 49 f.
 behaviorism, 33 f.
 Helmholtz, 31
 Kulpe, 31 f.
 Lotze, 30
 origins, 28 f.
 sensation, 45
 Titchener, 32
 visual space, 233-7, 258 f.
 Wundt, 31
 Epicritic sensibility, 471-5, 515
 Equal temperament, 317-9
 Equilibration, *see* Vestibular sensibility
 Error of localization, *see* Touch
 Eye, accommodation, 272-9, 305-7
 convergence, 271 f., 305
 retinal image, 222-5
 See also Vision
 Eye-movements, Donders' law, 230-2, 258
 Listing's law, 230-2, 258
 space perception, 230-3, 258
 torsion, 232 f.

 Fechner function, *see* Psychophysics
 Field theory, 246-52, 299-303, 311
 apparent movement, 599
 Figure and ground, 250 f., 253
 Formants, 372 f.
 Form-quality, 16 f., 47, 246
 Fourier analysis, 325-8, 347
 Fusion, tonal, 359-62, 393 f.

 Galton whistle, 333, 335 f.
 Gastric perceptual patterns, 563
 Gemeingefühl, 465, 467, 545-7
 Geneticism, *see* Empiricism
 Gestalt psychology, form, 252-6, 261 f., 299-303, 311
 Gestalten, visual form, 246-52, 261, 299-303, 311
 See also Visual space perception
 Gestaltqualität, *see* Form-quality
 Giddiness, 535 f., 568
 γ -movement, 597
 Golgi spindles, 531, 567
 Grave harmonics, 353
 Greek music, 313-6

- Harmonics, aural, 358 f., 393
 grave, 353
 limits of hearing, 333-6
 nature, 324 f., 347
 Ohm's law, 326-8
 timbre, 357-9, 393
 Hearing, *see* Auditory localization,
 Auditory theory, Ear, Music,
 Noise, Pitch, Tone, Vowels
 Heat, ambiguity of term, 508 f., 521
 cold-warm fusion, 504-8, 521
 grill, 506 f., 520 f.
 intensity, 508
 pain-warmth fusion, 508, 521
 quality, 507, 521
 Hering window, 168
 Horopter, 226-30, 257 f.
 retinal disparity, 284 f., 287 f., 308
 Hue, *see* Color
 Hunger, appetite, 551-3, 557 f.,
 562 f., 572
 central origin, 553, 571
 contractions, 555-7, 572
 drive, 551-3, 557 f., 562 f., 572
 early theories, 551-3, 571
 gastric stimulus, 553-7, 571 f.
 general sensation, 552 f., 571
 theories, 552-7, 571 f.
 Idea, Hume, 6
 Ideomotor action, 529
 Illusions, filled and empty space,
 239 f.
 geometrical, 238-45, 260 f.
 Helmholtz, 240, 243
 Hering, 241 f.
 Müller-Lyer, 243-5, 261
 Poggendorff, 241
 reversible perspectives, 268-71,
 304 f.
 theories, 244 f., 261
 vertical, 240
 Wundt, 242
 Zollner, 241
 Indifference point, time-sense, 578-
 82, 603
 Indirect vision, Aubert, 172
 early experiments, 171 f., 193
 zones, 172-6, 194
 Inner sense, 5
 Innervation, sensations, 527-9, 565 f.;
 ideomotor action, 529; reflex ac-
 tion, 529; will, 528
 Intensity, frequency theory, 67 f.
 Internal sensibility, 544-6, 570
 See also Visceral sensibility
 Intervals, musical, 313-8, 346; char-
 acteristics, 361 f.; fusion, 359-
 62, 393 f.; sensory magnitude,
 350
 Introspection, kinesthetic content,
 533 f., 567 f.
 Inversion of retinal image, 223-5,
 237 f., 259 f.
 Irradiation, 81
 Irritability, sense-organs, 71 f., 94
 muscle, 54
 Ishihara test, 187
 Isomorphism, anticipations, 84 f.
 apparent movement, 596, 599, 606
 conservation of energy, 86 f., 96
 criticism, 90, 96
 definition, 83 f.
 Fechner, 88
 G. E. Müller, 89
 Grassmann, 87 f.
 Hartley, 6
 Köhler, 90
 Lotze, 87
 Mach, 88
 parallelism, 85-90
 references, 95 f.
 tonal attributes, 377 f.
 visual depth, 303
 Isophonic contours, 377
 J.n.d., 36, 38, 43, 52
 magnitude, 350 f.
 Just temperament, 316-8, 346
 Kinesthesia, 524-35, 565-8
 conscience musculaire, 567
 conscious processes, 533 f., 567 f.
 Einstellung, 533, 567
 Golgi spindles, 531, 567
 ideomotor action, 529
 joint vs. muscle, 532
 lifted weights, 529 f., 532 f., 567
 muscle sense, 526 f., 565
 muscle spindles, 530 f., 566 f.
 Pacinian corpuscles, 530, 566
 perception of movement, 531 f.,
 567
 proprioception, 526 f., 534 f., 568
 rhythm, 586 f., 604

- Kinesthesia, *continued*
 sensations of innervation, 527-9, 565 f.
 terminology, 525, 566
 will, 528
 Koenig cylinders, 335
 Kundt dust method, 333, 335 f.
- Lens of eye, accommodation, 276-9, 306 f.
 function, 222 f.
 Leonardo's paradox, 283 f.
 Light, early theories, 98 f.
 refraction, 127 f.
 theories, 107 f.
 velocity, 99
 See also Color, Vision
 Limen, absolute, 36, 41 f.
 differential, 36, 42
 See also Psychophysics, Smell, Taste, Tone, Touch, Vision
 Limits of hearing, 332-9, 348 f.
 Listing's law, 230-2, 258
 Local signs, 29-32
 tactual, 234-6
 unconscious, 235 f.
 visual, 232-6
 Localization, visceral, 547, 549 f.
 visual, 236 f., 288
 See also Auditory localization, Touch
 Localization of brain function, Flourens, 74 f.
 Fritsch and Hitzig, 75
 Gall, 74 f.
 Lashley, 76 f.
 references, 94 f.
 sensory centers, 75-8
 speech center, 75
 See also Isomorphism
 Logical positivism, *see* Positivism
 Loudness, differential sensitivity, 342-6, 350
 tonal attribute, 377 f.
 Luminosity curve, 176-82, 194-6
- Masking, 365-7, 394 f.
 Meaning, imagery, 47 f.
 See also Context theory
 Meantone temperament, 318
 Measurement, *see* Psychophysics
 Mel, 351
- Memory color, 255 f.
 Mental measurement, *see* Psychophysics
 Method of average error, 41
 Method of constant stimuli, 38-41
 Method of just noticeable differences, 38
 Method of limits, 37 f.
 Method of right and wrong cases, 38-41
 Mind-body relation, conservation of energy, 86 f.
 interactionism, 85
 parallelism, 85-90
 See also Isomorphism
 Modalities, 10
 taste, 454 f.
 Monotic stimulation, 382
 Moon illusion, 267
 Movement, kinesthetic perception, 531 f., 567
 parallax and distance, 267 f., 304
 phenomenon, 250-2
 See also Apparent movement
 Muller's circle, 228-30, 257 f.
 Muscle sense, 526 f., 565
 nervous circle, 527 f., 565
 spindles, 530 f., 566 f.
 Muscle spindles, 530 f., 566 f.
 Music, consonance, 360
 Greek, 313-6
 pitch standards, 319-21
 scales, 313-8
- Nativism, Gestalt psychology, 33
 Hering, 29
 Joh. Müller, 29
 Külpe, 31 f.
 origins, 28
 phenomenology, 32 f.
 primitive spatiality, 234
 references, 49 f.
 Stumpf, 29 f.
 Titchener, 32
 visual space, 233-7, 258 f.
 Natural scale, 316-8, 346
 Nausea, 563 f.
 Need, *see* Drive
 Nerve conduction, Adrian, 59-61, 65, 68
 all-or-none law, 60-2, 66, 92, 214, 419, 423 f.
 animal electricity, 56-8, 91

- animal spirits, 53 f., 62, 91
- Bell-Magendie law, 63, 92 f.
- Bernstein, 58 f., 62
- Borelli, 54 f.
- current of injury, 57
- Descartes, 54
- du Bois-Reymond, 57 f.
- electricity, 55-62, 91 f.
- Erasistratos, 53
- Forbes, 65-8
- frequency theory, 67 f.
- Galen, 53 f.
- Galvani, 55-7
- Glisson, 54 f.
- Haller, 62
- Helmholtz, 58, 92
- instruments, 64
- irritability, 54, 71 f.
- kinds of impulses, 77, 95
- Lucas, 59-61
- membrane theory, 61 f., 92
- references, 91-3
- refractory period, 59-62, 66 f., 92
- sensory, 62-8
- sensory action potentials, 64-6
- sensory intensity, 67 f.
- succus nervus, 55
- Swammerdam, 55
- velocity, 58, 62
- vis nervosa, 55
- vis viva, 55
- Volta, 56 f.
- wave of negativity, 58 f.
- Wever-Bray effect, 65
- See also* Projection
- Nervous circle, 527 f., 565
- Noise, duplicity theory, 364 f.
 - generation, 363-5
 - thermal, 365, 394
 - tonality, 363-5, 394
 - vowels, 373
- Odorimetry, 443
- Ohm's law, 326-8, 347
- Olfactie, 441 f.
- Olfactometer, 441 f., 459
- Operationism, *see* Positivism
- Optics, early knowledge, 97 f., 100 f., 121 f.
 - eye, 99 f.
- Organic sensibility, classification, 523
 - See also* Deep sensibility, Hunger, Kinesthesia, Thirst, Vestibular sensibility, Visceral sensibility
- Oscillation, electrical, 332
- Pacinian corpuscles, 530, 566
- Pain, 465, 467-75, 513 f.
 - adaptation, 501, 520
 - end-organs, 470
 - intensive theory, 467, 469, 514
 - modality, 469, 494
 - stimulation, 493-5, 518
 - summation theory, 469, 499, 502 f., 519 f.
 - visceral, 546-51, 571
 - von Frey's theory, 494 f., 518
- Paradoxical cold, 469, 492 f., 504-7, 518
- Paradoxical warmth, 506 f., 520 f.
- Parallax, binocular, 282-5, 307 f.
- Parallax of movement, cue to distance, 267 f., 304
- Partials, 325
- Perception, association, 14-6
 - auditory localization, 331-92, 397 f.
 - auditory space, 381 f., 386 f.
 - Berkeley, 5 f.
 - context theory, 6, 17 f., 47 f.
 - Cornelius, 17
 - criteria, 264
 - Democritus, 4 f.
 - double representation, 135 f., 152
 - Ehrenfels, 16
 - Empedocles, 4
 - Epicurus, 5
 - field theory, 246-52, 299-303, 311, 599
 - figure and ground, 250 f., 253
 - form-quality, 16 f., 47
 - Gestalt psychology, 19
 - Heraclitus, 4
 - history of concept, 3 f., 13-9
 - horopter, 226-30, 257 f.
 - Hume, 6
 - illusions, 238-45, 260 f.
 - Jas. Mill, 15
 - J. S. Mill, 15, 18
 - Locke, 5
 - Meinong, 16 f.
 - mental chemistry, 9
 - modes of color, 135, 152
 - movement, 250-2, 588-602, 604-7
 - Protagoras, 4
 - reduction, 249 f., 311

Perception, *continued*

- references, 47 f.
- Reid, 13 f.
- retinal image, 222-5, 257
- rhythm, 583-7, 604
- Scottish school, 47
- sensation, 13 f.
- Stewart, 14
- tactual patterns for two points, 481 f., 516
- T. Brown, 14
- time, 575-7, 602 f.
- time-sense, 577-82, 603
- Titchener, 17 f.
- Tolman, 18
- touch blends, 509-12, 521 f.
- visceral patterns, 561-4, 573
- visual form, 238-56
- visual modes of appearance, 249 f.
- Wundt, 15 f.
- See also* Apparent movement, Auditory localization, Color, Constancy phenomenon, Empiricism, Hunger, Illusions, Kines-thesis, Nativism, Pain, Pitch, Pressure, Projection, Rhythm, Sensation, Size constancy, Smell, Specific nerve energies, Taste, Thermal sensibility, Thirst, Time, Time-sense, Tone, Touch, Two-point limen, Vestibular sensibility, Visceral sensibility, Vision, Visual space perception, Weher function
- Perspective, aerial, 266 f.
 - cue to distance, 264 f., 304
- See also* Reversible perspective
- Phenakistoscope, 589 f.
- Phenomenology, brightness, 135 f., 152
 - experimentalism, 117 f., 124 f.
 - Gestalt psychology, 33
 - Goethe, 113, 116, 124
 - nativism, 32 f.
 - perception, 19
 - positivism, 13
 - Purkinje, 116-8, 124 f.
 - size constancy, 288-99, 308-11
 - tonal attributes, 375 f.
 - tonal fusion, 361-3
 - touch, 512, 522
 - vision, 155-7, 190
 - visual form, 246-52

- Phrenology, 74, 94
- Physicalism, *see* Positivism
- Pitch, differential sensitivity, 339-42, 349 f.
 - frequency, 322-4, 346 f.
 - lower limit, 332-8, 348
 - musical scales, 313-8
 - standards, 318-21
 - stimuli, 319 f., 323 f., 328-32, 347 f.
 - stimulus intensity, 377 f., 396
 - tonal attribute, 377 f.
 - upper limit, 332-8, 348
- Plateau's spiral, 591 f.
- Positivism, empiricism, 33 f.
 - Gestalt psychology, 13
 - perception, 18 f.
 - sensation, 12 f., 46 f.
- ϕ -phenomenon, 595-8, 606
- Pressure, adaptation, 500 f., 519 f.
 - after-sensations, 502-4, 520
 - end-organs, 470
 - gradient, 487-90, 517 f.
 - macroscopic stimuli, 487 f.
 - microscopic stimuli, 487-90
 - spots discriminable, 479, 515 f.
 - stimulation, 487-90, 517 f.
 - summation, 499, 519
 - tension, 469 f., 489 f.
 - visceral, 546-51, 571
 - Weber function, 495-7, 518 f.
- Pressure sense, 465-75, 513 f.
- Projection, auditory, 82
 - Bernstein, 80 f.
 - quality, 78 f.
 - references, 95
 - spatial, 79-82
 - specific nerve energies, 78 f.
 - tactual, 80, 82
 - visual, 79 f., 82
- See also* Isomorphism
- Proprioception, 526 f., 534 f., 568
- Protopathic sensibility, 471-5, 515
- Pseudophone, 385
- Pseudoscope, 287
- Psychometric function, 39-41
- Psychophysical axioms, 88-90, 96, 141 f., 220
- Psychophysics, absolute limen, 36, 41 f.
 - average error, 41
 - constant stimuli, 38-41
 - differential limen, 36, 42

- equal category, 40, 51
- equivalents, 42
- Fechner, 34-7
- Fechner function, 36 f., 44 f.
- Herbart, 35
- j.n.d., 36, 38, 43, 52
- limits, 37 f.
- methods, 36, 51 f.
- origins, 34 f.
- psychometric function, 39-41
- quantity objection, 44 f., 52
- references, 50-2
- right and wrong cases, 38-41
- sense-distance, 37, 42 f.
- sense-ratio, 43 f., 52
- tonal scales, 350 f.
- Weber function, 34-6, 44 f.
- Pupil of eye, accommodation, 275
- Purkinje phenomenon, 177-82, 195 f.
 - dark adaptation, 181, 196
 - rods and cones, 181, 195 f.
 - visual purple, 181, 196
- Purkinje's images, accommodation, 277
- Pythagorean temperament, 315, 318
- Quantity objection, 44 f., 52
- Range of consciousness, 583 f., 604
- Rectification, electrical, 331 f.
- Reduction, 135, 311
- Reflexology, 534, 568
- Refractory period, 59-62, 66 f., 92
- Regression, constancy phenomenon, 256, 299, 311
- Resonance, 324 f., 347
- Resonance theory, *see* Auditory theory
- Retinal disparity, 284-8, 308
- Reversible perspective, fluctuation, 271
 - shading, 266, 269, 304
 - structure, 268-71, 304 f.
- Rhythm, early investigations, 583-5, 604
 - kinesthesia, 586 f., 604
 - objective conditions, 584-6, 604
 - range of consciousness, 583 f., 604
- Rotation, *see* Vestibular sensibility
- Sanson's images, accommodation, 277 f.
- Savart's spokes, 334, 348
- Savart's wheel, 334 f., 348
- Scales, musical, 313-8
 - pitch and loudness, 350 f.
- Scientific progress, dependence on instruments, 609
 - dependence on knowledge, 609 f.
 - facilitators, 613
 - individual attitudes, 612
 - individual inertia, 611 f.
 - inhibitors, 609-13
 - social attitudes, 612 f.
 - social inertia, 610 f.
 - thought, 608
- Secondary criteria of distance, 263-71, 303-5
- Semicircular canals, 536-42, 569 f.
 - Mach-Breuer-Brown theory, 537-43, 569
 - space perception, 386, 398, 541
 - See also* Vestibular sensibility
- Sensation, animal psychology, 11 f.
 - associationism, 3-10, 46
 - attributes, 3, 11, 19-27, 574
 - behaviorism, 11 f.
 - Bell-Magendie law, 8
 - Berkeley, 5 f.
 - cardiac, 564
 - classificatory criteria, 464
 - common attributes, 27
 - continuum, 10, 38
 - Democritus, 4 f.
 - discrimination, 11-3
 - Empedocles, 4
 - empiricism, 3-6, 45
 - Epicurus, 5
 - excretory, 564
 - French school, 7, 45 f.
 - gastric, 563 f.
 - Gestalt psychology, 4
 - Hartley, 6
 - Heraclitus, 4
 - history of concept, 3-13
 - Hume, 6
 - hunger, 551-8, 571 f.
 - interrelation of senses, 574 f., 602
 - Jas. Mill, 7 f.
 - Locke, 5
 - magnitude, 44 f., 52
 - nerve conduction, 62-8
 - neural intensity, 67 f.
 - number of elements, 10, 46
 - number of senses, 9 f.
 - phenomena, 4

Sensation, *continued*

- positivism, 12 f.
- projection, 78-83
- Protagoras, 4
- pulmonary, 564
- quanta, 38
- Scottish school, 3, 7, 13 f., 46
- sexual, 564
- specific nerve energies, 68-78, 94
- thirst, 558-61, 572 f.
- time-sense, 577-82, 603
- unity, 27, 49
- Watson, 11 f.
- Wundt, 3, 9
- See also* Color, Kinesthesia, Perception, Psychophysics, Smell, Specific nerve energies, Taste, Tone, Touch, Vestibular sensibility, Visceral sensibility, Vision, Weber function
- Sensationism, 5-10
 - See also* Sensation
- Sense-distance, 37, 42 f.
- Sense-ratio, 43 f., 52
- Sensory centers, 75-8, 94 f.
- Sensory circles, 476 f., 515
- Sexual sensation, 564
- Shading, cue to distance, 265 f., 304
- Shape constancy, 256
- Siren, 319 f., 328 f., 347
- Size, apparent, 289 f., 309
- Size constancy, alley problem, 290, 294-6, 310
 - animals, 296 f., 310
 - children, 297, 310
 - concept, 256, 303, 310 f.
 - determinants, 297 f.
 - early observations, 290-3
 - Emmert's law, 292 f., 310
 - law, 256
 - Martius, 293 f.
 - phenomenology, 298 f., 303
 - regression, 299
 - unconscious inference, 299, 303
- Smell, adaptation, 443, 459
 - benzene ring, 448 f.
 - chemistry, 446-8, 460
 - classification, 444-6, 459 f.
 - compensation, 443 f., 459
 - degenerate sense, 460
 - dichorhnic stimulation, 444
 - differential threshold, 442 f., 459
 - dirhnic stimulation, 444

- handbooks, 438-40, 458
- intensive threshold, 442, 459
- mixture, 443 f., 459
- molecular weight, 447
- olfactie, 441 f.
- olfactometer, 441 f., 459
- periodic series, 447 f.
- prism, 445 f., 460
- sense-organ, 440, 458
- specific nerve energies, 449
- stimulation, 441 f., 458 f.
- stimulus, 440 f., 458 f.
- Weber function, 442 f., 459
- Somesthesia, *see* Deep sensibility, Hunger, Kinesthesia, Thirst, Touch, Vestibular sensibility, Visceral sensibility
- Soné, 351
- Sound, conduction, 321 f., 347
 - Fourier analysis, 325-8, 347
 - harmonics, 324 f., 347
 - Ohm's law, 326-8, 347
 - pitch, 322-4, 346 f.
 - resonance, 324 f., 347
 - velocity, 322, 347
- Sound cage, 385 f.
- Specific nerve energies, adequate stimulation, 71, 94
 - Bell, 70
 - cerebral localization, 74-8
 - color, 111 f.
 - concept, 69
 - consequences, 73 f.
 - extension to qualities, 72-4, 94
 - Helmholtz, 73
 - Joh. Müller, 70-2, 94
 - Locke, 69 f.
 - projection, 78 f.
 - references, 94
 - sensory conduction, 63 f.
 - smell, 449
 - tactual, 467-71, 513 f.
 - taste, 454 f.
 - tones, 403 f., 408
 - Young, 70
- Spectra, after-images, 156, 160
- Spectrum, energy, 178 f., 195
- Speech center, 75
- Split movement, 597
- Static sense, 540, 569
 - See also* Vestibular sensibility
- Stereoscope, 285-7, 308
- Stereoscopy, 282-8, 301 f., 307 f.

- Stratton's experiment, 237
 Stroboscope, 589-91, 605
 Stumpf's paradox, 10, 46
 Summation, 469, 490-3, 498 f., 502 f., 518-20
 Summation tone, 354
 Superposition, cue to distance, 264
- Tactual localization, *see* Touch
 Tactual sensibility, *see* Touch
 Talbot-Plateau law, 145, 154
 Tartini's tones, 352 f., 392
 Taste, adaptation, 457, 461
 buds, 450 f., 460 f.
 chemistry, 455-7, 461
 classification, 453-5, 461
 handbooks, 449 f., 460
 ionization, 456
 modalities, 454 f.
 papillae, 451
 primary qualities, 453-5, 461
 reaction time, 458, 461 f.
 sense-organs, 450 f.
 sensitivity of tongue, 452, 461
 specific nerve energies, 454 f.
 tetrahedron, 455
 Weber function, 458, 462
- Telestereoscope, 287
 Temperament, musical, 316-8, 346
 Temperature sense, *see* Thermal sensibility
 Tendinous sensation, 532
 Tetrachords, 314 f.
 Thermal noise, 365, 394
 Thermal sensibility, 465-75, 513 f.
 adaptation, 498, 500, 519
 after-sensations, 503 f., 520
 cold receptors, 470 f., 514 f.
 heat, 504-9, 520 f.
 Hering's theory, 491 f., 518
 physiological zero, 491 f., 518
 stimulation, 490-3, 518
 summation, 498 f., 519
 theories, 490-3, 518
 visceral, 546-51, 571
 von Frey's diagram, 505
 warmth receptors, 470 f., 514 f.
 Weber function, 497 f., 519
 Weber's theory, 490-2, 518
- Thermionic valves, tonal stimuli, 330-2, 348
 Thirst, appetitive, 559-61, 563, 573
- dehydration, 558-61, 572 f.
 dryness of mouth, 558-61, 572 f.
 general sensation, 560 f., 572 f.
 salivation, 560 f., 572 f.
- Threshold, *see* Psychophysics, Smell, Taste, Tone, Touch, Vision
 Timbre, 357-9, 393
 fusion, 359 f.
- Time, conscious present, 576, 602
 given in experience, 576 f., 603
 perceived, 575 f., 602 f.
- Time-sense, apparatus, 581 f.
 errors of estimation, 579-81, 603
 indifference point, 578-82, 603
 long durations, 603 f.
 units of duration, 580-2
 Weber-function, 577-9, 603
- Tonality, tonal attributes, 380, 396 f.
- Tone, apparatus, 328-32, 347 f.
 attributes, 375-81, 396 f.
 audibility curve, 336-8, 348 f.
 audiogram, 337 f., 348 f.
 aural harmonics, 358 f., 393
 beats, 352-7, 392, 408
 brightness, 378, 396
 combination tones, 352-7, 392 f., 408
 consonance, 360
 density, 380, 397
 difference tones, 352-7, 392 f.
 distortion, 355-7
 Fourier analysis, 325-8, 347
 fusion, 359-62, 393 f.; diatonic intervals, 360; laws, 360 f.; Stumpf, 360-2
 harmonics, 324 f., 347
 highest pitch, 332-8, 348
 intensive threshold, 336-9, 348 f.
 intensity, differential sensitivity, 342-6, 350
 intervals, characteristics, 361 f.
 isophonic contours, 377
 localization, 384-91, 397 f.
 loudness, 377 f.; number j.n.d., 345, 350
 lowest pitch, 332-8, 348
 masking, 365-7, 394 f.
 modulation, 356 f.
 Ohm's law, 326-8, 347
 pitch, 322-4, 346 f., 377 f.; differential sensitivity, 339-42, 349 f.; number j.n.d., 341 f., 349 f.; scales, 313-8

Tone, *continued*

- pitch standards, 319-21
 - pure, 358
 - quanta, 345 f., 350
 - R. Koenig, 329-31, 347
 - scales of pitch and loudness, 350 f.
 - sensitivity, 336-9, 348 f.
 - specific nerve energies, 403 f., 408
 - summation tone, 354
 - timbre, 357-9, 393
 - tonality, 380, 396 f.
 - vocality, 367-75, 380, 396
 - volume, 377-9, 396
 - Weber function, 342-6, 350 f.
 - See also* Auditory theory
- Touch, adaptation, 500 f., 519 f.
- after-sensations, 501-4, 520
 - apparent movement, 599-601, 606 f.
 - blends, 509-12, 521 f.
 - clammy, 522, 511 f.
 - deep sensibility, 471-3, 523 f., 565
 - differentiation, 465-75, 513
 - end-organs, 469-71, 475, 514
 - epicritic sensibility, 471-5, 515
 - hard-soft, 511, 522
 - Head's theory, 471-5, 515
 - localization, anchorage points, 486, 517; sensory cues, 483 f.; visualization, 485 f., 517
 - nerve injury, 471-5, 515
 - pain, 465-75, 513 f.; stimulation, 493-5, 518; von Frey's theory, 494 f.
 - paradoxical cold, 469, 492 f., 504-7, 518
 - paradoxical warmth, 506 f., 520 f.
 - pressure, 465-75, 513 f.; gradient, 487-90, 517 f.; limen gauge, 487 f.; macroscopic stimuli, 487 f.; microscopic stimuli, 487-90; stimulus, 487-90, 517 f.; tension, 489 f.; Weber function, 485-7, 518 f.
 - protopathic sensibility, 471-5, 515
 - sensory circles, 476 f., 515
 - sensory spots, 467-71, 475, 513 f.; discriminable, 479, 515 f.
 - smooth-rough, 511, 522
 - sticky, 511, 522
 - summation, 469, 490-3, 498 f., 502 f., 518-20
 - thermal sensibility, 465-75, 513 f.;

- heat, 504-9, 520 f.; Weber function, 497 f., 519
 - thermal stimulation, 490-3, 518
 - thermal theories, 490-3, 518
 - tickle, 510 f., 521 f.
 - two-point limen, 475-85, 515 f.; criteria of 'two,' 482, 516; error of localization, 484 f., 517; fatigue, 481 f., 516; mobility, 477 f., 515; paradoxical judgments, 479 f., 516; perceptual patterns, 481 f., 516; practice, 480-2, 516
 - Vexirfehler, 479 f.
 - von Frey, 469-71, 475
 - Weber, 465 f., 513
 - wet, 509-11, 521
- Transposition, 254 f., 262
- Tuning fork, 319 f., 328-31, 347
- Two-point limen, 475-85, 515 f.
- criteria of 'two,' 482, 516
 - error of localization, 484 f., 517
 - fatigue, 481 f., 516
 - mobility, 477 f., 515
 - paradoxical judgments, 479 f., 516
 - perceptual patterns, 481 f., 516
 - practice, 480-2, 516
- Unity of senses, 27
- Valve, thermionic, 330-2, 348
- Vater-Pacini corpuscles, 530, 566
- Vertigo, 535 f., 542-4, 564, 568
- Vestibular sensibility, adaptation, 536, 539, 543 f., 570
- after-sensations, 536, 539, 543 f., 570
 - anesthetic, 543, 570
 - aviation, 543
 - central factors, 544, 570
 - deaf mutes, 542 f., 570
 - Flourens' experiments, 536-9
 - function of macula, 540 f.
 - Mach-Breuer-Brown theory, 537-43, 569
 - Mach's experiments, 537 f., 569
 - quality of sensation, 542, 544, 570
 - reflex sense, 542, 544, 570
 - rotation, 536-9, 569 f.
 - semicircular canals, 536-42, 569 f.
 - static sense, 540, 569
 - thermal stimulation, 538
 - vertigo, 535 f., 542-4, 564, 568

Vexirfehler, 479 f., 516
 Visceral sensibility, appetite, 551-3,
 557 f., 562 f., 572
 cardiac oppression, 564
 cold, 546-51, 571
 excretory needs and processes, 564
 gastric emptiness, 563
 gastric repletion, 563
 hunger, 551-8, 571 f.
 insensitivity, 546-8, 570 f.
 localization, 547, 549 f.
 nausea, 563 f.
 need vs. sensation, 561-3
 pain, 546-51, 571
 perceptual patterns, 561-4, 573
 pressure, 546-51, 571
 referred pains, 547
 sensitivity, 548-51, 570 f.
 sexual needs and processes, 564
 suffocation, 564
 thirst, 558-61, 572 f.
 warmth, 546-51, 571
 See also Hunger, Thirst
 Visibility curve, 176-82, 194-6
 Vision, accommodation, 109 f., 122 f.
 adaptation, 160-4
 after-images, 157-60, 190 f.; nega-
 tive, 158 f.; positive, 157 f.
 color blindness, 182-90
 contrast, 165-71, 192 f.
 dark adaptation, 181, 196
 Greeks, 97 f.
 Helmholtz, 119
 indirect, 171-6, 193 f.
 intensive j.n.d., 136-9
 lens, 99 f., 222 f., 276-9, 306 f.
 lens as receptor, 99
 light, 98-101, 121 f.
 luminosity, 176-82, 194-6
 Masson disk, 137 f.
 modern research, 120 f., 125
 nineteenth century, 116-21, 124 f.
 phenomenology, 113, 116-8, 124 f.,
 155-7, 190
 photopia, 182, 196
 Porterfield, 108, 122 f.
 Purkinje phenomenon, 118, 177-82,
 195 f.
 retinal image, 100, 222 f., 257; in-
 version, 223-5, 237 f.
 scotopia, 177-82, 196
 sensitivity, 176-82, 194-6; rods and
 cones, 179-81, 195

visual purple, 181, 196
 Weber function, 136-9, 152 f.
See also adaptation, Color, Color
 blindness, Color mixture, Con-
 trast, Purkinje phenomenon, Vis-
 ual space perception
 Visual space perception, accommo-
 dation, function, 109 f., 122 f.,
 272-4, 302 f.; mechanism,
 278 f., 306 f.; theories, 274-8,
 305 f.
 alley problem, 290, 294-6, 310
 apparent size, 289 f., 309
 Aubert-Forster law, 249, 309 f.
 binocular parallax, 282-5, 307 f.
 convergence, function, 271 f.,
 302 f., 305, 307
 corresponding points, 226-30,
 257 f.
 depth, dynamics, 299-303, 311; iso-
 morphism, 303; stereoscopic,
 282-8, 301 f., 307 f.
 distance, accommodation, 272-82,
 302 f., 305-7; aerial perspective,
 266 f.; convergence, 271 f., 279-
 82, 302 f., 305, 307; filled and
 empty, 267; light and shade,
 265 f., 269, 304; parallax of
 movement, 267 f., 304; perspec-
 tive, 264 f., 304; reversible per-
 spective 268-71, 304 f.; second-
 ary criteria, 263-71, 303-5; size,
 264 f., 288-99, 308-11; super-
 position, 264
 Donders' law, 230-2, 258
 double representation, 300 f.
 Emmert's law, 292 f., 310
 empiricism, 233-7, 258 f.
 eye-movements, 230-3, 258
 form, 238-56, 261 f.; Gestalt laws,
 253-5, 261 f.; Schumann, 247 f.;
 tridimensional, 299-303, 311
 form-quality, 246
 horopter, 226-30, 257 f.
 illusions, 238-45, 260 f.; reversible
 perspective, 268-71, 304 f.
 Listing's law, 230-2, 258
 local signs, 232-6, 288
 localization, 236 f.
 moon illusion, 267
 nativism, 233-7, 258 f.
 relativity, 237
 retinal disparity, 284-8, 308

Visual space perception, *continued*
 retinal image, 222-5, 257; inversion, 223-5, 237 f.
 reversible perspective, 268-71, 304 f.
 singleness of vision, 226-9, 257 f., size, 223 f.
 size constancy, 288-99, 308-11
 stereoscopy, 27-8, 307 f.; field theory, 301 f.
 vertical primacy, 233, 260
See also Illusions, Size constancy
 Vocality, tonal attribute, 380, 396
 Volley theory, 421-4, 435
 Volume, tonal attribute, 377-9, 396
 Von Frey hair, 488 f.
 Vowels, formants, 372 f.
 methods, 369, 371-5, 395 f.
 noise, 373

octave relations, 372, 374
 pitches, 370, 380
 theories, 368, 371-5, 395 f.

Warmth, *see* Thermal sensibility
 Water-fall illusion, 592 f.
 Weber function, loudness, 343 350 f.
 pressure, 495-7, 518 f.
 shape, 139, 153
 smell, 442 f., 459
 stellar magnitudes, 153
 taste, 458, 462
 temperature, 497 f., 519
 time-sense, 577-9, 603
 vision, 136-9, 152 f.
See also Psychophysics
 Wever-Bray effect, 420-3, 435